

A Broken System, Part II:

**Why There Is So Much Error in
Capital Cases, and
What Can Be Done About It**

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Executive Summary

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There is growing awareness that serious, reversible error permeates America's death penalty system, putting innocent lives at risk, heightening the suffering of victims, leaving killers at large, wasting tax dollars, and failing citizens, the courts and the justice system.

Our June 2000 Report shows how often mistakes occur and how serious it is: 68% of all death verdicts imposed and fully reviewed during the 1973-1995 study period were reversed by courts due to serious errors.

Analyses presented for the first time here reveal that 76% of the reversals at the two appeal stages where data are available for study were because defense lawyers had been egregiously incompetent, police and prosecutors had suppressed exculpatory evidence or committed other professional misconduct, jurors had been misinformed about the law, or judges and jurors had been biased. Half of those reversals tainted the verdict finding the defendant guilty of a capital crime as well as the verdict imposing the death penalty. 82% of the cases sent back for retrial at the second appeal phase ended in sentences less than death, including 9% that ended in *not guilty verdicts*.

Part II of our study addresses two critical questions: Why does our death penalty system make so many mistakes? How can these mistakes be prevented, if at all? Our findings are based on the most comprehensive set of data ever assembled on factors related to capital error—or other trial error.

Our main finding indicates that if we are going to have the death penalty, it should be reserved for the worst of the worst: **Heavy and indiscriminate use of the death penalty creates a high risk that mistakes will occur.** The more often officials use the death penalty, the wider the range of crimes to which it is applied, and the more it is imposed for offenses that are not highly aggravated, the greater the risk that capital convictions and sentences will be seriously flawed.

Most disturbing of all, we find that **the conditions evidently pressuring counties and states to overuse the death penalty and thus increase the risk of unreliability and error include *race, politics* and *poorly performing law enforcement systems*.** Error also is linked to overburdened and underfunded state courts.

MAIN FINDING

The higher the rate at which a state or county imposes death verdicts, the greater the probability that each death verdict will have to be reversed because of serious error.

- The overproduction of death penalty verdicts has a powerful effect in increasing the risk of error. Our best analysis predicts that:
 - Capital error rates more than *triple* when the death-sentencing rate increases from a quarter of the national average to the national average, holding other factors constant.
 - When death sentencing increases from a quarter of the national average to the highest rate for a state in our study, the predicted increase in reversal rates is *six-fold*—to *about 80%*.

In particular, the more often states impose death sentences in cases that are not highly aggravated, the higher the risk of serious error.

- At the federal habeas stage, the probability of reversal grows substantially as the crimes resulting in capital verdicts are less aggravated. For each additional aggravating factor, the probability of reversal drops by about 15%, when other conditions are held constant at their averages. Imposing the death penalty in cases that are not the worst of the worst is a recipe for unreliability and error.

Comparisons of particular counties' and states' capital-sentencing and capital-error rates illustrate the strong relationship between frequent death sentencing and error. For example:

- Among counties with 600 or more homicides and five or more death sentences during the study period, ten had the highest death-sentencing rates: Pima County (Tucson), Arizona; suburban Baltimore County, Maryland; Clark County (Las Vegas), Nevada; Pinellas County (St. Petersburg), Florida; Oklahoma (City), Oklahoma; Maricopa County (Phoenix), Arizona; Hamilton County (Cincinnati), Ohio; Hillsborough County (Tampa), Florida; Polk County, Florida; and Muscogee County, Georgia. These counties had an average capital error rate of 71% at the first and last appeal stages, and eight of them put a total of 16 people on death row who were later found not guilty. The ten comparable capital counties with the lowest death-sentencing rates are San Francisco, California; Richmond, Virginia; Fulton County (Atlanta), Georgia; Essex County (Newark), New Jersey; St. Louis City, Missouri; Pulaski County (Little Rock), Arkansas; Bernalillo County (Albuquerque), New Mexico; Davidson County (Nashville), Tennessee; Jackson County (Kansas City), Missouri; and Prince George's County (suburban Washington), Maryland. These counties had an average error rate of 41%, and none sentenced anyone to death during the study period or since who was later found not guilty.*

* Table 16, Page 304.

- All but one of the 10 states with the highest death-sentencing rates during the 23-year study period had overall capital reversal rates at or above the average rate of 68%.

PRESSURES ASSOCIATED WITH OVERUSE OF THE DEATH PENALTY

Four disturbing conditions are strongly associated with high rates of serious capital error. Their common capacity to pressure officials to use the death penalty aggressively in response to fears about crime and regardless of how weak any particular case for a death verdict is, may explain their relationship to high capital error rates.

- **The closer the homicide risk to whites in a state comes to equaling or surpassing the risk to blacks, the higher the error rate.** Other things equal, reversal rates are *twice as high* where homicides are most heavily concentrated on whites compared to blacks, than where they are the most heavily concentrated on blacks.
- **The higher the proportion of African-Americans in a state—and in one analysis, the more welfare recipients in a state—the higher the rate of serious capital error.** Because this effect has to do with traits of the population at large, not those of particular trial participants, it appears to be an indicator of crime fears driven by racial and economic conditions.
- **The lower the rate at which states apprehend, convict and imprison serious criminals, the higher their capital error rates.** Predicted capital error rates for states with only 1 prisoner per 100 FBI Index Crimes are about 75%, holding other factors constant. Error rates drop to 36% for states with 4 prisoners per 100 crimes, and to 13% for those with the highest rate of prisoners to crimes. Evidently, officials who do a poor job fighting crime also conduct poor capital investigations and trials. Well-founded doubts about a state's ability to catch criminals may lead officials to extend the death penalty to a wider array of weaker cases—at huge cost in error and delay.
- **The more often and directly state trial judges are subject to popular election, and the more partisan those elections are, the higher the state's rate of serious capital error.**

ADDITIONAL FINDINGS

Heavy use of the death penalty causes delay, increases cost, and keeps the system from doing its job. High numbers of death verdicts waiting to be reviewed paralyze appeals. Holding other factors constant, the process of moving capital verdicts from trial to a final result seems to come to a halt in states with more than 20 verdicts under review at one time.

Poor quality trial proceedings increase the risk of serious, reversible error. Poorly funded courts, high capital and non-capital caseloads, and unreliable procedures for finding the facts all increase the chance that serious error will be found. In contrast, high quality, well-funded private lawyers from out of state significantly increase a defendant's chance of showing a federal court that his death verdict is seriously flawed and has to be retried.

Chronic capital error rates have persisted over time. Overall reversal rates were high and fairly steady throughout the second half of the 23-year study period, averaging 60%. When all significant factors are considered, state high courts on direct appeal—where 79% of the 2349 reversals occurred—found significantly more reversible error in recent death verdicts than in verdicts imposed earlier in the study period. Other things equal, direct appeal reversal rates were increasing 9% a year during the study period.

State and federal appeals judges cannot be relied upon to catch all serious trial errors in capital cases. Like trial judges, appeals judges are susceptible to political pressure and make mistakes. And the rules appeals judges use to decide whether errors are serious enough to require death verdicts to be reversed are so strict that egregious errors slip through. We study four illustrative cases in which *the courts approved the convictions and death sentences of innocent men despite a full set of appeals.** These case studies show that judges repeatedly recognized that the proceedings were marred by error but affirmed anyway because of stringent rules limiting reversals.

SUMMARY EXPLANATION

The lower the rate at which a state imposes death sentences—and the more it confines those verdicts to the worst of the worst—the less likely it is that serious error will be found. The fewer death verdicts a state imposes, the less overburdened its capital appeal system is, and the more likely it is to carry out the verdicts it imposes. The more often states succumb to pressures to inflict capital sentences in marginal cases, the higher is the risk of error and delay, the lower is the chance verdicts will be carried out, and the greater is the temptation to approve flawed verdicts on appeal. Among the disturbing sources of pressure to overuse the death penalty are political pressures on elected judges, well-founded doubts about the state's ability to convict serious criminals, and the race of the state's residents and homicide victims.

* We study the cases of Lloyd Schlup, Earl Washington, Anthony Porter and Frank Lee Smith. See pp. 25-36.

METHODS

We employ an array of statistical methods to identify factors that predict where and when death verdicts are more likely to be found to be seriously flawed, and to assure that the analyses are comprehensive, conservative and reliable: We use several statistical methods with different assumptions about the arrangement of capital reversals and reversal rates to ensure that results are driven by relationships in the data, not statistical methods. We analyze reversals at each separate review stage and at all three stages combined. We use multiple regression to analyze the simultaneous effect on reversal rates of important general factors (state, county, year and time trend) and specific conditions that may explain error rates. We examine factors operating at the state, county and case level. And we check for consistency of results across analyses to determine which factors and sets of significant factors are the most robust and warrant the most confidence.

POLICY OPTIONS

The harms resulting from chronic capital error are costly. Many of its evident causes are not easily addressed head-on (*e.g.*, the complex interaction of a state’s racial make-up, its welfare burden and the efficacy of its law enforcement policies). And indirect remedies are unreliable because they demand self-restraint by officials who in the past have succumbed to pressures to extend the death penalty to cases that are not highly aggravated. As a result, some states and counties may conclude that the only answer to chronic capital error is to stop using the death penalty, or to limit it to the very small number of prospective offenses where there is something approaching a social consensus that only the death penalty will do.

In other states and counties, a set of carefully targeted reforms based upon careful study of local conditions might seek to achieve the central goal of limiting the death penalty to “the worst of the worst”—to defendants who can be shown without doubt to have committed an egregiously aggravated murder without extenuating factors. Ten reforms that might help accomplish this goal are:

- Requiring proof beyond *any* doubt that the defendant committed the capital crime.
- Requiring that aggravating factors substantially outweigh mitigating ones before a death sentence may be imposed.
- Barring the death penalty for defendants with inherently extenuating conditions—mentally retarded persons, juveniles, severely mentally ill defendants.
- Making life imprisonment without parole an alternative to the death penalty and clearly informing juries of the option.
- Abolishing judge overrides of jury verdicts imposing life sentences.

- Using comparative review of murder sentences to identify what counts as “the worst of the worst” in the state, and overturning outlying death verdicts.
- Basing charging decisions in potentially capital cases on full and informed deliberations.
- Making all police and prosecution evidence bearing on guilt vs. innocence, and on aggravation vs. mitigation available to the jury at trial.
- Insulating capital-sentencing and appellate judges from political pressure.
- Identifying, appointing and compensating capital defense counsel in ways that attract an adequate number of well-qualified lawyers to do the work.

CONCLUSION

Over decades and across dozens of states, large numbers and proportions of capital verdicts have been reversed because of serious error. The capital system is collapsing under the weight of that error, and the risk of executing the innocent is high. Now that explanations for the problem have been identified and a range of options for responding to it are available, the time is ripe to fix the death penalty, or if it can't be fixed, to end it.

Guide to the Report

This Report provides detailed descriptions of: the current death penalty debate (Part IA); the amount and seriousness of the capital error found by courts during the 1973-1995 study period (Parts IB-II); our research methods (Part III); the results of 19 statistical analyses of our data on capital reversal rates (Parts IV-VI); our interpretation of those results (Part VII); and available policy options (Part VIII).

Readers interested in **a comprehensive summary of our main findings and conclusions** may wish to read the following portions of the Report:

- Executive Summary (pp. i-vi).
- Table of Contents, which states the conclusion of each section of the Report (pp. ix-xxvii).
- Summary and Interpretation of Regression Results (pp. 337-90).
- Conclusion (pp. 422-28)

Readers interested in **descriptions of particular cases in which serious capital error was found** may wish to read:

- Our study of four illustrative cases in which courts approved the convictions and death sentences of innocent men despite a full set of appeals (pp. 25-35).
- A description of the errors found in each of 352 state post-conviction decisions reversing a capital verdict between 1973 and April 2000 (Appendix C).
- A description of 46 illustrative federal habeas corpus reversals during the 1973-1995 study period (Appendix D).

Readers interested in **comparisons of the 34 capital-sentencing states** studied here may wish to examine:

- Figure 1B, p. 51, which shows the states' overall reversal rates during the 1973-1995 study period.
- Figure 11, p. 121, which shows the states' death-sentencing rates during that period.
- Table 18, pp. 344-45, which ranks states based on how much risk of serious capital error they faced based on our best analyses of each of eight conditions found to be related to error, holding other conditions constant at their averages.

- State Report Cards (Appendix A) documenting each state's use of the death penalty and capital-error rates during the study period.

Readers interested in **comparisons of the error and death-sentencing rates of relatively populous counties and cities in the U.S.** that imposed 5 or more death sentences during the study period may wish to examine:

- Tables 13 and 14, pp. 294-95, 297-99, comparing high death-sentencing and low-death sentencing counties and cities based on their capital-error rates and how frequently they sentenced persons to die who later were shown to be not guilty.
- Table 16, pp. 304-05, comparing the 73 largest capital counties and cities based on their death-sentencing and capital-error rates.
- Table 19, p. 346, comparing the 6 counties in the nation with over 100 death sentences and the 9 counties with between 50 and 100 death sentences based on their death-sentencing rates.
- Table 20, p. 348, giving examples of high and low death-sentencing counties in the same state.

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A Broken System, Part II:
**Why There is So Much Error in Capital Cases,
and What Can be Done About It**

I. Introduction: Rising Doubts About the Administration of the Death Penalty

The nation is in the midst of its most serious and sustained public discussion of the death penalty in decades.¹ The discussion has many parts, but one theme is constant: **People who have not committed a crime for which the law permits the death penalty should not be sentenced to die.**² Yet, as Supreme Court Justice Sandra Day O'Connor said recently, **"If statistics are any indication, the system may well be allowing some innocent defendants to be executed."**³

A. The Recent Wave of Rethinking and Reform

Over the last two years, these concerns have led to changes in the public's thinking about the death penalty and a variety of policy initiatives:

- The Governor of Illinois declared a moratorium on executions in that state, and appointed a blue ribbon committee to address the problem.⁴ In the meantime, the Illinois Supreme Court issued comprehensive rules to improve the quality of capital trials in the state,⁵ and the Illinois House of Representatives approved an even more demanding package of reforms.⁶
- The Nebraska Legislature passed a moratorium on executions so that a comprehensive study of the state's death penalty could occur. The moratorium was vetoed, but the study went forward, finding economic and geographic disparities and proposing reforms.⁷
- In the Winter-Spring 2001 legislative cycle, 37 of 38 death penalty states (all but Kansas) considered legislation to reform and moderate the use of the death penalty; at least one such reform became law in 21 of those states.⁸
- Maryland's Governor also ordered a comprehensive study of that state's death penalty.⁹ In March 2001, that state's House of Delegates passed a bill imposing a moratorium on executions until the study is completed. The bill reportedly had majority support in the state Senate but was blocked by an end-of-session filibuster.¹⁰ Days later, the state's high court imposed its own moratorium, granting review on an issue affecting all Maryland cases.¹¹
- In Nevada, the state Senate passed legislation by a wide margin imposing a moratorium on executions. The Assembly instead proposed and the Legislature adopted a bill funding a broad

study of the state's death penalty.¹² The Legislature also directed the study commission to consider broadened availability of DNA tests and bans on executing mentally retarded individuals (such a ban passed the State Assembly¹³) and juvenile offenders.¹⁴

- In Texas:
 1. The Legislature passed and the Governor signed emergency legislation giving death row inmates greater access to DNA evidence that might exonerate them.¹⁵
 2. The Legislature adopted and the Governor signed legislation for the first time ensuring timely and non-patronage appointment of, and minimum standards for, defense lawyers, affording extra assistance to lawyers handling capital cases and providing nearly \$20 million (compared to zero previously) to help pay for counsel.¹⁶
 3. The state also raised the amount it provides innocent people found to have been erroneously convicted from a maximum of \$50,000 to \$250,000.
 4. Both houses adopted, but the Governor vetoed, legislation banning execution of mentally retarded persons.¹⁷
 5. The Governor endorsed and the Senate and a House committee adopted, but the full House defeated, a bill making life without parole the alternative to a death sentence.¹⁸
 6. The House passed a bill banning executions for crimes committed by juveniles.
 7. The Senate Criminal Justice Committee unanimously approved a bill barring consideration of the defendant's race in deciding whether to sentence him to die and requiring hearings on the issue in appropriate cases.
 8. Committees in both houses endorsed a resolution letting voters decide whether to impose a 2-year moratorium on executions while the state's death penalty is studied.
 9. The number of people executed dropped from 40 in 2000 to 17 in 2001.¹⁹
- North Carolina adopted a new law allowing prosecutors to seek life without parole, not the death penalty, for aggravated first-degree murder. The absence of this power had previously inflated the number of death sentences in marginal cases.²⁰
- In Virginia, which has long put strict limits on review of capital verdicts, the Legislature and Governor agreed on "a major overhaul of the state's death-penalty laws, giving those on death row the right to seek new DNA testing."²¹
- "Major campaigns to suspend executions have been launched in 19 [eventually 20] states" in 2001,²² including moratorium and death-penalty study bills in Alabama (passed by the Senate

Judiciary Committee), Arkansas, Delaware, Illinois, Indiana, Kentucky,²³ Louisiana, Maryland (discussed above²⁴), Missouri, Mississippi, Nevada (discussed above²⁵), New Jersey, North Carolina, Oklahoma, Pennsylvania, Tennessee, Texas (discussed above²⁶), Virginia and Washington—up from 2 in 1999 and 2000 combined, and virtually none during the rest of the 1990s. Abolition bills nearly passed in New Hampshire and New Mexico this Spring, and were proposed in Illinois, Indiana, Kentucky, Louisiana, Missouri, Mississippi, Montana (hearings held in House Judiciary Committee), Nebraska, New Jersey, New York, Oregon, Pennsylvania, Tennessee and Virginia.²⁷

- Bills to reinstate the death penalty failed in Maine and Massachusetts by large margins.²⁸
- Major death penalty studies have occurred or are occurring in Arizona,²⁹ Connecticut, Illinois, Indiana, Maryland, Nebraska, Nevada, New Jersey, North Carolina, Virginia, and the Federal system.³⁰
- At least 26 states across the country, including Arizona, Arkansas, California, Delaware, Florida, Idaho, Indiana, Louisiana, Maryland, Missouri, Nebraska,³¹ New Mexico, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, Tennessee, Texas, Utah, Virginia and Washington, have recently adopted reforms making it possible for capital prisoners to obtain post-conviction DNA testing.³² Over half the statutes were passed in 2001. Similar legislation was considered in Alabama, Kentucky, Nevada, New Jersey, Mississippi, North Carolina, Ohio, South Dakota (adopted by legislature but vetoed by the Governor) and Tennessee.³³
- Six states—Arizona, Connecticut, Florida, Missouri, North Carolina and Tennessee—barred the death penalty for mentally retarded persons in 2001, joining 13 states that had done so in the previous two decades.³⁴ Not least among the motivations for such action is that retarded people are especially vulnerable to being convicted and condemned when innocent—as happened to three men exonerated in 1999-2001: Anthony Porter, Earl Washington and Albert Burrell.³⁵ Similar concerns may have led the United States Supreme Court to agree to reconsider a 12-year-old ruling allowing states to execute mentally retarded defendants.³⁶
- Bans on executing juveniles were considered this year in Arkansas (passed by the Senate), Florida (passed by the House), Indiana, Kentucky (endorsed by the Governor) and South Carolina, in addition to Nevada and Texas, discussed above.³⁷
- Plans for improving capital representation were adopted in Arkansas, Mississippi, Oklahoma and Virginia,³⁸ in addition to Texas (discussed above³⁹). Proposals for similar improvements are under consideration in Alabama, Georgia, North Carolina and Washington.⁴⁰
- Bipartisan legislation to assure capital prisoners' access to DNA testing, to improve the quality of representation at capital trials and to increase penalties for prosecutorial suppression of evidence and other causes of unreliable capital verdicts has been proposed in Congress. Its Senate co-sponsors include Gordon Smith (R-Or.), Susan Collins (R-Me.) and John Warner (R-Va.), all death penalty supporters. In the House it has 214 co-sponsors, 44

of them Republican, including respected conservatives John Boehner (OH), Jennifer Dunn (WA), Lindsey Graham (SC), Ray LaHood (IL), George Nethercutt (WA), Rob Portman (OH) and Joe Scarborough (FL).⁴¹

- Prominent death penalty supporters have recently expressed doubts about the reliability of, and in some cases the continuing need for, the nation’s death penalty, including: Illinois Republican Governor George Ryan,⁴² Connecticut Republican Governor John Rowland (citing concerns about unfair use of the death penalty against racial minorities),⁴³ New Mexico Republican Governor Gary Johnson⁴⁴ and Minnesota Reform Party Governor Jesse Ventura;⁴⁵ conservative commentators Oliver North,⁴⁶ George Will (*Washington Post*),⁴⁷ Bruce Fein⁴⁸ and Paul Craig Roberts⁴⁹ (both in the *Washington Times*), Rod Dreher (*New York Post*),⁵⁰ Gerald Seib (*Wall St. Journal*)⁵¹ and Emmett Tyrell, Jr. (*American Spectator*);⁵² the CIA’s former station chief in Pakistan (expressing concerns about the effect of the death penalty on America’s ability to extradite and try terrorists);⁵³ Timothy McVeigh prosecutors James Orenstein (citing concerns about race discrimination)⁵⁴ and Beth Wilkinson;⁵⁵ conservative religious leaders Rev. Pat Robertson,⁵⁶ John Diulio (director of President Bush’s faith-based public service initiative),⁵⁷ The Christian Life Commission of the General Board of Texas Baptists,⁵⁸ the North Carolina Conference of The United Methodist Church,⁵⁹ Rutherford Institute founder and president John W. Whitehead⁶⁰ and the editors of the influential evangelical Christian magazine *Christianity Today*;⁶¹ the Republican author of Ohio’s death penalty law (now a Justice on its Supreme Court),⁶² the author of Texas’s death penalty statute (now a senior state district judge),⁶³ the just-retired Chief Justice of the Washington Supreme Court and his successor⁶⁴ and other conservative officials in a variety of states who have supported proposals for death penalty moratoria or other reforms.⁶⁵
- Oklahoma Republican Governor Frank Keating, speaking to the National Press Club, and John Podhoretz—both staunch death penalty supporters—recently called for laws limiting death verdicts to cases where guilt is proved to an “actual certainty” and not just “beyond a reasonable doubt.”⁶⁶
- A leading legal academic defender of the death penalty recently wrote that “[w]e are witnessing today a true crisis of confidence in the death penalty in the United States. For the first time in more than twenty-five years, public support for the death penalty seems to be waning. The evidence of trouble is everywhere.”⁶⁷
- Supreme Court Justice Ruth Bader Ginsburg recently took the unprecedented step of endorsing a death penalty moratorium, noting that she has “yet to see a death penalty case among the dozens coming to the Supreme Court on eve-of-execution stay applications in which the defendant was well-represented at trial.”⁶⁸
- Not long afterwards, Justice Sandra Day O’Connor, a long-time death penalty supporter who helped write Arizona’s death penalty statute before moving to the nation’s high court, gave speeches in two states raising “**serious questions’ about whether the death penalty is being fairly administered in the United States**” and whether “**the system may . . . be**

allowing some innocent defendants to be executed.” Justice O’Connor said the “quality of defense lawyers for people in capital cases has been inadequate in too many cases.”⁶⁹

- Since 1994, support for the death penalty in national polls has dropped from about 80% to a 20-year low of 63%.⁷⁰ When the choice posed is between life without parole and the death penalty (life without parole is in fact the alternative to death sentences in all capital states except Texas⁷¹) support for the penalty drops to 50% or below.⁷² Half of all Americans support a suspension of executions while the death penalty is studied.⁷³
- Changes in public opinion may be having an important effect on public action. Jurors and judges are imposing fewer death sentences, which dropped 29% between 1998 and 2000 (the last year when data are available).⁷⁴ The number of executions declined 32% between 1999 and 2001.⁷⁵ Governors are commuting more death sentences.⁷⁶

B. Error and the Risk of Executing the Innocent as the Spur to Reform

Catalyzing national concern about the reliability of capital convictions and sentences is the fact that, **in the same year the nation carried out its 700th execution in the modern death-sentencing era, it also approached its 100th exoneration of a person sentenced to die despite being factually or legally innocent.**⁷⁷

The capital system is supposed to single out offenders who committed crimes for which the law allows the death penalty, and to carry out the penalty with the swiftness and sureness needed to provide credible retribution and deterrence. The statistics to which Justice O’Connor and others refer—99 exonerations nationally compared to about 750 executions⁷⁸—are a good test of the quality of the outcomes the capital system produces. One alarming failure for every 7 or 8 routine successes is a troubling bottom line. This would be true for any process. It is especially true for one run by the Government that says who lives and who dies and takes years, even decades, to fix its mistakes.

As Justice O’Connor further notes, there is an even more disturbing lesson from the ratio of exonerations to executions: **When so many failures are known to occur, and when mistakes are so hard to catch, it is likely that some are *not* caught. There is reason to fear that some**

executions counted as successes are actually undiscovered *failures*—executions of defendants who were innocent, or did not commit a crime for which the death penalty is allowed, but whom the courts inadvertently allowed to be executed.⁷⁹

Regrettably, researchers cannot study or accurately count the number of innocent people executed in the United States since 1973, because available information often cannot show whether executed people were innocent. The truth may die with them or with the victims they were convicted of killing. The death penalty is unique in this respect. Unlike other situations in which innocent people are accidentally killed—an airplane crash, an e-coli outbreak or an explosion at a chemical plant—the total number of innocent fatalities caused by the death penalty is unknowable, even when all evidence is examined.

Nor can researchers examine all the evidence. Along with the press, policy makers and the public, researchers are often denied access to the single existing repository of evidence, such as DNA samples, that can best establish whether executed people were innocent: The police file in the case of executed persons. Notwithstanding requests from the media, churches, charities and the relatives of executed individuals, and even in cases involving weak and compromised evidence of guilt and repeated protestations of innocence, officials routinely deny access to police files on executed individuals,⁸⁰ or even destroy the files.⁸¹ The reason officials give for withholding and destroying files increases concern about what the files would show—that if the evidence were subjected to DNA and other tests, and if the tests went badly for the state, “‘it would be shouted from the rooftops that the Commonwealth of Virginia [or another state] executed an innocent man.’”⁸²

Nor do American capital jurisdictions conduct inquests like those used in Britain, Canada and Australia to review questionable criminal convictions and sentences after judicial review has ended

and sentences have been carried out.⁸³ The one state that is currently conducting such an inquest due to concerns about faulty testimony by state forensic experts has decided that questionable “cases in which inmates have already been executed will be considered last, if at all.”⁸⁴ These obstacles to research and informed policy making are especially disturbing where people may have been executed though they were innocent of a crime. They are also troubling where people may have been sentenced to death for crimes they did commit, but for which death is not a legal penalty.

The death penalty thus is unique among government actions (or private ones, for that matter) that put innocent life at risk. Only in this context are researchers and policy makers unable to learn how many innocent casualties an activity causes—including because officials decline to disclose evidence on the question.⁸⁵ For the same reason, the death penalty is uniquely an area where researchers and policy makers must rely entirely on evidence of a *risk* that the state is taking innocent lives, *i.e.*, on the amount and rates of near misses and other serious errors found before the worst harms occurred.

Consideration of risk is of course appropriate in regard to all potentially harmful activities, even where investigators *can* identify innocent fatalities once they occur, so harms can be prevented. Delta Airlines, Hormel Meats or Goodyear Tires would risk severe heavy fines and loss of business if they stopped conducting safety inspections or ignored the results until it was shown that their products and services had injured or killed people. **If risk management based on flaws observed in quality control inspections is a crucial way to assess the safety and effectiveness of enterprises in which actual injuries and fatalities *can* be detected,⁸⁶ it is indispensable where unavoidable circumstances and the withholding of information in government files keep the public from even knowing whether innocent fatalities have occurred.**

C. The Disturbing Amount of Error, Unreliability and Risk Revealed by our June 2000 Report, *A Broken System*

Our June 2000 report, *A Broken System: Error Rates in Capital Cases, 1973-1995* (“*A Broken System*”), documented the rates of serious, judicially reversible error that the American capital system’s quality control inspectors discovered between 1973 and 1995. Based on results from all three levels of judicial review of death verdicts—direct appeals to state high courts, state post-conviction review by state trial and high courts and federal habeas corpus review—*A Broken System* made the following findings, among others in the National Report Card, p. 9 below:

- State high courts found that **41%** of the over 4500 capital verdicts finally reviewed on direct appeal in the 23-year study period were too flawed to carry out and had to be reversed.⁸⁷
- Of the 59% of capital verdicts that survived direct review, at least **10%** were found to be flawed and were reversed by state post-conviction courts.⁸⁸
- Of the remaining 54% of death verdicts, **40%** were reversed by federal habeas judges because of serious error.
- Overall, only 32% of the capital verdicts imposed and finally reviewed between 1973 and 1995 were approved for execution.* Conversely, **68% of those verdicts were so seriously flawed that they had to be reversed and sent back for a new trial of guilt or sentence.**

* The proportion surviving direct review was 59%: $100\% - 41\%$ reversed on direct appeal = 59%. The proportion surviving direct appeal and state post-conviction was 53%: 59% surviving direct review - 10% of those 59% that were reversed on state post-conviction = $59\% - 6\% = 53\%$. The proportion surviving all three stages was 32%: 53% surviving the first two stages - 40% of those 53% that were reversed on federal habeas corpus = $53\% - (.40 \times 53\%) = 53\% - 21\% = 32\%$. (Note: The numbers in the National Report Card below are calculated without rounding at each stage of the calculation, but only at the end. For ease of exposition, the example in this footnote rounds at each stage of the calculation.)

National Composite Capital Punishment Report Card

History

First Death Sentence	1973
First Direct Appeal	1973
First Consensual Execution	1977
First Non-Consensual Execution	1979
Years From First Death Sentence to First Non-Consensual Execution	6
Average Number of Years from Sentence to Execution	9
Average Number of Years from Sentence to Final Federal Relief	9

Sentences and Executions

Total Number of Death Sentences	5,826
Total Number of Executions	313
Percentage of Death Sentences Carried Out	5%

Error Rates

State Direct Appeal (34 States)

Number Reviewed on Direct Appeal	4,546
Number Reversed on Direct Appeal	1,852
Percentage Reversed on Direct Appeal	41%
Number Awaiting Direct Appeal	1,280
Percentage Awaiting Direct Appeal	22%
Number Forward to State Post-Conviction	2,694

State Post-Conviction (28 States)

Number Reviewed on Post-Conviction	Unknown
Number Reversed on Post-Conviction	≥257
Percentage Reversed on Post-Conviction	≥10%
Number Forward to Federal Habeas Corpus	Unknown

State Direct Appeal and State Post-Conviction Combined (28 States)

Overall Rate of Error Found by State Courts	46%
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Federal Habeas Corpus (28 States)

Number Reviewed on Habeas	598
Number Reversed on Habeas	240
Percentage Reversed on Habeas	40%

Overall Rates Including [and Excluding] State Post-Conviction (28 States)

Overall Error Rate	68% [65%]
Overall Success Rate	32% [35%]

Sources: DRCen; Death Row U.S.A., Fall 2001; DADB; HCDB; Appendix C

For reasons discussed above,⁸⁹ *A Broken System* could not and did not study the number of people executed for crimes they did not commit or for crimes for which the law does not allow the death penalty. What it instead studies is the number and rates of seriously flawed, and thus unreliable, death verdicts, and the resulting *risk* that people have been, and will continue to be, executed for crimes they did not commit or ones for which the law does not allow the death penalty.

The next part of this Report explains why the high number of flawed capital verdicts poses an intolerable risk of seriously unreliable capital outcomes, including execution of the innocent, and why it therefore is imperative to study the causes of high rates of capital error. Thereafter, Parts III-VII identify and interpret factors associated with high error rates and with error itself at the state level (where overall capital policy is largely made), the county level (where decisions whether to charge capitally and whether to convict and condemn are mostly made), and in individual federal habeas cases. Part VIII then lays out some options for reducing the risk of serious error and thus of executing innocent people and others for whom the law bars death as a punishment.

II. Why It Is Important to Study the Causes of Reversible Error in Death Cases.

The Part discusses three reasons to study factors related to serious capital error:

- ***A Broken System* found that American capital verdicts were compromised by high numbers and rates of serious, reversible error throughout the 1973 to 1995 period.**
- **The problem of reversible error may be worse than *A Broken System* reports because its conservative methods probably undercount the numbers and rates of error.**
- **The errors *A Broken System* documents reveal a significant risk that the existing capital system is convicting, capital sentencing and executing individuals who are innocent of any crime, or any for which the law allows a death sentence.**

A. A System Overwhelmed by Error

At p. 9 above is an updated National Report Card documenting the functioning of the death penalty in the United States from 1973 to 1995.⁹⁰ **Nationally, state and federal courts reversed, 68% of all capital verdicts imposed and fully reviewed between 1973 and 1995.** Based on **5401** final state and federal court decisions in capital cases—**4546** final direct appeal decisions, **257** final state post-conviction reversals, and **598** final federal habeas decisions—*A Broken System* found that:

- **Substantial proportions of the death verdicts reviewed at each of three review stages were reversed: 41% of the verdicts reviewed by state supreme courts on direct appeal; at least (probably well more than) 10% of the remaining verdicts reviewed by state post-conviction courts;⁹¹ and 40% of the remaining verdicts reviewed by federal habeas courts.**
- **Serious, reversible error was the rule, not the exception, among states using the death penalty in the 1973-1995 period. As Figure 1A, p. 51 below, reveals, over 90% of the states with capital verdicts reviewed at all three review stages had overall error rates of 50% or more; 85% had error rates of 60% or more; three-fifths had error rates 70% or more.**
- **Serious, reversible error also was the rule throughout the study period. As Figure 2A, p. 55 below, shows, over half of all death verdicts reviewed were found seriously flawed in 17 of the last 18 study years. In 18 of the 23 study years, the overall error rate was 60% or more.**
- **Reversals occurred far more often in the capital cases studied than it does in non-capital cases. Data on non-capital cases is sparse, but our best estimate—revised since *A Broken System* was**

released—is that the reversal rate in non-capital cases is less than 10% and probably less than 5%. **Capital verdicts are 7 to 14 times more likely to be reversed than non-capital ones.**

Reversible error affected both phases of trial: 38% of state post-conviction reversals tainted verdicts reached at the first trial phase (sometimes loosely called the “guilt” phase); the rest affected verdicts at the second phase (sometimes called the “sentencing” phase).⁹² 43% of the errors found on federal habeas tainted first-phase verdicts, while 58% compromised second-phase verdicts.⁹³

As the above paragraph begins to show, **reversible error undermines the reliability of many guilt-focused, as well as many sentencing, decisions.** A more precise allocation of error between guilt and sentencing decisions is possible, however. In capital cases, the question of guilt or innocence includes the question of the level of offense the defendant committed: Was it manslaughter (as opposed to negligent or non-culpable homicide), second- or first-degree murder or capitally aggravated murder? The law allows the death penalty only for this last category of murder. A few death penalty states assign all these decisions to the first phase of trial. Most states, however (*e.g.*, Alabama, Arizona, the Carolinas, Florida, Georgia, Illinois, Indiana, Mississippi, Pennsylvania, Tennessee and Washington) make the last of these decisions at the second phase of trial.⁹⁴ In these states, not only all error at the first phase of trials but also some error at the second phase undermines the decision that the defendant committed capitally aggravated murder.⁹⁵ Counting all error tainting the decision of the level of offense, if any, that the defendant committed reveals that **at least half the serious capital error found on state post-conviction and federal habeas review was guilt-innocence-related error.**⁹⁶

There are three reasons why **all error requiring capital verdicts to be reversed and retried is serious, whether it affects the reliability of the finding that the defendant committed capital murder or the reliability of the finding that death is the proper penalty for the crime:**⁹⁷

- The decision whether to take a person’s life for a crime is as serious as any decision a state can make. As the Supreme Court said in the leading capital punishment decision of the modern era, the legal treatment of capital punishment in the United States:

rests squarely on the predicate that the penalty of death is qualitatively different from a sentence of imprisonment, however long. Death, in its finality, differs more from life imprisonment than a 100-year prison term differs from one or only a year or two.⁹⁸

The decision to impose the death penalty depends on two crucial conclusions—(1) that the defendant committed capital murder, and (2) that the law permits his execution for that offense given the relevant aggravating and mitigating factors. Because error undermining the reliability of either of these conclusions compromises the most serious of state decisions, all such error is a matter of serious policy concern that warrants study and reform.

- The decision to impose death is a matter of grave concern to all participants in the criminal justice system, not just the defendant. This is clear from large impact on the victim’s family, the community, the taxpaying public, the media, and the trial jurors, lawyers and judge of a decision to impose death instead of a lesser sentence. That impact typically is far greater than the affect of a decision to impose life without parole rather than a term of years. So is the impact of a reviewing court’s decision to *overturn* a death sentence—and of a retrial verdict rejecting that penalty—compared to the impact of decisions overturning other sentences.
- It is difficult and dangerous to study errors affecting the reliability of one part of a capital trial while ignoring errors affecting other parts. When a lawyer is incompetent or a prosecutor withholds material evidence, the harm nearly always affects all decisions the jury makes. A decision that reversible error occurred affecting any part of the capital verdict thus undermines the fairness and accuracy of the trial as a whole. In such situations, the typical reviewing decision overturning a death sentence but not the finding of capital murder holds that there was, say, a 45% or 60% or 90% probability the violation affected the sentencing decision but “only,” say, a 30% probability it affected the guilt determination. To treat such a case as not serious is not only to treat decisions between life and death as unimportant, but also to ignore evidence compromising the verdict of guilt. This is particularly so given the consensus among most modern public and private enterprises that people and systems prone to error in any part of the process of making products or delivering services are probably prone to error in all parts of the process—so that the detection of serious error is as important for what it says about the safety and effectiveness of the enterprise as a whole as for what it says about the particular error.⁹⁹

For these reasons, our best judgment is that **the most crucial public policy issue in the death penalty area is the reliability of verdicts choosing between life and death based on whether the defendant committed capitally aggravated murder and whether the accompanying**

circumstances allow death as a penalty. It is the reliability of that decision on which we focus.

B. A System that May Be Fraught with Even More Error than Our Cautious Methods Counted

This section discusses **seven reasons why our counts of error are likely to be under-inclusive because they probably understate the number of death verdicts reversed due to serious error, and because they define as error-free some verdicts that were approved for execution by all three levels of review but later were shown to have condemned innocent people:**

- Identifying all decisions finally reviewing capital verdicts during the 23-year study period is difficult. We undoubtedly missed some. Because decisions reversing death verdicts are harder to find than ones approving them, the decisions we missed likely include more reversals than affirmances. As a result the reversal rates we report are likely lower than the actual rates.
- Our conservative method of calculating error rates at the second, state post-conviction review stage assures that the reversal rates we report for that stage are lower than the actual rates.
- We do not count scores of reversed capital verdicts that occurred when the Supreme Court struck down entire capital-sentencing statutes. Instead, we count only reversals under statutes that generally satisfy the Court’s “guided discretion” requirements.
- We count only reversals occurring on the federal courts’ review of a first habeas petition, omitting a number of reversals in second or successive federal habeas proceedings.
- We use a restrained method of calculating overall reversal rates at the three review stages.
- By not counting death verdicts imposed during the study period that were still under review when the period ended, we understate error found at the federal habeas stage.
- We count only errors the courts actually discovered and relied on as a reason to reverse death verdicts. As a result, we omit many errors the courts found but chose to overlook, and some serious errors the courts failed to spot, including ones that put innocent people on death row.

1. Decisions reversing death verdicts are harder to find than ones approving them.

Information about decisions reviewing capital verdicts is unevenly available and harder to find when decisions reverse than when they affirm capital verdicts. If we missed cases, therefore —and

we almost certainly missed some—they are more likely to be reversals than affirmances.

There is no official, publicly available list of the decisions in, or outcomes of, capital appeals for any state in the country. The only systematic way to find those decisions, therefore, is by using the names of death row inmates as search criteria in data bases collecting judicial decisions. Regrettably, neither the federal government nor any capital state has a list of all people sentenced to die between 1973 and 1995, or even “snapshot” lists of persons on death row at given moments during the period. The unofficial list of people on death row across the country kept by the NAACP Legal Defense Fund (“LDF”) does provide snapshots of the people on death row, but only at four points each year. People on death row for only brief periods in between LDF’s reporting dates are missed. The one complete list of a *subset* of death row inmates is LDF’s roster of persons who were executed after their death verdicts were approved by all courts to which they appealed.

Under these circumstances, **the best information for identifying court decisions reviewing capital verdicts is biased against finding decisions that overturn those verdicts and in favor of finding decisions that approve them.** As noted, death row inmates whose verdicts were approved at all stages of review and were executed—including ones executed as late as the middle of 2000, but whose court cases were decided during our 23-year study period—are all counted in our study. Because each court decision approving a death verdict makes it more likely that the prisoner will be executed, and because a person’s execution assures that we found all decisions approving his verdict, each decision reviewing a particular verdict makes it more likely that we discovered that and all other judicial affirmances of that verdict. Judicial decisions reversing death verdicts have the opposite effect: They make it certain that the inmate will *not* be executed under the verdict the court disapproved, and that the decision overturning it is not accessible via the single complete list of

(executed) death row inmates.

Also, the longer someone is on death row before being removed by execution, reversal of the verdict or death from another cause, the more likely it is that he appears in one of LDF's periodic death row censuses and that we found the court decisions in his case. Because decisions approving verdicts either trigger the inmate's execution (which assures that all the affirmances in his case were found and counted) or extend the inmate's time on death row while he pursues the next level of court review, each court affirmance makes it more likely that we found the inmate and the decisions in his case. Decisions reversing death verdicts again have the opposite effect: They remove the inmate from death row, assuring he will not appear on later LDF censuses based on the faulty verdict.

Third, publicly available on-line legal data bases omit some *unpublished* decisions, but include all published ones, so that unpublished decisions are harder to find than published ones. Because at least some courts are more likely to use unpublished opinions to overturn than to approve death verdicts (perhaps because reversals are more controversial than affirmances, creating an incentive to make the former less visible to the public than the latter),¹⁰⁰ decisions available to researchers may be skewed in favor of those that affirm, and against those that reverse, death verdicts.

We worked hard to find every decision finally reviewing a capital verdict between 1973 and 1995, including by supplementing LDF's informal snapshot lists of death row inmates with lists and decisions collected intermittently by death penalty resource centers, lawyers and others in particular states, and by the Death Penalty Information Center in Washington, D.C.; keyword searches of legal and newspaper databases; and word of mouth. Using these methods, we believe we found most final decisions—certainly 90% or more. But we undoubtedly missed some, and those we missed are more likely to have reversed death verdicts than to have approved them.

2. We understate error rates at the state post-conviction stage.

To calculate error rates, we worked to identify the number of death verdicts *overturned* as a result of reversible error at each relevant stage of review and then to divide that number by the number of verdicts *reviewed* at that stage:

$$\text{Number Reversed/Number Reviewed} = \text{Reversal Rate}$$

As we discuss in *A Broken System*, this approach proved impossible at the state post-conviction stage because of the high volume of unpublished opinions at that stage.¹⁰¹ To figure reversal rates at that stage, therefore, we tried to find all the death verdicts *reversed* at that stage (the numerator), while using a substitute figure derived from our direct appeal study to estimate the number of verdicts *reviewed* at that stage (the denominator). The substitute figure was the total number of death verdicts that cleared the first (direct appeal) stage of review and thus were *available* for state post-conviction review. Because we know for certain that, at any given time, a substantial number of death verdicts available for state post-conviction review (because they cleared direct appeal) are stuck in the delay-ridden review system and thus have not been finally reviewed by state post-conviction courts, we also know for certain that substituting the number of verdicts available for review as a proxy for the number actually reviewed inflates the latter. By systematically inflating the denominator, we systematically *deflated* the error rate at that review stage. (If the actual rate is, *e.g.*, 3/5 (60%), inflating the denominator to, say, 3/6 (50%) or 3/7 (43%) produces a deflated estimate.)

The extent of our under-estimate of state post-conviction reversals is indicated by *Nashville Tennessean* reporter John Shiffman's study of Tennessee reversals of capital verdicts between 1980 and 2001.¹⁰² Shiffman identified all state post-conviction affirmances as well as reversals, enabling him to determine Tennessee's actual state post-conviction reversal rate during his 22-year study

period—which overlaps but is not the same as our 23-years study period. The actual rate is **51%** (35/68)—more than triple our conservatively estimated rate for Tennessee of 16%. **If this difference held nationally, the 68% overall national error rate we calculated was instead 75%.**¹⁰³

3. We exclude dozens of reversals caused by Supreme Court rulings overturning entire death statutes.

Our study period begins in 1973, the year after the Supreme Court in *Furman v. Georgia*¹⁰⁴ overturned all prior death statutes and verdicts in the nation, and ordered states to adopt new statutes satisfying the Court’s revised standards. As a result, ***A Broken System counts none of the hundreds of death verdicts Furman reversed.*** We use 1973 as a starting point because it is then that states began using modern capital statutes designed to avoid the defects found by *Furman*.

Even so, we do not count all court reversals of death verdicts imposed under statutes passed after and designed to correct the problems found in *Furman*. In decisions in the mid- and late 1970s, the Supreme Court established rules under which it and other courts overturned entire capital statutes in Louisiana,¹⁰⁵ North Carolina,¹⁰⁶ Ohio¹⁰⁷ and at least 12 other states.¹⁰⁸ In doing so, the courts reversed many 1973-and-after death verdicts. We do not, however, count those reversals, though they occurred during the study period. **We instead count only reversals occurring (1) during the study period but (2) after the relevant state adopted a death-sentencing scheme that passed Supreme Court muster.** We omit these reversals to assure that our measure of serious error is constrained and reflects only the performance of statutes like those currently in effect across the nation.

We do not know how many 1976 reversals there were nationally. We know there were 20 in Louisiana.¹⁰⁹ **Had we counted these reversals, Louisiana’s direct appeal reversal rate would have risen from 47% to 55%, and its overall reversal rate would have gone from 63% to 69%.**

4. We exclude reversals in second or successive federal habeas cases.

We count only federal habeas reversals occurring in an *initial* round of federal review. During the study period, federal courts reversed at least seven capital verdicts in *second* or *successive* habeas proceedings that were previously upheld in initial habeas cases. We thus count five death verdicts as having been affirmed at all three levels of review, even though they in fact were reversed during the study period in successive federal habeas proceedings.¹¹⁰ This again was an effort to be cautious: Because successive habeas reversals are considered controversial by some, we omit them.

In sum, our best estimate is that if we had not used the above four conservative ways of collecting and counting reversals, the overall national reversal rate would be between 75% and 80%.

5. We use a cautious method of calculating overall error rates.

To arrive at an overall reversal rate of 68% and success rate of 32%, we combined the reversal and success rates at each of the three review stages, as follows:

$$\begin{array}{r} .41 \text{ (reversed on direct review)} \\ + \quad .10 \text{ (reversal rate on state post-conviction)} \times .59 \text{ (surviving direct appeal)} = .06 \text{ of} \\ \text{original group reversed on state post-conviction} \\ \hline .06 \\ \hline .47 \text{ (reversed cumulatively on state direct appeal and state post-conviction)} \\ + \quad .40 \text{ (reversal rate on federal habeas corpus)} \times .53 \text{ (portion of original group surviving} \\ \text{state review)} = .21 \text{ of original group reversed on habeas corpus} \\ \hline .21 \\ \hline .68 \text{ (overall reversal rate)} \end{array}$$

$$\text{Overall success rate} = 1.00 - .68 = .32.^{111}$$

Using this method of calculating overall success and error rates treats many death verdicts as

having been *approved* at earlier stages, though they eventually were *reversed* at a later stage. This has led to criticism that we greatly overstate the capital success rate, which instead should be calculated as the number of death verdicts that survived review at *all* stages (358 verdicts) taken as a proportion of death verdicts that were in the review process (4546 verdicts), or **8%**. According to this criticism, the success rate we report (32%) is 400% *greater* than the actual success rate, and the failure rate we calculate (68%) is about 40% *lower* than the actual failure rate. This method of calculating overall success rates may have some appeal, but we reject it in order to be cautious in our judgments. While accurately indicating the exceedingly small proportion of death verdicts imposed during the study period that survived all court review in the period, it ignores the fact that some verdicts did not make it through the review process, *not* because they eventually were reversed, but because after being approved at an earlier stage, they got stuck in the system while awaiting review at a later stage. To take account of that fact, we analyze the combination of all verdicts' success and error rates at every stage at which a verdict was reviewed. This provides a more complete picture of how verdicts fare on review than looking only at the bottom line.¹¹² **The effect of this cautious judgment, however, is a more conservative error rate than a different approach would calculate.**

6. We understate the amount of error by not counting outcomes of verdicts imposed in the study period but still under review when the period ended.

To confine our reversal data to known outcomes and avoid extrapolation, we calculate reversal rates by counting only the results of death verdicts actually and finally reviewed at one or more review stages during the fixed study period ending in 1995. This prevents us from counting reversals, or estimating reversal rates, for the many death verdicts imposed during the study period that were not finally reviewed until after that period ended. This limitation probably leads us **to understate the federal habeas reversal rate among verdicts imposed in the study period.** During

the study period, it took a year or two longer on average for flawed than for unflawed death verdicts to be finally reviewed at the federal habeas stage.¹¹³ As a result, a disproportionately high number of the delayed outcomes we did not count were reversals, while a disparately high number of the more timely outcomes we did count were affirmances. This almost certainly led us to understate the actual reversal rate among death verdicts imposed in the study period.¹¹⁴ (There is no analogous disparity in the time it takes for death verdicts to be approved or reversed at the first, direct appeal stage.¹¹⁵)

This point shows why it would be “absurd” (to borrow Professor Joseph Hoffmann’s conclusion¹¹⁶) to assume that the outcomes our study did not count because they were delayed beyond the study period were all later *approved* by the courts. Given that 68% or more of the death verdicts imposed during the 1973-1995 period that were fully reviewed during the period were reversed, and given the above reason why 1973-1995 verdicts that were finally reviewed after 1995 are even *more* likely to have been reversed, it would be irresponsible to base any policy judgment on an assumption that verdicts imposed during the 1973-1995 period but reviewed after 1995 were all affirmed. We point this out because that unreasonable assumption is the premise of a method of calculating reversal rates that has been proposed.¹¹⁷ That method would divide the number of verdicts reversed during the study period by the number of verdicts *imposed* in the period, even if the verdicts were never reviewed during the study period. Doing so assumes that the only 1973-1995 verdicts that were ever reversed were those reversed in 1995 or before—*i.e.*, absurdly, that the thousands of 1973-1995 verdicts that were finally reviewed after 1995 were all approved. **Even when deflated in this irresponsible way, the overall reversal rate is still a “depressing” 40%.**¹¹⁸

7. Our measure of serious error is conservative.

We count as errors only those defects that lead courts to conclude that capital verdicts are too flawed to be carried out and have to be retried. Such errors impose a heavy burden on the system. They squander resources spent on failed trials and lengthy appeals, waste still *more* resources on retrials, frustrate the expectations of victims, and undermine the system's deterrent and retributive goals as well as the integrity of the courts.¹¹⁹ **All such error is serious, most of all, because it compromises the reliability of the verdict that the defendant is guilty and deserves to die.**

For these reasons, the errors we count include only serious errors. But they do not include *all* serious errors—or even the *most* serious ones, because we count only errors courts spotted and chose to cure. We limit ourselves to these errors because they are serious enough to cause courts to reverse and order costly retrials, and because they are objectively identified by court decisions without requiring controversial judgment calls by us about what to count. **This cautious approach assures, however, that we fail to count many disturbing errors, including the *most* disturbing ones: mistakes that put innocent people on death row but were missed by all phases of court review.**

Uncounted error falls into two categories: errors that appellate courts find but choose to ignore (so called “harmless,” “non-prejudicial” and “waived” error), and errors—even egregious ones—that appellate courts fail to discover.

a. Errors courts found that we did not count.

Start with the first category. Suppose a capital defendant's appointed lawyer is addicted to alcohol or drugs, which he ingests repeatedly during the trial, preventing him from providing a decent defense. Or suppose the lawyer fails before trial to interview any witnesses or otherwise to investigate the case. The Supreme Court has said that this behavior falls below the minimum the Constitution

requires.¹²⁰ But there are many cases involving this or equally poor representation by capital defense attorneys that courts did not reverse, and that we did not count as serious error, because the defendant could not additionally prove, years after the fact—as the controlling law requires—that a better lawyer would probably have changed the outcome of the trial.¹²¹ We likewise did not count numerous death verdicts where police and prosecutors suppressed evidence suggesting that the defendant was innocent or did not qualify for the death penalty or presented evidence or made arguments the law forbids; nor did we count cases where trial judges barred defense evidence or cross-examination of accusing witnesses the law permits, or gave instructions misinforming jurors about the law governing their determination of guilt or innocence, and sentence.¹²² We declined to count these many errors because reviewing courts ruled them “not prejudicial” (meaning the defendant could not prove that, if the error had not been committed, the outcome of the case probably would have been different) or “harmless” (meaning the error had no or no “substantial” effect on the outcome).¹²³ In our federal habeas study, we tabulated the number of death verdicts that courts refused to reverse because any error was “harmless.” (We did not, however, tabulate findings of “no prejudice.”¹²⁴) Harmlessness findings were made in **18%** of the cases in which death verdicts were approved on federal review.

Courts declined to reverse, and thus we declined to count, still other errors occurring at capital trials that evidently *were* prejudicial but were allowed to stand because the defense lawyer incompetently forgot to object. When lawyers negligently fail to object to errors, courts usually will not correct the error, even if it was serious and would have required reversal if the lawyer had done his job.¹²⁵ Such “waivers” were found in **32%** of the habeas cases in our study in which relief was denied. **Overall, federal courts chose to overlook error as harmless, waived or both in 44% of the habeas cases in which verdicts were upheld.**¹²⁶ Including just a third of these uncounted errors

identified at only one of three review stages would raise the overall national reversal rate **from 68% to 73%**.

b. Errors courts failed to find.

Between 1973 and the first week of January 2002, 99 men and women sentenced to die were later exonerated by official findings that they were **not guilty** of the capital offense.¹²⁷ In each case, the original trial verdict reached a conclusion that either was factually wrong because the defendant was innocent, or legally wrong because there was not enough evidence for a reasonable grand jury to indict or a trial jury to convict. In **63** of the 99 exonerations, the mistake was discovered only after the highest state court had upheld the capital verdict at least once. On **35** of those occasions, either a state, or a state and a federal, post-conviction court affirmed the verdict a second or third time before the mistake was discovered. And in some cases, all three levels of courts missed the error, clearing the way for execution, before someone else discovered the mistake. In most of the 99 known cases of factually or legally innocent people sentenced to die, therefore, our method of defining serious error led us to count at least one, and often two or three, judicial decisions as finding *no* error.¹²⁸ **Our cautious and objective measure of error—mistakes appellate courts actually discovered and held serious enough to require reversal—assures that we undercount the actual amount of serious error.**

c. **Four illustrative cases in which stringent rules limiting reversals led courts to approve the capital verdicts of innocent men despite a full set of appeals.**

How can innocent men and women be convicted of a capital crime and sentenced to die? And how can the mistakes escape detection by multiple courts that approved the prisoners' execution? Four typical cases provide an answer: **The courts define error serious enough to require reversal so cautiously and under-inclusively that they often hold known errors—even ones that put innocent people on death row—to be harmless, not prejudicial or waived.** Because we use the same judgments to define serious error, our counts of error are also cautious and under-inclusive.

i. Lloyd Schlup was convicted and sentenced to die by Missouri for killing another inmate in prison. After the Missouri Supreme Court on direct appeal, the trial court and Missouri Supreme Court a second time on state post-conviction, and a United States District Court and the United States Court of Appeals on habeas rejected his claims that errors in his case had led to his conviction for a crime another prisoner committed, thus clearing him to be executed, a prison videotape and a guard's testimony about the time of the events revealed by the tape confirmed, as Schlup had always said, that he was in another part of the prison when the killing occurred. Fourteen years after his arrest, Schlup agreed to a settlement of the case so his conviction of capital murder could be withdrawn.

How did three levels of reviewing courts approve this miscarriage—leading A Broken System to count Schlup's verdict among the 32% in which no serious error occurred? The answer lies in the harmless error, no-prejudice and waiver rules noted above. On direct appeal, Schlup objected to the admission of photos supposedly showing that a guard who falsely identified Schlup as the assailant could see the site of the killing from his guard station. The photos had not been "authenticated" by anyone who could say they showed the view of the crime scene from the guard post, rather than from

a different vantage point. Authentication is a legal requirement some call a technicality, and the Missouri Supreme Court treated it as such: “The fact that Maylee [the guard who said Schlup was the killer] . . . did not testify that the photos depicted his exact vantage point,” the court said, although an error, was harmless, so that “[t]he trial court did not abuse its discretion in admitting the photographs.” The Missouri Supreme Court then concluded—as the jurors also apparently did—that the photos strongly “corroborate[d] Maylee's testimony by demonstrating that he could have witnessed the murder from his station.”¹²⁹ In fact, the photographs did not show what Maylee could see from his post; contrary to his trial testimony, the guard could not and did not see Schlup at the scene. But because of the Missouri high court’s reluctance to reverse based on “technical” error, Maylee’s flawed identification sent a man to death row for a crime someone else committed.

This same treatment of uncorrected (and so, by us, uncounted) error continued on state post-conviction review. There, the Missouri Supreme Court chose to ignore another, this time *non-technical*, error because it was not “prejudicial.” The known error was the prosecutor’s “fail[ure] to disclose exculpatory evidence” tending to show the defendant’s innocence. State lawyers failed to reveal that the warden of the prison where the killing occurred “had evidence that another individual may have committed the murder, and the warden [told police] he did not believe appellant would intentionally hurt someone.” Although prosecutors are required to disclose exculpatory evidence, their failure to do so is ignored if the defendant fails to show that the prosecutors’ withholding of exculpatory evidence probably changed the trial outcome. Applying this exception, the Missouri high court chose to ignore the error, calling mere “rumor” the warden’s belief about what occurred in his prison and the information making him think another man was the killer.¹³⁰ The warden was correct, of course. But the courts refused to cure the error (and we did not count it) for lack of “prejudice.”¹³¹

The same thing happened on federal habeas review. There, Schlup showed that his trial lawyer incompetently failed to interview or call three known alibi witnesses. The court did not dispute that the lawyer failed to give Schlup decent legal help, but the court chose to ignore the error because it was not shown to be prejudicial. Accepting the lawyer's claim—though he never talked to the three witnesses—that their testimony that Schlup was not near the killing would be “repetitive or . . . damaging,” the court ruled that Schlup had not shown that the denial of his right to counsel had probably led to the wrong outcome. As a result, the error went uncorrected by all three stages of court review (and uncounted by us), and the three alibi witnesses went unnoticed by the judicial system until the videotape and supporting testimony finally showed that Schlup was with those witnesses, away from the killing, when it occurred.

The procedure Schlup used to prove he was not guilty after all three regularly available review stages failed him no longer exists. Congress decided to abolish it in 1996.¹³²

ii. Earl Washington's death verdict is also counted by us as error-free because it was affirmed at all three stages of court review. A recent press account describes Washington's conviction and death sentence, despite his innocence:

“Did you stab a woman in Culpeper?” the state police detective asked. The illiterate farm worker nodded.

“Was this woman white or black?”

“Black.”

A few questions later, Special Agent C. Reese Wilmore tried again. “Was she white or black?”

This time Earl Washington Jr. said, “White.” That answer launched the biggest mistake ever made by Virginia's judicial system—and landed Washington on death row.

It wasn't until Oct. 2 [2000]—17 years after that police interview—that new DNA

tests cleared Washington of the 1982 rape and slaying of Rebecca Lynn Williams. Recent interviews with Washington and Williams’s widower as well as dozens of police officers, judges and lawyers involved in the case turned up warnings that went unheeded along the way:

* Police and prosecutors moved forward with a case based almost entirely on a statement full of inconsistencies from an easily persuaded, somewhat childlike special-education dropout. Washington told investigators he “stuck her . . . once or twice,” but Williams bled to death from 38 stab wounds. He said she was alone. But there was a baby in a playpen and a toddler roaming the small apartment. The defense made no mention of most of these inconsistencies during the trial.

* A judge ruled that the statement was admissible after hearing from a state mental health expert that a man with an IQ of 69 was competent to waive his rights to a lawyer during initial questioning—even though Washington still doesn’t know what the words “waive” and “provided” mean.

* No eyewitness or physical evidence put Washington at the scene. His blood type did not match a semen stain, and police instructed the state lab not to test key hair evidence. A judge rejected defense efforts to test the hair, and the defense lawyers never told the jury about the mismatched blood types.

* Six courts rejected the inmate’s claims of innocence, *including a panel of federal judges who determined that Washington’s trial attorney had failed to meet minimal standards but upheld the conviction anyway*. Virginia’s appeals judges . . . ruled that Washington’s confession was properly admitted and the blood evidence was inconclusive.

* * * * *

In October, Gov. James S. Gilmore III (R) pardoned Washington after more sophisticated genetic testing found no trace of him at the scene.

* * * * *

Although state officials have reopened the investigation, Williams’s widower, Clifford, feels betrayed by Culpeper authorities, who assured him that Washington was the right man and now won’t talk to him, he says.

“What do they have to hide? Why won’t they talk about it?” he asked in a recent interview. “I went for nearly 18 years believing Washington did it. Now I don’t know what to think.”¹³³

According to another news report:

Genetic material found on Williams’s battered body did not match [Washington], her

[the victim's] husband or any man in the state's DNA data bank of convicted felons. But lab tests done on a blue blanket at the crime scene found the DNA of a convicted rapist [who was never punished for the 18-year-old offense], Gov. Gilmore said in a statement.¹³⁴

These accounts again show that the court standards for judging serious error—the same ones we use here and in *A Broken System*—were too forgiving to spot the errors leading to Washington's false conviction. Answers to more specific questions about the case compel the same conclusion.

How could the courts have ruled that a retarded man, whose memory of the events clashed with the known facts on several crucial points, could understand his rights and validly confess? Here is what the Virginia Supreme Court said:

On appeal, the defendant argues . . . that he made no waiver of his right to counsel [when he made his alleged confession] on May 22, 1983, and that he was, in any event, incapable of making a voluntary and intelligent waiver of his constitutional rights. . . . These contentions lack merit. The record clearly shows that on at least three occasions . . . [Washington] gave his questioners clear indications that he understood and waived his rights, both orally and in writing.¹³⁵

Washington's inexperienced trial lawyer had a copy of a blood report showing that all the semen evidence at the crime scene had a blood type *different* from Washington's. He decided the report wasn't important and never told the jury about it.¹³⁶ At first, the U.S. Court of Appeals thought this might be incompetent representation, and ordered a hearing:

[Washington's] allegation [that his lawyer was incompetent] was supported by 2 affidavits. One, by an . . . expert in the field, opined that the laboratory reports of the blood type and PGM [enzyme] type of the semen stains, as compared to Washington's, excluded Washington as the depositor of the semen. The other, by his trial counsel . . . stated that counsel had received the laboratory reports but did not recognize their arguably exculpatory nature.

The district court rejected this claim of ineffective assistance without an evidentiary hearing on alternative grounds: that counsel's conduct, as alleged, did not fall outside the range of acceptable professional conduct, and that in any event there was no reasonable probability that the outcome of the proceeding would have been different but for the challenged conduct. . . .

If, as Washington alleged, his counsel failed to offer available evidence which in a significant way drew his factual guilt in issue, counsel's performance obviously fell below an objective standard of reasonable professional conduct, unless some cogent tactical or other consideration justified it. . . . The allegation that the laboratory reports indicated Washington's blood type as O with PGM type of 2-1 whereas four samples of the semen stains on the blanket from the crime scene showed blood type A with PGM type of 1, was undisputed. The allegation that this disparity of types indicated that Washington could not have been the depositor of the semen in the stains was supported by the . . . affidavit of a . . . qualified expert that was not disputed by opposing expert opinion or other evidence.

* * * * *

[As for the ruling that] there was no reasonable probability, given the evidence of Washington's guilt, that the result of the proceeding would have been different had the challenged conduct not occurred, . . . we believe the district court could not properly make that assessment without an evidentiary hearing [Unless shown otherwise at a hearing] . . . , the exculpatory quality of the forensic evidence . . . made it reasonably probable that had it been laid before the jury, it would at the least have created in that body a reasonable doubt as to guilt or resulted in the recommendation of a lesser sentence reflecting that doubt.

[T]he evidence of guilt presented to the jury . . . was not without its difficulties The evidence consisted essentially of a confession obtained by interrogation almost a year after the crime, from a mildly retarded person upon whom suspicion had not earlier focused during the crime's investigation, and who was not indeed suspected when the critical interrogation which elicited his inculpatory statement was commenced, apparently blindly, while he was in custody in connection with an unrelated crime.¹³⁷

After holding a hearing, the lower court ruled that the lab report indeed showed the semen stains did not match Washington, but concluded that the lawyer's error in failing to tell the jury about the report should be ignored because it was *not prejudicial*. On appeal, the higher court agreed—revealing the strictness of the courts' (and our) definition of error serious enough to require reversal:

We cannot say the district court erred in concluding that petitioner was not prejudiced by [his lawyer's failure to introduce] the forensic evidence. . . . Even assuming that petitioner had presented the stained blanket and his experts at trial, the prosecution still had a strong case against petitioner [based on "Washington's confession to the crime"]. . . . [G]iven the case's strength, we cannot say that inconclusive forensic evidence would have overcome it.¹³⁸

iii. Anthony Porter is another retarded victim of a flawed capital trial who spent 17 years on (Illinois's) death row for a crime another man committed. His death verdict also was upheld at all

three stages of court review, and so is counted by us as error-free. But as the courts knew all along, Porter's trial was in fact marred by two major problems—a biased juror and an incompetent lawyer. The courts held the errors unimportant given the supposedly strong evidence of guilt.

On his first appeal, Porter pointed out that one of the jurors who voted to convict and condemn him had failed to tell the judge, when asked directly, that she knew the mother of one of the murder victims. Once on the jury, the woman urged the other 11 to “vote guilty right then . . . before any discussion was had on the evidence.” The Illinois Supreme Court ruled there was no prejudice because Porter's trial lawyer showed only that the juror “knew the victim's mother as someone who attended the same church that she attended,”¹³⁹ but did not show that “the relationship between the juror and the victim's mother” was close. When Porter's new lawyer, on his second appeal, supplied the missing information—that the juror and the victim's mother were good friends—the court again chose to ignore the error, saying the error was waived by the first lawyer's incompetent failure to discover the information.¹⁴⁰

That incompetence went even further. Due to a dispute with Porter over his fee, the lawyer refused to interview or call five witnesses (including three close relatives of the victims) who said a man named Alstory Simon had killed the victims in a fight over drugs. Without disagreeing that the lawyer incompetently failed to investigate evidence identifying a different killer, the second reviewing court ruled the error *non-prejudicial*—again showing how narrow the courts' (and our) measure of serious error is:

Even assuming counsel performed incompetently in not generating the proposed testimony, sufficient prejudice did not result to support the claim. . . .

Prejudice is measured by looking at findings unaffected by error and accounting for the error's effect on remaining findings to answer whether the decision would “reasonably likely” have been different. The assessment “must exclude the possibility of arbitrariness,

whimsy, caprice, ‘nullification,’ and the like.” The showing of prejudice must be a strong one. [That standard was not met here, because t]he evidence against defendant [Porter] was considerable.¹⁴¹

Both errors again went unremedied at the federal habeas stage of review. As for the biased juror and several other errors, the federal district court wrote:

Porter[’s lawyer] did not [properly] raise several of his asserted grounds for relief in the Illinois courts . . . ; as such, those arguments are procedurally barred. “In all cases in which a state prisoner has defaulted his federal claims in state court . . . , federal habeas review of the claims is barred . . . Under these standards, the following claims now raised by Porter are procedurally barred: use of allegedly perjured testimony, use of constitutionally unfair procedures, and denial of an adequate hearing on the extent of juror bias”¹⁴²

As for his trial lawyer’s incompetent failure to interview five witnesses who identified Alstory Simon as the killer, the federal district court again illustrated how difficult it is to show that even clearly below-standard lawyering is prejudicial enough to be reversible error (and, thus, to be counted by us as serious error). To overturn a capital conviction, the court said, a

“defendant must show that there is a probability that, but for counsel’s unprofessional errors, the result of the proceeding would have been different.” . . . [D]eficient performance, by itself, “does not warrant setting aside the judgment of a criminal proceeding if the error had no effect on the judgment.” Porter has not made the requisite showing . . . [because he] was convicted by a jury which heard considerable evidence that Porter committed the crimes.¹⁴³

A federal appellate court agreed, rejecting Porter’s claim that he was prejudiced by his lawyer’s admitted incompetence with a rhetorical question that speaks volumes about how hard it is to satisfy the courts’ (and our) test for serious, reversible error:

Porter asserts that his counsel should have presented evidence that Alstory Simon and Inez Johnson were responsible for murdering Green and Hilliard. Porter has offered a number of affidavits and sworn statements by people in the neighborhood stating, among other things, that Simon and Johnson went to the park that night with Green and Hilliard, that Simon had just been released from the penitentiary and had a financial dispute with Hilliard regarding drug dealing, that Hilliard was seen arguing in the park that night with a man who was not Porter, . . . that Simon threatened someone who asked Johnson what had happened at the park[, and that Inez Johnson had been overheard admitting that she and Simon committed the killings]. None of this evidence was offered at trial, although the State concedes that Simon

and Johnson were in the park with [the victims] at some point on the night of the murders.

. . . [But h]ow much credence can we reasonably give to third-hand information when it contradicts two eyewitnesses and a police officer who put Porter right at the scene of the crime?¹⁴⁴

These decisions cleared the way for Porter's execution, which was hours away when he received an emergency reprieve on the ground that he might be too retarded to understand why he was being executed. In the ensuing pause, some Northwestern undergraduate students, as a class project, tracked down Alstory Simon in Milwaukee where he had fled after Porter's arrest. The result—when someone finally followed-up on the leads Porter's lawyer had incompetently ignored—was Simon's taped confession to the killings. Porter was released. Simon pleaded guilty to killing the two people,¹⁴⁵ and is believed to have killed a third person after Porter's arrest.¹⁴⁶

The *Chicago Tribune's* report on the Porter case again illustrates how high the courts (and thus *A Broken System*) set the bar for establishing serious, reversible error in capital cases:

It took two days to put Anthony Porter behind bars and send him on his way to Illinois' Death Row. It took nearly 17 years to set him free.

Between those bookends of Porter's incarceration, the criminal justice system failed him at several critical turns, according to police and court records as well as interviews.

When initially investigating the crime, for instance, police never seriously considered other suspects, and they discounted Porter's alibi.

Witnesses who could have exonerated him lied, although some say they were coerced by police. And others who knew the real details of the crime kept silent, even when they knew an innocent man faced execution.

Although the justice system is supposed to ensure that everyone—even the destitute—is provided an attorney to defend himself, the reality is that Porter's lack of financial resources meant he received only the most basic defense, even though he was facing the most serious punishment.

By his trial attorney's own admission, efforts on Porter's behalf were spare. . . .

After Porter’s conviction, judges in state and federal courts—including the U.S. Supreme Court—turned away more than a half-dozen of Porter’s appeals and other filings, dismissing arguments raised on grounds ranging from ineffective counsel to claims of innocence.

* * * * *

[A]s Porter’s case moved through the courts, Chicago police and the Cook County state’s attorney’s office saw their work validated. Questions of innocence were denied by higher courts, and the procedural appeals were turned away. An appeal that examined whether a juror was biased was unsuccessful.

“We had good claims,” said Daniel Sanders, Porter’s appellate attorney. “It’s just because of the tough rules in the court that we kept losing.”¹⁴⁷

iv. Frank Lee Smith’s recent exoneration for a 1985 Florida rape-murder followed the same distressing pattern, but with a tragic twist:

DNA evidence has exonerated Death Row inmate Frank Lee Smith of the rape and murder of an 8-year-old Broward County girl.

But he died 11 months ago.

Another man, Eddie Lee Mosley, is now the main suspect in Shandra Whitehead’s 1985 death, police and prosecutors said. DNA tests have also linked Mosley to the murder of another Fort Lauderdale child . . . police said Thursday. . . .

Smith died of cancer on Jan. 30 while his attorneys and family fought to prove his innocence. . . . The victory that came with this week’s FBI release of the DNA test results was bittersweet, said . . . the Tallahassee attorney hired by Smith’s family to try to clear his name.

“The state prosecutors had resisted testing while Frank Lee Smith was alive and pursuing his appeals,” said the attorney “Once he was dead, they relented and became more cooperative about letting us get the tests done.”

* * * * *

Smith’s sister . . . and his aunt . . . broke down and cried earlier this week when they heard that DNA tests conducted by the FBI had exonerated him.

“They knew from the very beginning he was innocent,” [the lawyer] said. The family believed in Smith’s innocence, he said, because he was convicted on such scant evidence—the word of a witness who later recanted and said she was pressured by police¹⁴⁸

The Florida Supreme Court also recognized flaws in Smith’s trial and the weakness of the evidence against him. But the court relied on the harmless error, no-prejudice and waiver rules, and the strict standards for court relief to affirm Smith’s capital conviction and sentence—which in turn required us to count Smith’s verdict as error free. In its opinion, the Florida high court wrote:

Appellant . . . argues that there were repeated instances of prosecutorial misconduct which cumulatively denied him a fair trial. All but one of these claimed instances are procedurally barred by the failure to object at trial. . . . In the one instance clearly brought to the trial judge’s attention, . . . a relative of appellant claimed she had seen the prosecutor in the hallway coaching an identification witness by identifying the appellant for the witness. The trial judge inquired into the matter and found the relative’s testimony incredible. We see no abuse of discretion.

Appellant argues that the trial court erred in calling a court witness on request of the state which [indicated that the court] vouch[ed] for [the witness’s] credibility Although we have disapproved of calling such witnesses as court witnesses, the error here was harmless. [It was true t]he witness exhibited a hazy recall of non-essential particulars of previous statements . . . [but] on the critical point of his testimony, he unequivocally [but, we now know, falsely] identified appellant in court as the man he had seen on the street just prior to the crimes and as the man he had previously identified in photographic and live lineups.

Appellant argues that the evidence is insufficient to support the convictions because it is largely circumstantial and is not inconsistent with a reasonable hypothesis of innocence. . . . In support, appellant argues that the eyewitness testimony placing him at the crime scene is questionable. This argument was made to the jury and obviously it found the testimony credible. . . . It is not for us to substitute our judgment for that of the jury.¹⁴⁹

* * * * *

As these cases show, **state and federal courts do not reverse death verdicts for weak or technical reasons. Instead, their decisions (and thus our test for serious error) run in the opposite, highly cautious direction: Absent clear proof of error with a proven effect on the verdict, even doubts about guilt do not lead courts to reverse capital verdicts.**

C. A System Fraught with *Serious* Error

The serious nature of the errors leading courts to reverse thousands of capital verdicts during

the 23-year study period may be shown systematically, as well as with the above examples.

Every decision that a death verdict is too flawed to be carried out is serious because it

- torments victims, forcing them to relive excruciating events at a retrial, and risks either dashing their expectations with a noncapital verdict or causing them to endure another round of appeals;
- requires a costly retrial of one or both phases of a capital trial—meaning, more judges, prosecutors, police officers, witnesses, defense lawyers, and court personnel to compensate, and more jurors and lay witnesses to inconvenience;¹⁵⁰
- risks a new set of appeals—meaning still more judges, defense lawyers, states attorneys, and lay and expert witnesses to compensate or inconvenience;
- delays the processing of other court cases; and
- casts the court systems in a bad light.

Reversible error is also serious because of its devastating impact on the *capital* system. The rejection of more than two thirds of all finally reviewed capital verdicts over nearly a quarter-century reveals a system collapsing under the weight of its own errors:

- In the 23-year study period, **2349 death verdicts were reversed and sent back for retrial.**
- In the same period, it took an average of **9 years** from trial to execution; by the end of the period, it was taking **11 years**, and the figure now is **12 years**.¹⁵¹
- Only **5%** of the 5826 death verdicts imposed from 1973 to 1995 were carried out in the period.
- On average since 1981, capital states have managed to execute only about **1.2%** of their death row inmates each year. Those inmates now number about **3700**.

The case studies above and the analysis below reveal an even more compelling reason to take seriously the huge amounts of error found by *A Broken System*: **Neither modern capital trials nor multi-layered appeals are reliable ways to keep people from being sent to death row or executed for crimes they did not commit or for which the law bars the death penalty.**

This section explores the answers to four questions:

- Given what we know about reviewing judges, should we expect them to be stingy or generous with their findings of reversible error?
- Given the rules governing when error is, and is not, reversible, and given the actual reasons for reversal, is it likely that reversible error affects verdicts' accuracy, or is merely technical?
- What do the outcomes of retrials following reversals reveal about the effect of reversible error on the reliability of death verdicts?
- Do patterns of reversible error across states, time and court systems suggest that high rates of error are localized or widespread?

Answering these questions confirms what the case studies show: Not every known error that occurs at capital trials—nor even every error that sends an innocent person to death row—qualifies as reversible error and is included in the 68% rate of serious error discovered by *A Broken System*. **To qualify as reversible error and be counted by us, mistakes in capital verdicts must be *serious*; they must stand out in the record and undermine faith in the result; they must impress reviewing judges—and should impress others—as matters of grave public concern.**¹⁵²

1. Reviewing judges' political and professional incentives dispose them to affirm, not reverse, death verdicts and to make full use of legal rules that allow them to avoid reversing death verdicts despite known error.

Nearly all judges in capital states are elected, and for decades their decisions in capital cases have been closely scrutinized by opponents and voters.¹⁵³ Rulings overturning death verdicts are frequently cited as reasons to remove judges from office, including in well-known cases in California, Mississippi, Tennessee and Texas where judges were turned out of office because of such rulings.¹⁵⁴ Election promises to impose and uphold death sentences for convicted killers are just as routinely given as reasons to vote *for* judicial candidates—or, indeed, as a threshold qualification for being a judge.¹⁵⁵ By contrast, a careful search of the relevant sources reveals not a single report of a judge

being turned out of office or denied election for imposing or affirming a death verdict, including the 99 verdicts imposed since 1973 on people who were factually or legally innocent.¹⁵⁶ Even candidates for *appointment* to the state and federal bench have frequently been denied appointments to higher courts—and, when appointed, denied confirmation—because of occasional votes to overturn death sentences (among many others to affirm them) while serving on lower courts.¹⁵⁷

These political realities are pronounced and one-sided, and convey a powerful message:

Unless the reason is strong and apparent—and even then—voting to reverse a death verdict creates a big risk that judges will be voted off the bench, or denied promotion to higher office. Voting to affirm a death verdict has *no* adverse political effects.

Given this message, the fact that reversals occur with great frequency—and, indeed, that **reversal is the usual final outcome**—indicates the seriousness of error in capital verdicts.

This is particularly so given that **just under 90% of the 2349 capital reversals from 1973 to 1995 were by *elected state judges***.¹⁵⁸ Because reversals by elected judges are good indicators of the seriousness of capital error, the fact that those judges were responsible for the vast majority of capital reversals during the study period is another indication that most reversals reflect poorly on the reliability of the capital system.

The same conclusion may be drawn from the party affiliations of a majority of the unelected federal judges responsible for the remaining 10% of reversals:

- **In 54% of those cases, appointees of Republican Presidents were a majority of the judges voting to reverse the verdict—even though, as we show below, Republican appointees are less disposed to reverse death verdicts than Democratic appointees.**¹⁵⁹
 - i. **In 73% of those cases, one or more Republican appointees were on the review panel, and a majority of them voted to reverse the capital verdict.**
 - ii. **In 80% of the cases, half or more of the Republican appointees voted to reverse.**
 - iii. **In 83% of the cases, at least one Republican appointee agreed that the capital**

verdict had to be overturned.

- **All told, about 95% of the thousands of reversals documented by *A Broken System* were by *elected state judges* or by *Republican appointees to the federal bench*.**

A good measure of the seriousness of defects inspectors find is how disposed they are to reject products. We consider that question in the case studies above and in this section's study of the disposition *against* reversal of the vast majority of *reversing* judges. Studies of appellate review of death verdicts in Illinois, Ohio and Texas address the same issue.¹⁶⁰ They, too, show that **reviewing judges use a wide mesh to sift capital verdicts for reversible error: Only clear and acute error—and not even all of that—is corrected. Yet even these loose inspections find large amounts of reversible capital error.** By this measure, reversible capital error is serious, and the need to study its causes is substantial.¹⁶¹

2. **Legal rules bar judges from reversing based on errors with no effect on the outcome, and most documented reversals are for errors that probably affected the outcome or are inherently prejudicial.**

- a. **Stringent rules limiting reversal.**

The available evidence indicates that judges are “tough sells” when it comes to finding capital error. But do the rules they follow require them to reverse death verdicts for technicalities or other unimportant errors? The case studies above of four innocent men sentenced to die and then approved for execution by three sets of state and federal reviewing judges, reveal the opposite: Technical rules often *bar* courts from reversing, even for serious error. Courts repeatedly found defects in capital verdicts but did not reverse because the defendant's lawyer “waived” the error by not following the rules for pleading claims; because the violation was ruled “harmless”; or because the defendant could not prove what was in fact the case, that admitted errors had destroyed the accuracy of the jury's guilt and sentencing decisions. Because of these limits on relief, courts let pass:

- the confession of retarded defendant Earl Washington containing statements directly contradicted by the most obvious facts of the crime such as the victim’s race—which courts held **reliable enough to support the conviction**;
- the failure of Washington’s lawyer to tell the jury that the blood type of the semen at the scene *excluded* his client as the rapist—**ruled non-prejudicial**;
- the admittedly incompetent failure of Anthony Porter’s and Lloyd Schlup’s lawyers to interview or call witnesses known to have identified someone else as the killer—**also ruled non-prejudicial**;
- the seating in Porter’s case of a juror known to have misled the trial judge about her close relationship with the victim’s mother—**held non-prejudicial and waived**; and
- the apparent unreliability of crucial eye-witness accounts in Frank Smith’s case—**ruled not unreliable enough to undermine the conviction**.

Given these and other case studies,¹⁶² it is probable that waiver, harmless error and non-prejudicial error findings led judges to ignore mistakes of a similar magnitude in most of the 60-plus cases where innocent defendants were approved for execution by one or more sets of reviewing courts.¹⁶³

b. Serious reasons for reversal.

The above analysis suggests that cautious judges applying stingy legal rules are more likely to miss serious error than to reverse based on non-serious error. We are led to the same conclusion by data we collected on the reasons for court reversals at the final two review stages (state post-conviction and federal habeas). These stages inspected only the cleanest 59% of death verdicts, because those stages occurred after direct appeal already had removed the 41% of the verdicts with the most glaring errors. For example, a lack of evidence of guilt is a fairly common reason for reversal at the first, direct appeal stage, but few such obvious errors are left to be caught at later review stages.¹⁶⁴ The two review stages covered by our data also are the ones critics of multi-stage review cite as the most likely to reverse due to technical error not affecting the reliability of verdicts.¹⁶⁵ If the reasons for reversal at those stages—with the cleanest cases and (some say) the most

suspect judges—are clearly serious, there is reason to think that many or most errors caught at the first review stage by the least suspect judges are also serious.¹⁶⁶

At the state post-conviction review phase, **four errors account for 80% of the reversals:**

- **egregiously incompetent lawyering (39%);**
- **prosecutorial suppression of evidence of innocence or that death is not a proper penalty and other police and prosecutor misconduct (19%);**
- **improper instructions to jurors on the law governing when defendants may be convicted and sentenced to die (19%); and**
- **judge or juror bias (4%).¹⁶⁷**

Reversal on the first three these grounds requires proof that the error **probably affected the outcome**, and most errors in the last category are **inherently prejudicial**.

At the federal habeas stage, four analogous errors also account for most of the reversals:

- **egregious ineffective assistance of counsel (a basis for reversing 27% of the death verdicts reviewed on habeas),**
- **misinstruction of jurors (39%);**
- **prosecutorial suppression of seriously exculpatory evidence or other police and prosecutor misconduct (18%), and**
- **judge or juror bias or the deliberate exclusion of African-Americans from the pool of prospective jurors (7%).**

These errors sometimes occurred in the same case. After accounting for the contribution of each error to all reversals, the four errors are responsible for **74%** of all federal habeas reversals.

Together with these errors, four others account for **81%** of the federal habeas reversals:

- **forced trials of mentally incompetent defendants** who could not communicate with their lawyers or understand the proceedings against them—including one who was acquitted after medical treatment restored his competency;¹⁶⁸

- **prejudicially denying impoverished defendants funds for experts on DNA and other forensic analysis;**
- **coerced confessions not found to be harmless; and**
- **trial court rulings excluding exculpatory evidence of innocence.**¹⁶⁹

As is true of these seven types of error accounting for over 80% of state post-conviction and federal habeas reversals, **legal mistakes are rarely a basis for reversing capital verdicts unless they are found to have impaired the reliability of the outcome. This tendency to undercut the reliability of death verdicts is another reason the reversible errors we count are *serious* errors.**

Television and other fictionalized accounts sometimes suggest that the most common reason convictions are reversed is to punish the police for not following technical rules when gathering reliable and damning evidence. As far as our data show, however, courts *never* reverse capital verdicts based on technical error freeing demonstrated perpetrators of serious crimes:

- **Not a single capital verdict reversed at the state post-conviction or federal habeas stage during the 23-year study period was due to a police officer's or other official's unlawful search, seizure or presentation in court of reliable evidence.**¹⁷⁰
- **Instead, most errors found at those stages were:**
 - **defense lawyers' incompetent failure to discover or present evidence that the defendant was innocent or not subject to the death penalty;**
 - **prosecutors' unlawful suppression of the same kind of evidence; or**
 - **trial judges' improper exclusion of or instructions to ignore such evidence.**¹⁷¹
- **Statistical analysis below of traits of cases in which reversals tend to occur finds a **strong relationship between the *weakness* of the evidence supporting a death verdict and the probability of reversal.****¹⁷²

Our data show that **reviewing courts are inclined to, are legally permitted to, and usually do reverse capital verdicts only because of real doubts about the verdicts' accuracy. If anything,**

courts err on the side of ignoring doubts, and approving flawed verdicts. That, even so, most capital verdicts are reversed suggests that capital error, and the frequency with which it occurs, pose a serious problem calling for careful study of why so much error occurs.

3. Retrial results following state post-conviction reversals provide strong evidence that verdicts with reversible error are unreliable.

Above we find that most capital reversals require evidence that the verdict was the result of error rather than a reliable assessment of the facts, and that 95% of the reversals were ordered by elected and Republican-appointed judges who have strong incentives to approve death verdicts absent convincing evidence of unreliability. When reversals do occur, the case usually goes back for a retrial of one or both phases of the capital trial and a new verdict. The question we consider here is whether curing such error on retrial changes the outcome often enough to indicate that most reversed death verdicts are indeed inaccurate and thus that most reversible errors are serious.

Tracing post-reversal outcomes is difficult. They are not reported, and there is no central source of even unofficial information. When we decided late in the data-collection process to make the painstaking inquiries needed to count state post-conviction reversals,¹⁷³ we also undertook to collect retrial results for those cases as well. For the 352 known state post-conviction reversals between 1973 and April 2000, 306 retrial outcomes were known as of April 2000. (The other retrials were still pending, except in 5 cases where the defendant died before retrial or the outcome is unknown.) Our findings run in the expected direction: **Reversed death verdicts tend to end in non-capital outcomes on retrial.** The lopsided proportion of changed outcomes surprised us, however:

- **Discovering reversible error and curing it on retrial changed the outcome of 82% of the underlying capital verdicts.**
- **73% of those retrials ended in a sentence less than death; 9% ended in acquittals.**¹⁷⁴

These findings provide strong additional evidence that reversible error is serious error. Assessing their full implications requires answers to two questions. First, how likely is it that capital retrials end in lesser verdicts because evidence was lost in the meantime, and not because reversible errors led to inaccurate outcomes that retrials cured?

This possibility is substantially dispelled by (1) how evidence is preserved between trials and retrials and the rules on presenting evidence at retrials, (2) the kinds of error leading to retrials and (3) the nature of decisions to accept pleas to lesser crimes. Apart from a finding that evidence at the first trial was false, unreliable or illegal—in other words, unless the verdict was reversed because it resulted from unreliable evidence—all evidence admitted at the first trial is admissible at retrial. Moreover, all such evidence is preserved while appeals take place. Physical evidence is kept in the court record subject to strict rules for its handling. Testimony at the first trial is preserved in a trial transcript the court reporter prepares. The transcript then may be used by prior witnesses to refresh their recollections before testifying at retrial, as may prior statements to police, photographs and other available information.¹⁷⁵ If these aids still do not enable witnesses to remember their prior testimony, or if they cannot attend the retrial, their prior testimony and other recorded recollections are *themselves* used as evidence and read to the jury.¹⁷⁶ Thus, although important evidence can conceivably be lost between trials, it rarely happens.

Moreover, death verdicts typically are reversed only after defendants show (among other things) that a second jury will be better informed than the first. Most reversals—*e.g.*, for incompetent lawyering, police suppression of exculpatory evidence, court rulings excluding such evidence and jury instructions improperly telling jurors to ignore it¹⁷⁷—occur only after defendants prove that the jury at the first trial was illegally denied available evidence that would likely lead to

a different outcome at a proper trial.¹⁷⁸ In addition, retrial investigators often find entirely new evidence, further enhancing the information available to retrial, as opposed to original, juries.

Finally, there is reason to conclude that agreements to impose non-capital verdicts or to acquit the defendant that prosecutors sometimes reach at retrials are reliable indicators of how the first trial would have ended, but for the errors that occurred there. Nationwide, prosecutors settle more than 85% of all criminal cases, relying on their expert analysis of the facts and law, their experience identifying proper outcomes, and their commitment—enforced by frequent elections—to the imposition of stiff sentences for dangerous criminals. Prosecutors who originally put someone on death row, and the victims they consult, are unlikely to agree to a lesser sentence or release, therefore, unless their now better informed and more deliberate judgment justifies that outcome. Below we report evidence that reversible error is related to pressures to impose death verdicts in marginal cases where the evidence of guilt or facts calling for a death sentence are weak.¹⁷⁹ This tendency may be especially strong when prosecutors make snap charging decisions absent full investigation in response to outrage at a serious crime or doubts about the ability of existing law enforcement strategies to solve the crime and prevent its recurrence.¹⁸⁰ These kinds of pressures on prosecutors at the original trial may lead to precipitous refusals to agree to outcomes less than death that turn out on retrial, following cooler and better informed reflection, to be justified. Thus, **pressures that sometimes arise at initial capital trials may trigger over-charging and error, whereas the better informed and more reasoned decisions that are possible at retrials may be more conducive to accurate outcomes.**

A second question is posed by our data on retrial outcomes following state post-conviction reversals: What, if anything, do these outcomes following reversals at the second review stage suggest about retrial outcomes following reversals at the first review stage (direct appeal) and the third review

stage (federal habeas)? Absent data on outcomes following first- and third-stage reversals, what we can say is that there is **no evident reason to expect that retrial outcomes following those reversals are dramatically different from the retrial outcomes on which we have data.**

We begin with a comparison of the first, state direct appeal, and the second, state post-conviction, review stages. State practices vary, but there often are claims that may be raised at the first review stage that cannot be raised at the second stage, and vice versa. This could mean that errors leading to reversal at the second stage are more serious than errors triggering reversal at the first stage, which could suggest in turn that first-stage reversals are less likely than second-stage reversals to prompt a lesser outcome on retrial. Four factors tend to disprove this hypothesis:

- Because direct appeal comes first, that review process is likely to catch the most glaring errors—and thus many of the most serious mistakes.
- State high court judges who oversee state post-conviction review are the same judges who review verdicts on state direct appeal. Use of the same judges with the same dispositions about how serious error must be to warrant reversal suggests similarity, not difference, in the seriousness of error prompting reversal at the two stages.
- Certain errors of a non-constitutional nature are bases for direct appeal reversal but not for state post-conviction reversal. Nearly all such error, however, is subject to the same harmless error rule as applies on state post-conviction review. Because the same judges apply the same ultimate decision rule to the same evidence of guilt at the two stages, there is little reason to suppose that trivial errors are substantially more likely to lead to reversal on direct review than on state post-conviction review.
- Certain serious constitutional errors may be raised on state post-conviction review but not on direct appeal, such as incompetent counsel. But other serious constitutional errors may be cured on direct appeal, but not generally on state post-conviction review, including the most serious error of all—that there was not enough evidence to convict the defendant.¹⁸¹ Because this latter error automatically leads to post-reversal acquittal, and because no category of post-conviction reversals does so, it is likely that **more direct appeal reversals lead to retrial acquittals than do post-conviction reversals.** Claims of insufficient evidence to allow a death sentence also are usually adjudicated on direct appeal, not state post-conviction, and they too—unlike any kind of claim available on state post-conviction—automatically require a non-capital sentence on retrial.

- Overall, the allocation of serious constitutional claims between direct appeal and state post-conviction review is sufficiently balanced that their retrial outcomes should not differ greatly.

Retrial outcomes following federal habeas reversals are also unlikely to differ much from those following state post-conviction reversals. The main reasons for reversals at the two stages are similar,¹⁸² as are the procedures and rules that apply and the lawyers who litigate there.

Available data support the judgment that retrial outcomes do not greatly differ depending on the review stage where reversals occur. John Shiffman recently examined the aftermath of all death row reversals ordered by Tennessee direct appeal, state post-conviction and federal habeas courts between 1980 and mid-2001.¹⁸³ Of the 76 retrials ordered during that period, 58 had reached a verdict or other disposition by the time Shiffman completed his study. Of those, **89% (51) resulted in an outcome less than death—which dovetails with our finding, based on retrial outcomes following reversals at the state post-conviction stage, that 82% ended in sentences less than death.** Although retrials following state post-conviction reversals were somewhat more likely to result in lesser outcomes than retrials following direct appeal and federal habeas reversals, Shiffman’s central finding is that **rates of changed verdicts were remarkably high at all three stages.**¹⁸⁴

	Direct Appeal	State Post-Conviction	Federal Habeas
Outcomes Less than Death / Completed Retrials (Percent)	24/29 (83%)	24/25 (96%)	3/4 (75%)

All available data suggest that **a large majority of capital retrials end in a non-capital sentence or acquittal. This would not be true if courts often reversed capital verdicts for technical reasons with no effect on the accuracy of the verdict. Instead, it supports the same conclusion as the cautious predilections of most reversing judges, and the seriousness of their reasons for reversing: Reversible error is error that tends to produce unreliable outcomes.**

4. The stability of high rates of reversible error across states, courts and time shows that flaws in death verdicts are serious, chronic and continuing.

Below, we study variations in the amount of reversible error in death verdicts across states, counties and time to help identify factors that lead error to occur. But before focusing on what *varies* across place and time, it helps to see what is constant across those dimensions. Although the Rocky Mountain peaks crossing central and western Colorado from north to south vary greatly from 9,000 to 14,000 feet, and although higher and lower peaks bunch together in places, their chief attribute is not variation, but constancy. They are all high. If one is looking for temperate winters, easy passage or long stretches of sandy beach, the same answer applies: You're in the wrong place.

The same is true for error rates in death verdicts nationwide, from 1973 to 1995, as the Figures discussed in this section reveal. Despite significant differences *at the top of the range*, rates of error in capital verdicts almost always are high, no matter *when* the death verdicts were imposed during the 23-year study period, *where* they were imposed among the 34 states studied, and *which* court system—state or federal, trial or appellate—reviewed them. **High error rates are not the problem of a few atypical jurisdictions or periods with unusually flawed verdicts or unduly meticulous judges. They are the *norm*—the solid and unrelieved massif underneath the irregular peaks and valleys; a feature of the modern death penalty terrain so chronic and disturbing that it consistently commands the attention and unfavorable decisions of reviewing judges.**

a. Consistency across states.

Figure 1A, p. 50 below, compares overall rates of reversible error found in the 26 states with death verdicts finally reviewed at all three review stages from 1973 to 1995.¹⁸⁵ The horizontal line indicates the national overall error rate of 68%. As Figure 1A reveals, **24 of the 26 states with**

verdicts reviewed at all three inspection stages during the 23-year study period had overall error rates *over 50%*. In the vast majority of these leading capital states, that is, death verdicts were more likely to fail than pass inspection for reversible error.

Figure 1B extends this analysis to all 34 states in which at least one death verdict was fully reviewed at one or more review stages from 1973 to 1995. It compares the state direct appeal reversal rates of six states that had death verdicts reviewed at only the first review stage,¹⁸⁶ to the overall direct appeal and federal habeas reversal rates for two states for which we have data on outcomes at only those two stages,¹⁸⁷ and compares all those rates to the overall direct appeal, state post-conviction and federal habeas reversal rates for the 26 states that had one or more death verdict reviewed at all three stages.¹⁸⁸ On this comparison, **85% of the states had overall reversal rates of 50% or higher.**

A wide variety of additional information on each of the 34 states with active death penalties during the study period is reported in the State Capital Punishment Report Cards in Appendix A.

Figure 1A. Combined Reversal Rate on State Direct Appeal, State Post-Conviction and Federal Habeas, 1973-95⁺

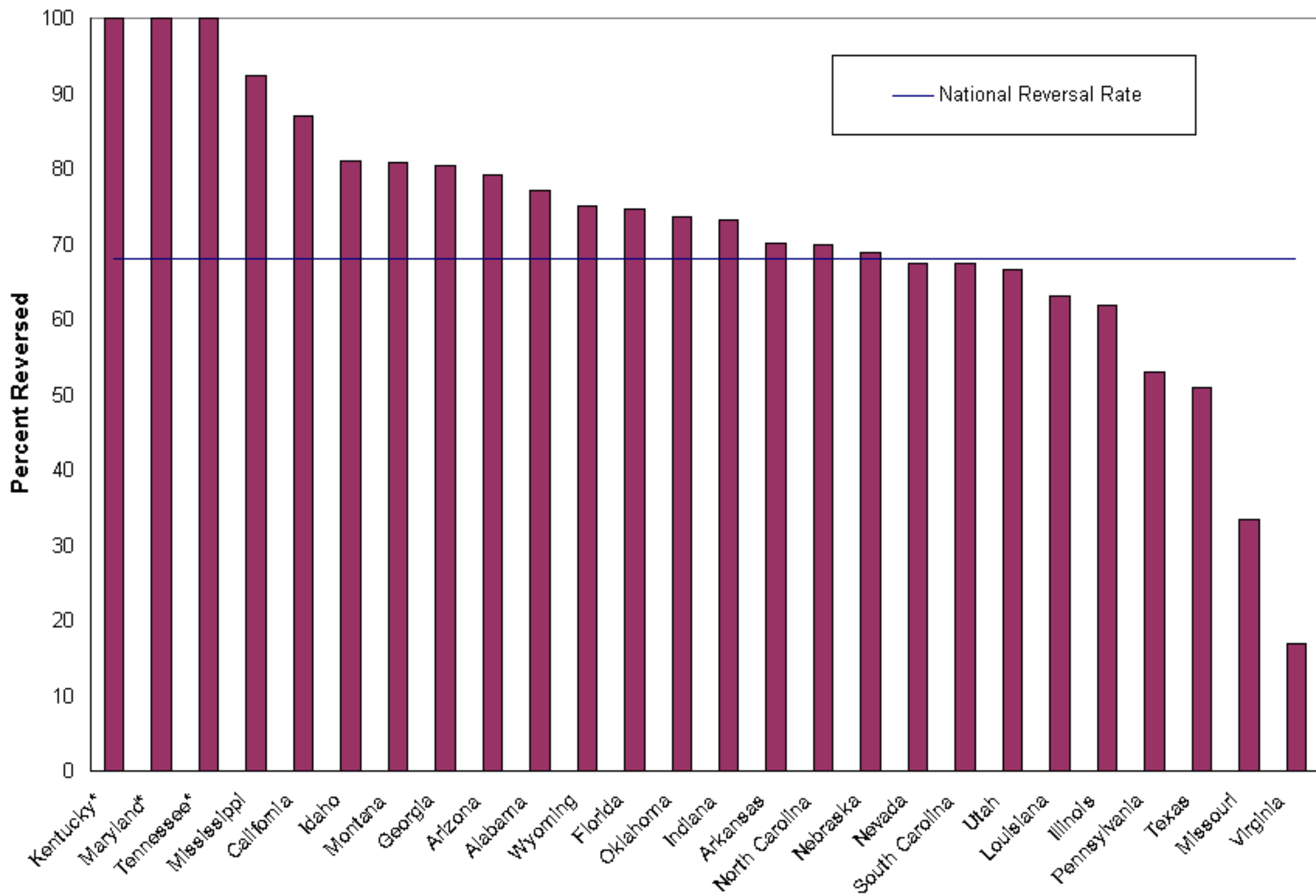


Figure 1B. Combined Reversal Rate for Completed Stages of Review, 1973-95⁺

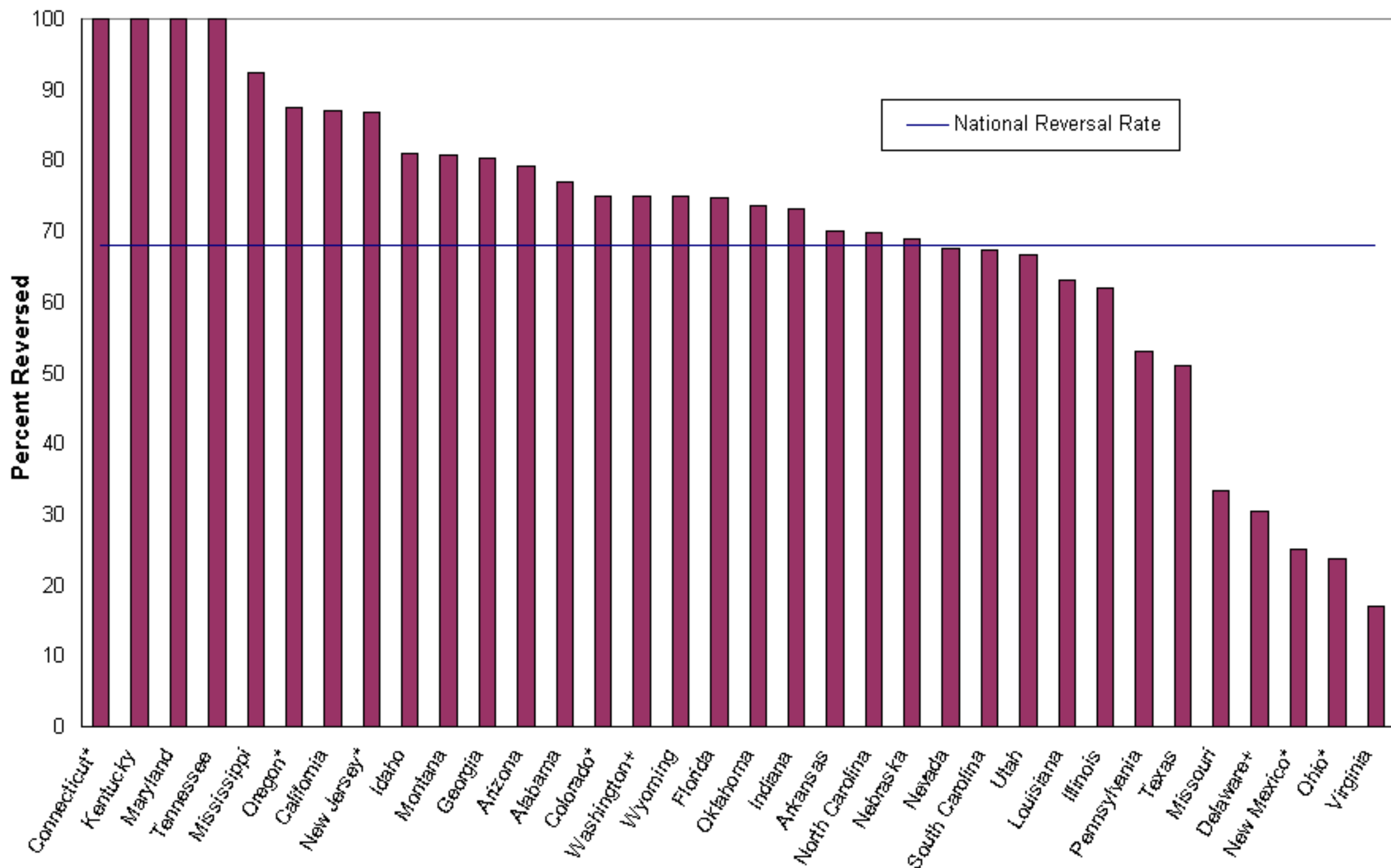


Figure 1B establishes the order in which states are displayed on most of our comparison graphs below. Because reversal rates are the condition being explained, ordering states based on their reversal rates enables graphs to give a rough and preliminary sense whether there is any relationship between states' reversal rates and the factor studied in the graph (*e.g.*, states' homicide rates).¹⁸⁹ That relationship is only roughly indicated in this manner, however, and is much more reliably tested by the regression analyses below for two reasons, among others:

- Some states have only a few cases reviewed at one or more stages (*e.g.*, Connecticut on direct appeal, Kentucky on federal habeas), while other states have many cases reviewed at all stages. Multiple regression analyses more reliably indicate the strength of relationships between reversal rates and other measured conditions than these graphs, because they give more weight to relationships appearing in states with more reviewed death verdicts than to relationships appearing in states with fewer reviewed verdicts. This is appropriate given the higher degree of confidence we can have in the existence of relationships that are based on many observations as opposed to those that seem to appear after only a few observations.
- Graphs displaying states in the order of their reversal rates and comparing states based on another factor (say, homicides rates) examine only the relationship between two factors: reversal rates and that particular factor. Often, though, two conditions that seem to be related when compared only to each other turn out not to be related when other factors are analyzed. Conversely, factors that do not seem to be related when compared to each other may turn out to be related after controlling for other factors. For this reason, as well, multiple regressions are more informative than the bivariate comparisons in these graphs.

b. Consistency over time.

i. 1973 to 1995. The regression studies below carefully analyze the effect of time on capital reversal rates after controlling for other explanatory factors.¹⁹⁰ Figures 2A-2D, pp. 55-58 below, are a cruder, more preliminary analysis of the effect of time which does not account for other factors.

Figure 2A, p. 55 below, shows the combined reversal rates for death verdicts finally reviewed on state direct appeal and federal habeas in each year from 1973 to 1995.¹⁹¹ Figure 2B, p. 56 below, shows the combined reversal rate on state direct appeal and federal habeas for all verdicts that were *imposed* in each year and were finally reviewed at those two review stages by the end of the study

period. Because it takes years for verdicts to move through each of the two state stages of review and then to be decided at the federal habeas stage, Figure 2B stops in 1989. No death verdict imposed after that year was finally reviewed on federal habeas as of the end of the study period, in 1995.

Both these measures of reversal rates over time are sensitive to two things: changes in how carefully reviewing judges scrutinize capital verdicts for error, and changes in the quality of capital verdicts imposed at trial. Analyses of reversal rates for each study year in which verdicts were finally reviewed are more sensitive to changes in how carefully judges review verdicts for error in different years.¹⁹² Changes in reversal rates for each study year in which verdicts were imposed are more responsive to changes in the quality of verdicts over time.¹⁹³ Here, we are interested in the stability of high rates of capital error, no matter how change over time is measured, so we examine reversal rates for both review year (Figure 2A) and sentence year (Figure 2B). The remainder of the Report focuses on factors related to changes in the quality of death verdicts, and for that reason studies changes in reversal rates based on the year death verdicts were imposed.

Figure 2A shows that reversal rates for verdicts reviewed at the state direct appeal and federal habeas stages in each study year fluctuated during the early years but stabilized around 1983. The combined reversal rate for each decision year thus hit peaks in 1973 and 1975 (100%) and in the early 1980s (86-100%) and a valley in 1976 (32%), but **remained at around 60% during most of the last 13 years. In all but one of the last 18 study years, the overall reversal rate for the direct appeal and habeas stages was greater than 50%.**

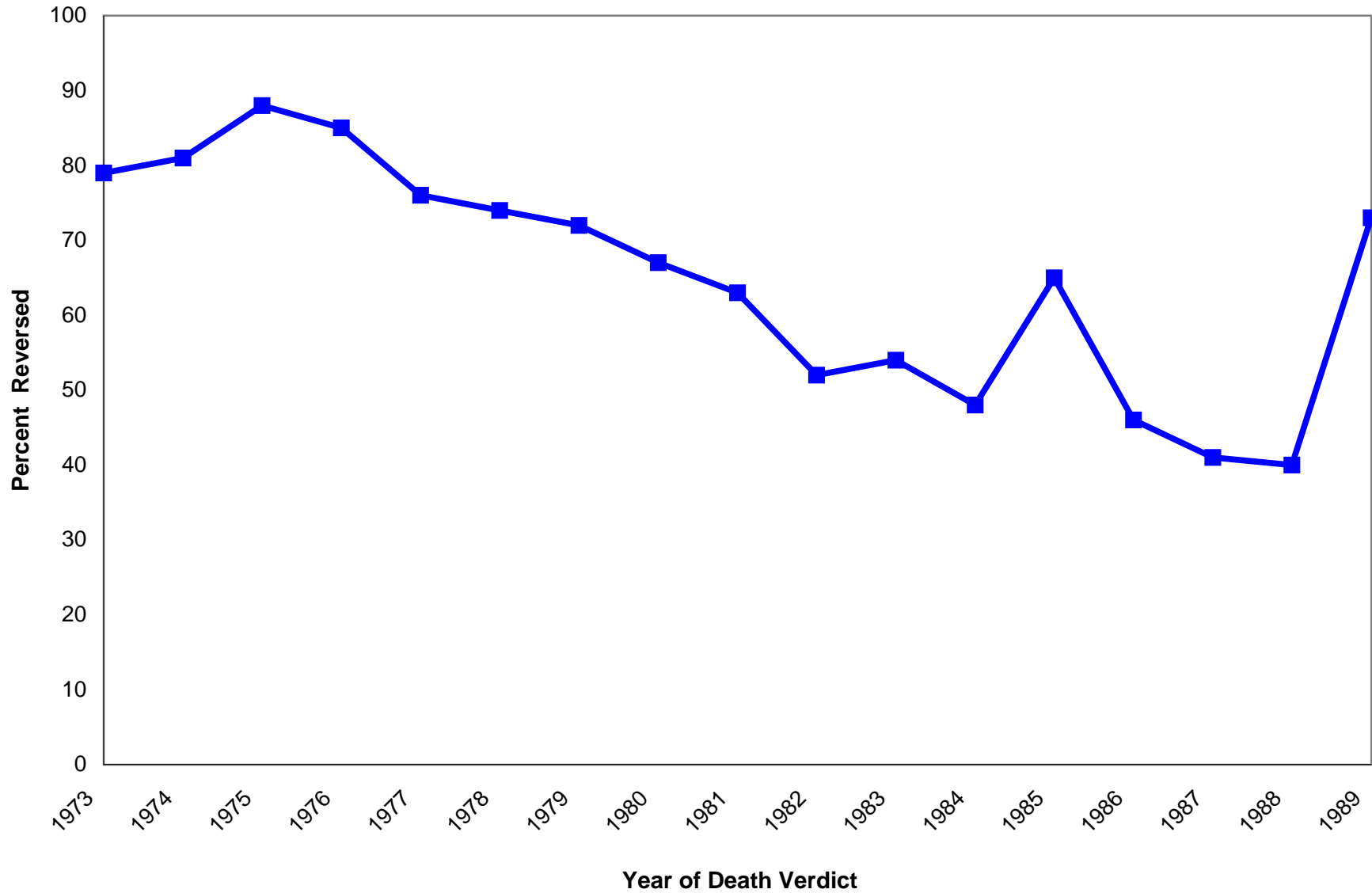
Figure 2B shows consistently high (63-88%) but somewhat declining overall reversal rates for verdicts imposed from 1973 through 1981. After that, reversal rates wobble between 40% (1988 verdicts) and 73% (1989) with an average rate of about 52% from 1982 until 1989.

Our regression analyses below generate important findings about the trend of reversible error over time at the state direct appeal stage after controlling for other factors. The direct appeal stage is particularly important because it accounts for 79% of the reversals during the study period.¹⁹⁴ To provide a benchmark for our later analyses, Figures 2C and 2D, pp. 57-58 below, display changes in direct appeal reversal rates over time *without* accounting for other factors. Figure 2C analyzes direct appeal reversals by the year in which they occurred. Figure 2D analyzes direct appeal reversals by the year in which the reviewed death verdicts were imposed, ending in 1993 because only a handful of verdicts imposed after that year were finally reviewed on direct appeal by the end of the study period. Both figures reveal a pattern similar to the one noted for overall reversal rates—volatility toward the beginning of the period, with more stable levels of reversals starting in the early 1980s.

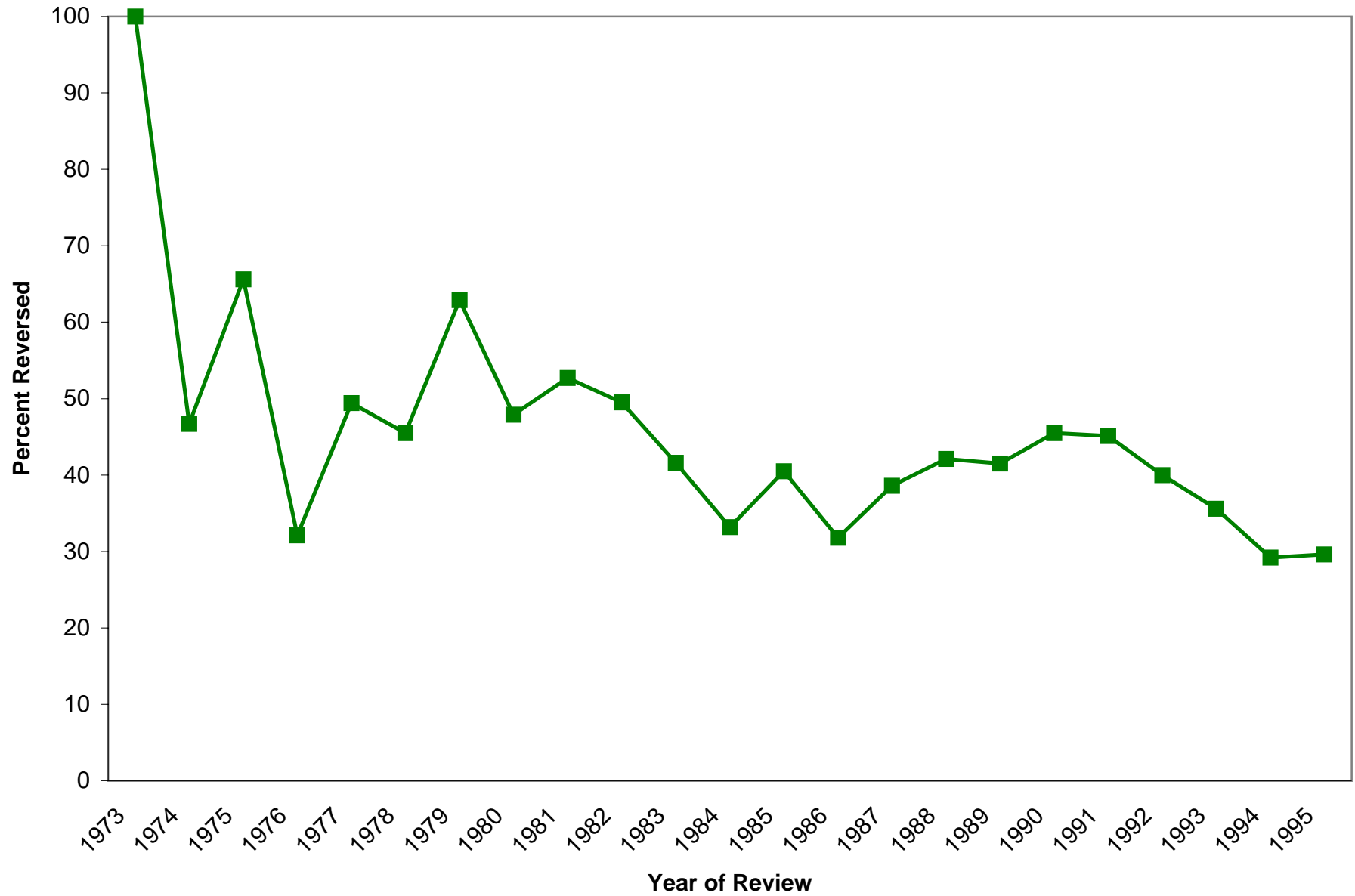
Figure 2A: Combined Reversal Rate for State Direct Appeal and Federal Habeas by Year of Review, 1973-95



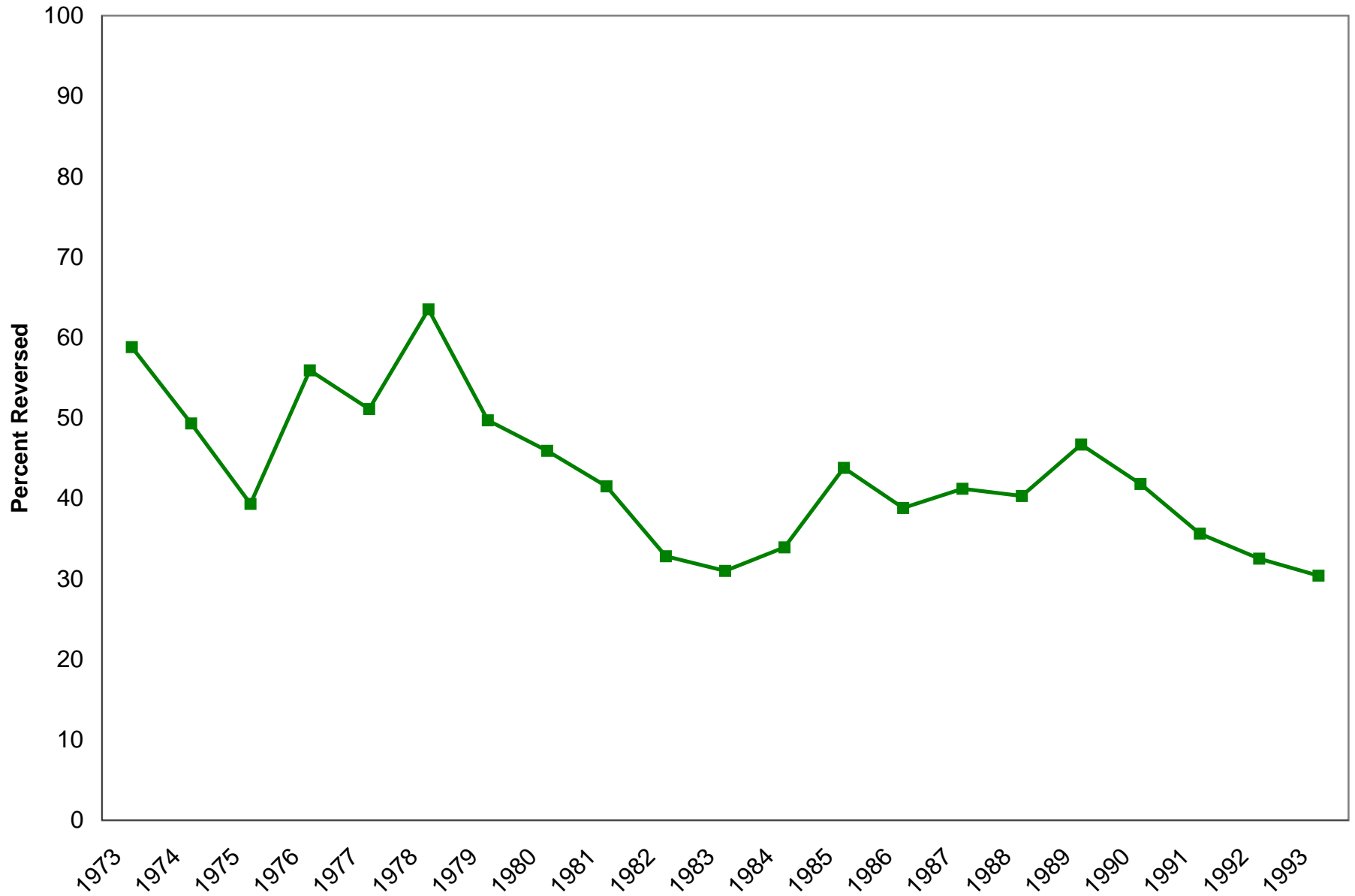
Figure 2B: Combined Reversal Rate for State Direct Appeal and Federal Habeas by Year of Death Verdict, 1973-89



**Figure 2C: Direct Appeal Reversal Rate
by Year of Review, 1973-1995**



**Figure 2D: Direct Appeal Reversal Rate
by Year of Death Verdict, 1973-1993**

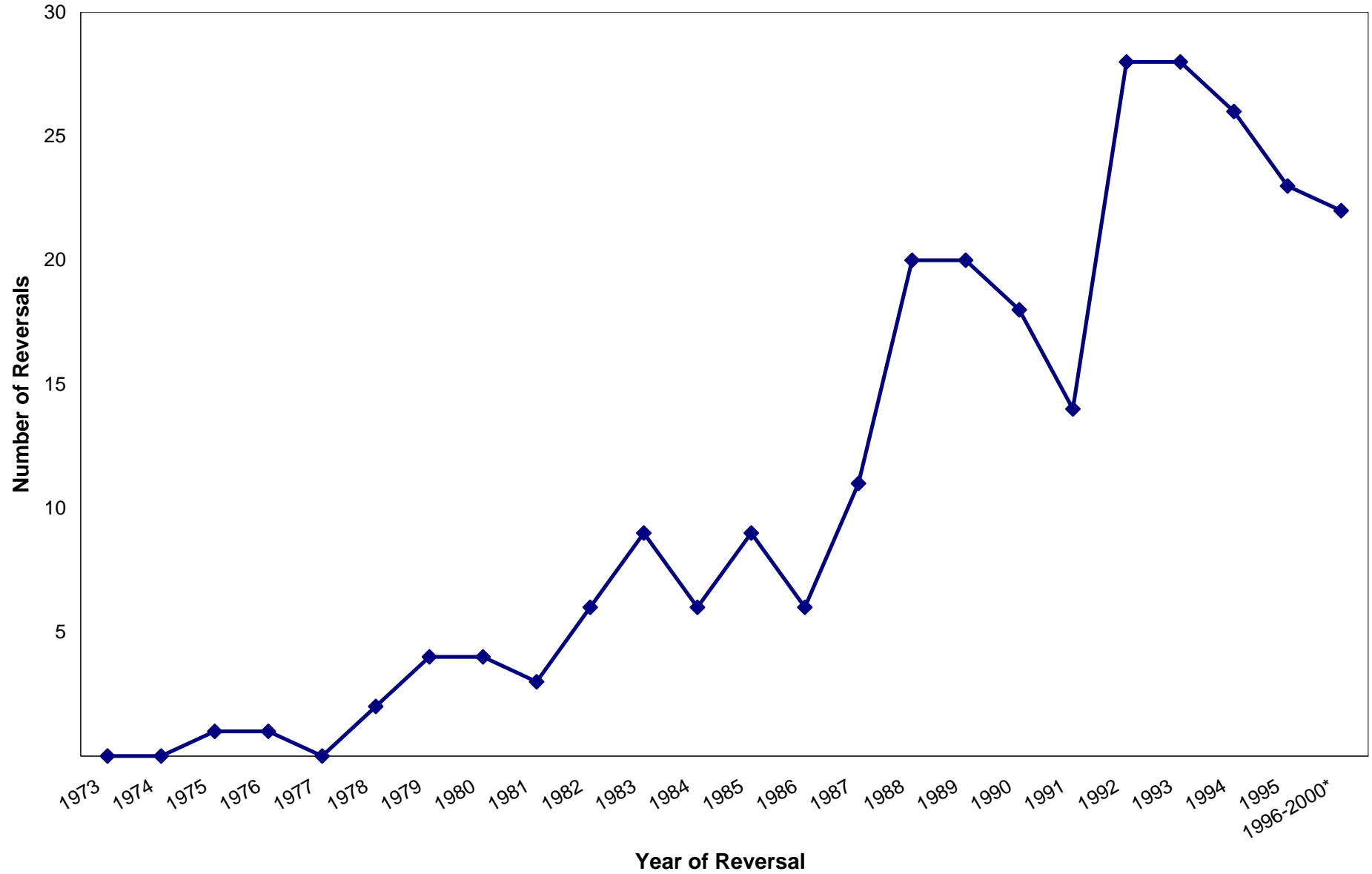


The reversal rates by decision and sentence year in Figures 2A-2D do not include state post-conviction error rates. As we note above, because state post-conviction decisions often are unpublished and difficult to find, we only collected information on state post-conviction reversals.¹⁹⁵ Although this makes the task of estimating state post-conviction error rates manageable,¹⁹⁶ it does not allow us to determine the trend of reversal rates over time for this middle stage of review. Figures 3A and 3B, pp. 60-61 below, are a rough effort to gauge the direction of that trend.

Figure 3A shows that **the number of state post-conviction reversals by year tripled as of 1987, then rose again in the early 1990s.** To provide a rough sense of whether the number of death *verdicts* also was increasing at the time—which could explain a rise in the number of reversals, without indicating a rise in reversal *rates*—Figure 3B indicates the number of death verdicts imposed each year in the study period. Because state post-conviction review generally occurs between four and seven years after a death verdict is imposed, it is possible to project forward from increases in death verdicts to increases in post-conviction review. Figure 3B shows that the number of death verdicts imposed nationally increased substantially from 1977 through 1982 (the spike in 1975 and 1976 was caused by mandatory death verdicts that the Supreme Court wiped off the books in the latter year,¹⁹⁷ keeping them from reaching state post-conviction review), but held fairly steady after 1982. Together, Figures 3A and 3B suggest that an increase in the number of verdicts under review may explain the first increase in state post-conviction reversals beginning in 1988, but cannot explain the second increase after 1992. **State post-conviction reversals rates thus appear to have risen starting in the early 1990s, at a time when reversal rates at the other two stages were holding steady.**

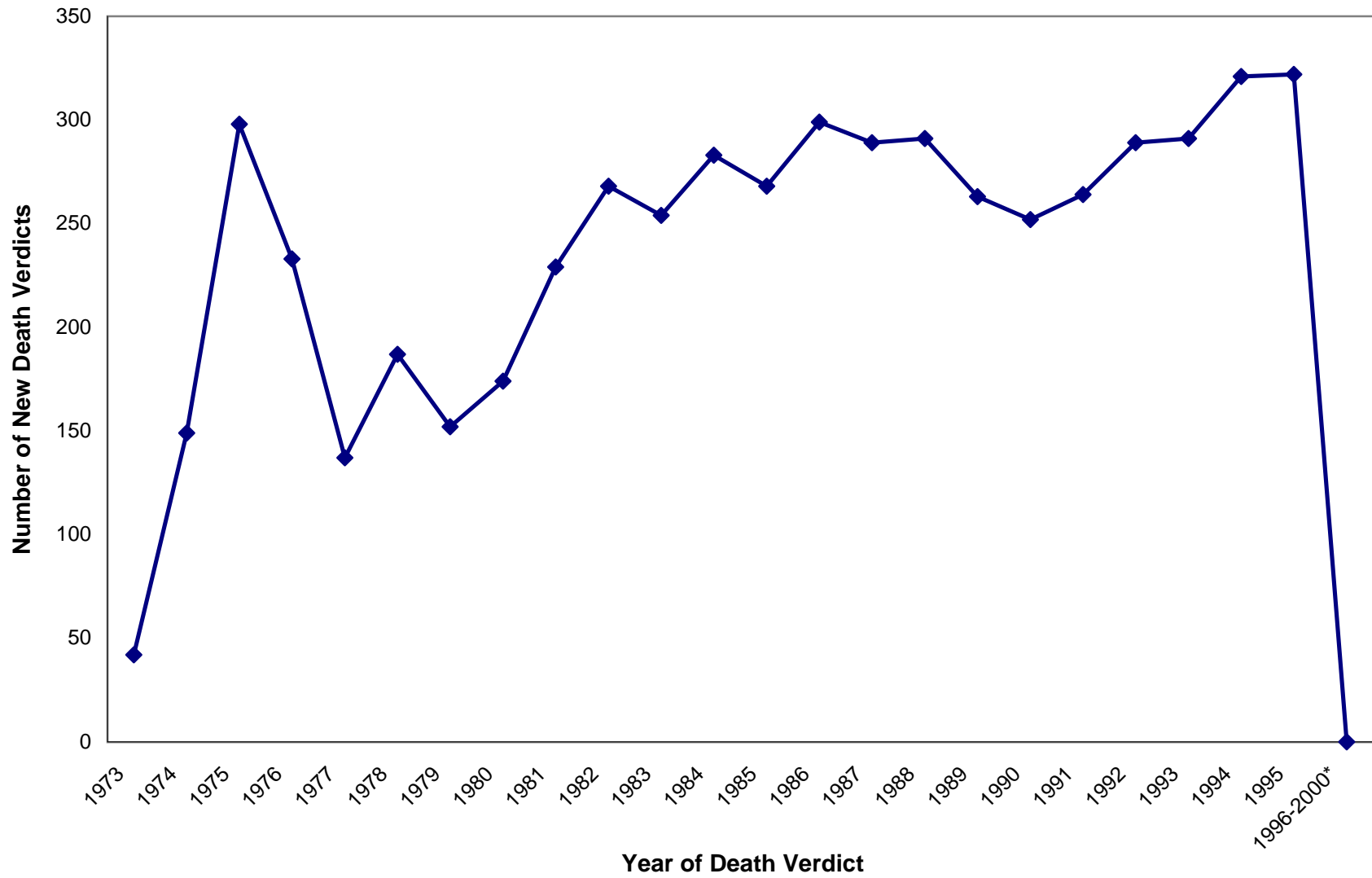
Overall the chronological picture is the same as the geographic one: **High error rates have plagued the American death penalty system for years.**

**Figure 3A. Known State Post-Conviction Reversals
by Year, 1973-2000**



* Average for 5 year period.

Figure 3B. New Death Verdicts by Year, 1973-2000



* Average for 5 year period.

ii. 1996-2001. Because many state post-conviction decisions are unpublished, data on them must be collected by direct contact with informed persons in each capital state. Since we already were making those contacts to enable us to count state post-conviction reversals during the study period, we decided to ask as well about state post-conviction outcomes in the 1996-2000 period.¹⁹⁸ As Figures 3A and 3B above reveal, the number and, apparently, the rates of state post-conviction reversals remained at about the average for the 1990s as a whole during the last half of that decade.

We do not have the resources required to collect information on the larger number of decisions approving and reversing death verdicts at the state direct appeal and federal habeas stages since 1995. What information is available suggests that reversal rates may have climbed some or significantly in Florida (state direct appeal reversals), Illinois (direct appeal), Kansas, Kentucky, Nevada, Pennsylvania, Tennessee (direct appeal), Virginia and Washington; held steady in California; dropped slightly (from 77%) in Alabama; and dropped significantly in Louisiana (direct appeal), North Carolina (direct appeal) and Ohio (direct appeal).¹⁹⁹ The patterns elsewhere are unknown.

Whether high rates of serious error in capital verdicts during the 23-year study period are a guide to reversals in following years depends on answers to two questions: First, is there reason to think the quality of death verdicts has changed much recently? If systematic steps were taken to improve or undermine the quality of capital trials recently, one might expect a corresponding decrease or increase in overall reversal rates. Absent systematic changes, one might expect error rates to remain near the 50% to 60% plateaus prevailing in the second half of the study period. Second, is there reason to think that the care with which state and federal courts scrutinize death verdicts for serious error has changed substantially during that period? Here we ask about 1996-2001 the same question asked above about 1973-1995: Do court reversals validly reflect the amount of serious error? For the

23-year study period, we have found that court reversals are a conservative measure of serious capital error.²⁰⁰ If the review process has become *more* careful since then, the correspondence of reversals to serious error might be something closer to one-to-one, as opposed to the current situation where there probably are more serious errors than reversals. If, on the other hand, the review process has become *less* careful recently, there would be reason to worry that, instead of conservatively indicating how often serious error occurs in capital trials, the number of reversals would provide an inaccurately optimistic picture of the system's reliability.

Although the 2001 legislative cycle may signal a different trend,²⁰¹ there were only two major nationwide changes in imposing and reviewing death verdicts in the latter half of the 1990s:

- the 1995 de-funding of resource centers that, from the mid-1980s until then, had provided assistance to capital trial lawyers in most capital states, and had themselves represented death row inmates on appeal and in state and federal post-trial review;²⁰² and
- the adoption of major pieces of state legislation (*e.g.*, Texas 1995) and federal legislation (1996) imposing strict time limits on state post-conviction and federal habeas review and creating numerous technical barriers to reversal because of waiver and default, non-retroactivity of rights, harmless error, non-exhaustion of remedies, “reasonable error,” and successive assertions of rights—even where capital verdicts were demonstrably the result of serious legal violations that substantially undermined their reliability.²⁰³

Starting in 1995, the first of these changes immediately increased the probability that lawyers appointed for capital defendants lack the expertise and resources needed to try death cases competently.²⁰⁴ *Both* changes increased the probability that errors in verdicts imposed before and after 1995 but reviewed after then will not be corrected.²⁰⁵ The changes' combined effect is just beginning to be felt, because flawed verdicts imposed after the withdrawal of defense resources in 1995 are only now reaching the state and federal post-conviction stages that were truncated in that and the next year.

Given all these events, it is reasonable to infer that **rates of error seriously undermining the reliability of capital verdicts have not decreased since 1995, and may well have increased,**

though the rates at which serious error is being *detected* may have declined in the federal courts and in some state courts, while rising in other state courts including those noted just above.²⁰⁶

A more complete answer to the 1996-2001 question awaits the collection of additional data.

c. Consistency among state and federal court systems.

i. The general rule: similar state and federal court reversal rates. Figure 4, p. 66 below, compares the rates of reversible error discovered by state courts on direct and post-conviction review and by federal courts on habeas review in the 26 states where we have data on verdicts fully reviewed at all three review stages during the study period. The states are arrayed to reveal those where the rates of reversible error discovered by state courts (the grey line) and federal courts (the black line) are similar or different. In the middle range of the graph are states where state and federal reversal rates are similar; towards the left and right margins are states with successively larger differences between the two rates. In states on the left side, state court reversal rates exceed federal court rates; in states on the right side, federal court reversal rates exceed state court rates. Figure 4 shows that:

- Differences *between* states in regard to their overall—state plus federal—reversal rates are greater than differences *within* states, comparing their reversal rates in state vs. federal court: **If a state’s verdicts tend to be reversed relatively more often by state courts, they also tend to be reversed relatively more often by federal courts. Even the actual rates of state and federal court reversals are similar.** This is especially so if one removes Kentucky and Tennessee (far right margin), which had only one death verdict reviewed on federal habeas during the study period—not enough to give a good picture of federal reversal rates—and when one notes that both North and South Carolina (far left margin) are in the Fourth federal judicial Circuit which has very low capital reversal rates compared to all other circuits.²⁰⁷
- Figure 4 distinguishes (1) states where state and federal courts roughly agree on the amounts of error in capital verdicts (in absolute terms, and relative to the amounts of error other courts find in verdicts from other states) from (2) states where there the state and federal courts disagree. Notably, states where local and federal courts roughly agree outnumber states where the two sets of courts do not see eye to eye. This, again, suggests that in general **it is not the different dispositions of different types of judges that determine whether they find reversible error. Instead, it is the presence of clear and serious error that drives judges—state and federal, elected and life-tenured—to reverse death verdicts.**²⁰⁸

ii. *A few exceptions: federal court compensation for lax state court review, and vice versa; California and Georgia vs. North and South Carolina.* Even where state and federal courts differ in the amounts of error they find in capital verdicts, they do not differ in ways that are predictable based on judges' status: **State courts reverse higher proportions of their own verdicts than federal courts in 11 states; federal courts reverse higher proportions than state courts in 13 states; reversal rates are identical in 2 states. Considering all verdicts nationally, state court reversal rates are 46%, compared to 40% for federal courts.** This, and Figure 5, p. 68 below, suggest that disagreement in amounts of error detected is not a function of the different proclivities of state as opposed to federal judges and, instead, that inter-court compensation may be occurring:

- In reviewing verdicts from some states, **federal courts may find it necessary to reverse at relatively higher rates because the courts of that state are unusually tolerant of error.** This may be the case in, *e.g.*, **California and Georgia.** As Figure 5 reveals, both states have especially high federal court reversal rates and atypically low state court reversal rates.
- Figure 5 suggests the reverse pattern in **North and South Carolina.** There, **state courts may exercise more care because they know that death verdicts receive atypically forgiving review in the relevant federal court (the Fourth Circuit).**

This, again, suggests the stability and seriousness of error in capital verdicts: **If one set of courts (state or federal) is unusually willing to let error pass—*e.g.*, California state courts, where harsh political discipline has been exacted against judges thought to have reversed too many death verdicts²⁰⁹—the seriousness of error slipping through that system's review process may pressure other courts to redouble their efforts to catch error—as federal judges report having to do when reviewing California death verdicts.²¹⁰**

Figure 4. Percent of Capital Verdicts Reversed on State Direct Appeal or State Post-Conviction and on Federal Habeas, 1973-95⁺

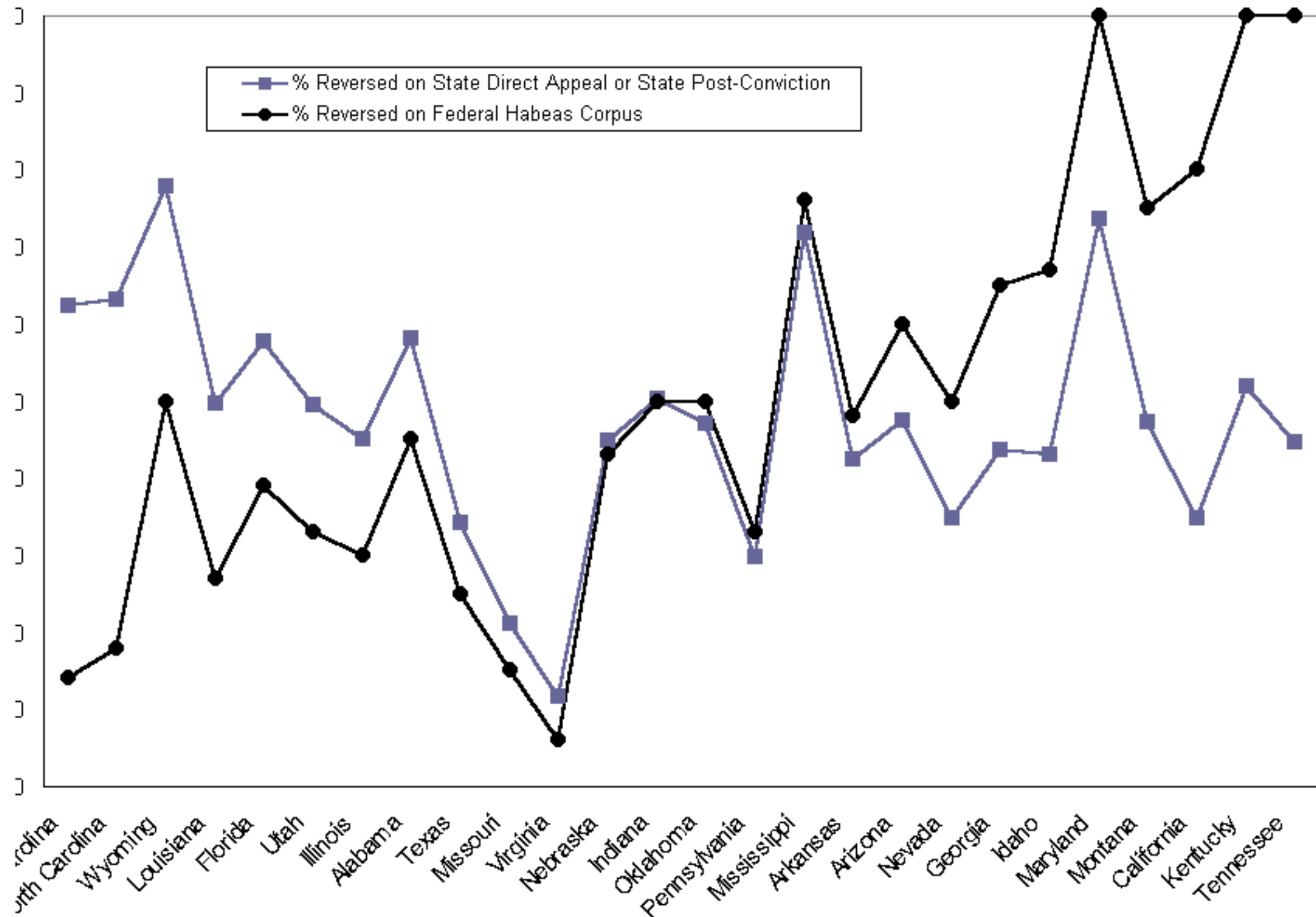
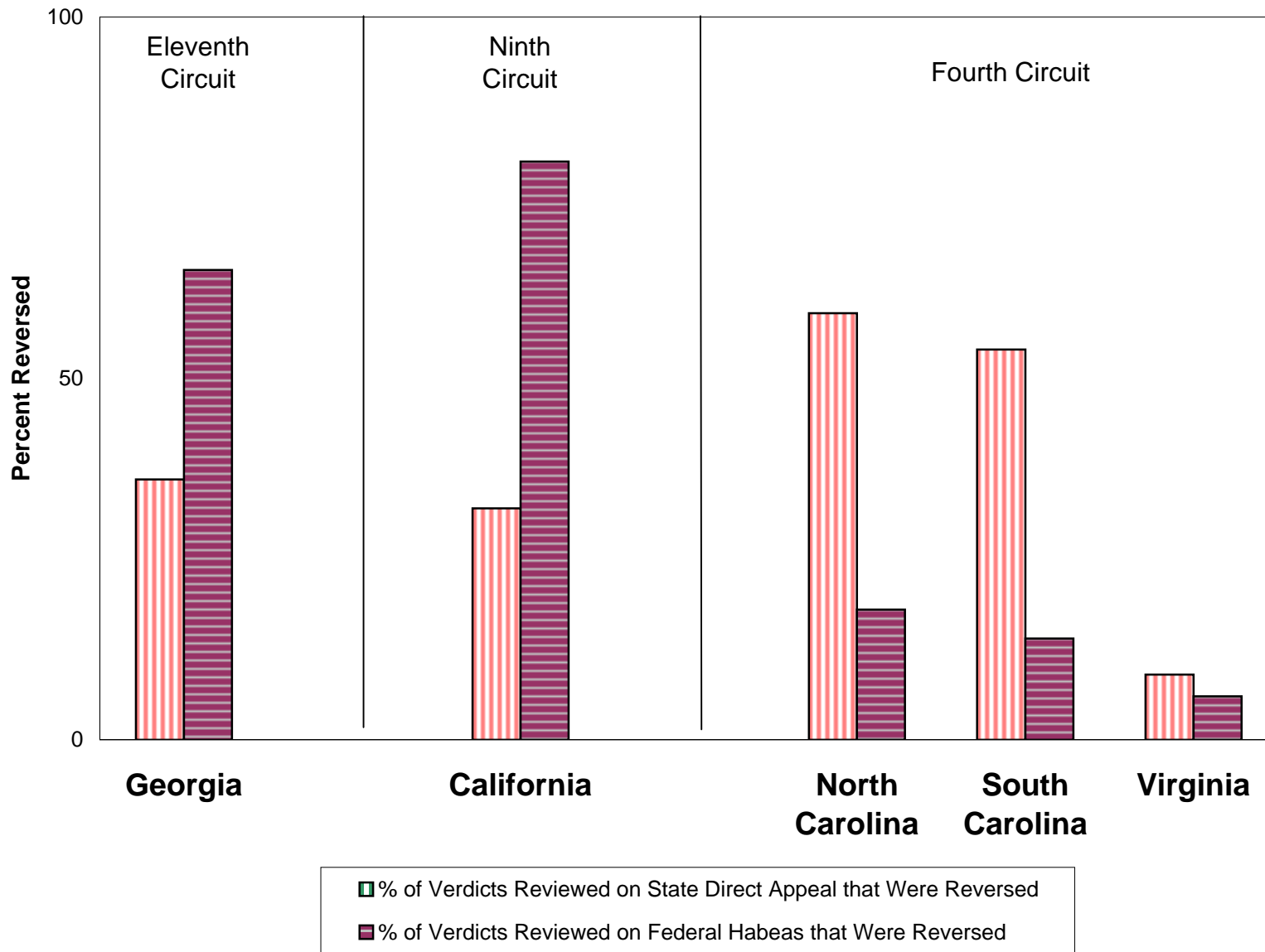


Figure 5. Percent of Death Verdicts Reversed by Selected State Direct Appeal and Federal Habeas Courts, 1973-95



iii.. *Virginia: low state and federal reversal rates.* As Figure 5 illustrates, Virginia has the unique confluence of state *and* federal reviewing courts with the lowest rates of error detection in capital cases among state and federal courts nationally. Together, those courts generate an overall error rate for Virginia (18%) more than two standard deviations below the mean for the other states in Figure 1A above: **Less error is detected in Virginia capital verdicts than the experience of other states suggests should be the case.** This could mean there are disturbingly low rates of error *detection* in Virginia capital cases—given the confluence of state and federal court systems with high tolerances for serious error and no desire to compensate for the other system’s lax review.²¹¹ This explanation is suggested by comparing Virginia to the three neighboring states in the same (Fourth) federal judicial circuit. In Maryland, North Carolina and South Carolina, atypically low federal court reversal rates are compensated for by unusually high state court reversal rates.²¹² Alternatively, Virginia death verdicts may have commendably low rates of error—the likes of which other states have not come near achieving given Virginia’s unique traits. We come back to Virginia below.²¹³ Here, we simply note two warning signals from emergency systems that catch some errors missed in the normal course of state and federal court review:

- The U.S. Supreme Court can but rarely does grant review of death verdicts after state or federal circuit court review. From 1995 to 2001, it did so in fewer than 25 cases. But a substantial majority of those cases were from only 1 of 50 states—Virginia—or 1 of the 12 federal judicial circuits—the Fourth, including Virginia—or (usually) from *both*. The reasons for this disparity are hard to know (the Court doesn’t explain decisions to grant or deny review), but our study suggests one reason: **In the unique situation of Virginia, where *both* the state and the federal courts are unusually disposed to let capital error pass—and where neither has beefed up its review to compensate for the other’s review—it has fallen to the court of last resort to provide the inspection function that elsewhere is performed by state courts, lower federal courts, or both.**²¹⁴
- During the last decade, Virginia Governors have spared two inmates from execution under suspect convictions that state courts on direct and post-conviction review and the courts on habeas review had declined to cure through the regular judicial channels.²¹⁵ This experience

(which is duplicated in no other state) also suggests a break-down in the court system for containing the effects of serious capital error, and a need to rely on fall-back systems that are not designed to provide routine protection against unreliable death verdicts.

iv. The high risk that courts miss some serious error. High reversal rates at the last as well as the first review stages reveals a substantial risk that appellate courts cannot catch all serious errors that compromise capital verdicts. Multiple inspections are designed to increase confidence in the outcome, not only by sending flawed products back to be reworked or scrapped, but also by showing that prior inspections have done their job. To serve the latter goal, however, the amount of error found at successive review stages must *drop* substantially, with the last stage finding no or only infrequent errors. By this measure, **the capital inspection system is a not working properly:**

- **The 41% — 10% — 40% pattern of reversal rates at successive stages of review does not reveal the downward trend needed to create confidence that prior review stages catch most serious errors.** Indeed, our regression analyses identify types of error that the first two review stages do a poor job of catching, leaving it to the last stage to fix.²¹⁶
- **The 40% rate of serious error found at the last review stage undermines confidence that no or few errors will pass that final checkpoint and go on to cause serious harm.** This is especially so given evidence in our regression study that federal reviewers have blind spots of their own.²¹⁷

The 3-tier review process reveals the size of the problem of serious capital error. But that process does not solve the problem. Instead, it leaves a high risk that some seriously flawed verdicts are approved for execution and carried out. The problem may be especially acute in states like California, Georgia, North Carolina and South Carolina where there is evidence that one review stage is having to compensate for the weakness of other review stages, and could be most worrisome in Virginia, given the potential laxness of both state and federal review.

D. A System that Does Not Work, and Is Fraught with Costly and Serious Error

Throughout the 23-year study period—and evidently since—serious error has not been just an attribute of the death penalty system. It has been the system’s defining trait:

- **Reversible error—the only error counted in our study—is serious error.** By legal definition, reversible error affects the reliability of the outcome. In actual fact, where the facts are known, reversible error produces capital outcomes that do not hold up on retrial in over 4 cases out of 5, and are replaced by *acquittals* nearly 1 time in 10.²¹⁸
- **But not all serious error is reversible error, so that by counting only reversible error, we under-count serious error.** Courts often miss error, or spot it but ignore it as harmless, not prejudicial or waived—even when doing so approves innocent people to be executed.²¹⁹
- Even though we do not count all error, but only reversible error, and even given that our judicious methods substantially *understate* the amount of such error, **state and federal courts still found reversible error in just under seven-tenths of all death verdicts imposed and reviewed in the 23-year study period.**²²⁰
- **Reversible capital error is chronic across place and time.** All but two states with at least one case that progressed through the entire review process had rates of reversible error greater than 50%. And error rates higher than 50% were discovered in death verdicts reviewed in all but two of the 23 study years.²²¹
- **High reversal rates persist from the first to the last review stage, creating a high risk that the review process cannot catch all serious error that infects death verdicts.**²²²
- **This risk is a reality: Not only have trial courts repeatedly sentenced innocent capital defendants to die, but all three stages of reviewing courts have repeatedly approved innocent capital defendants for execution.**²²³ Still more defendants have been approved for execution though the law bars death as a penalty for their crimes.

Capital trials make too many serious errors, and capital appeals miss too many of those mistakes, to satisfy any reasonable definition of a system that works. The system is broken.

Typically, **capital error is reversible only because it is serious**, undermining the reliability of death verdicts. In addition, **error is serious because it causes reversal**. When, as has been true for decades, reversal is the rule, not the exception, it devastates the system. The most obvious way

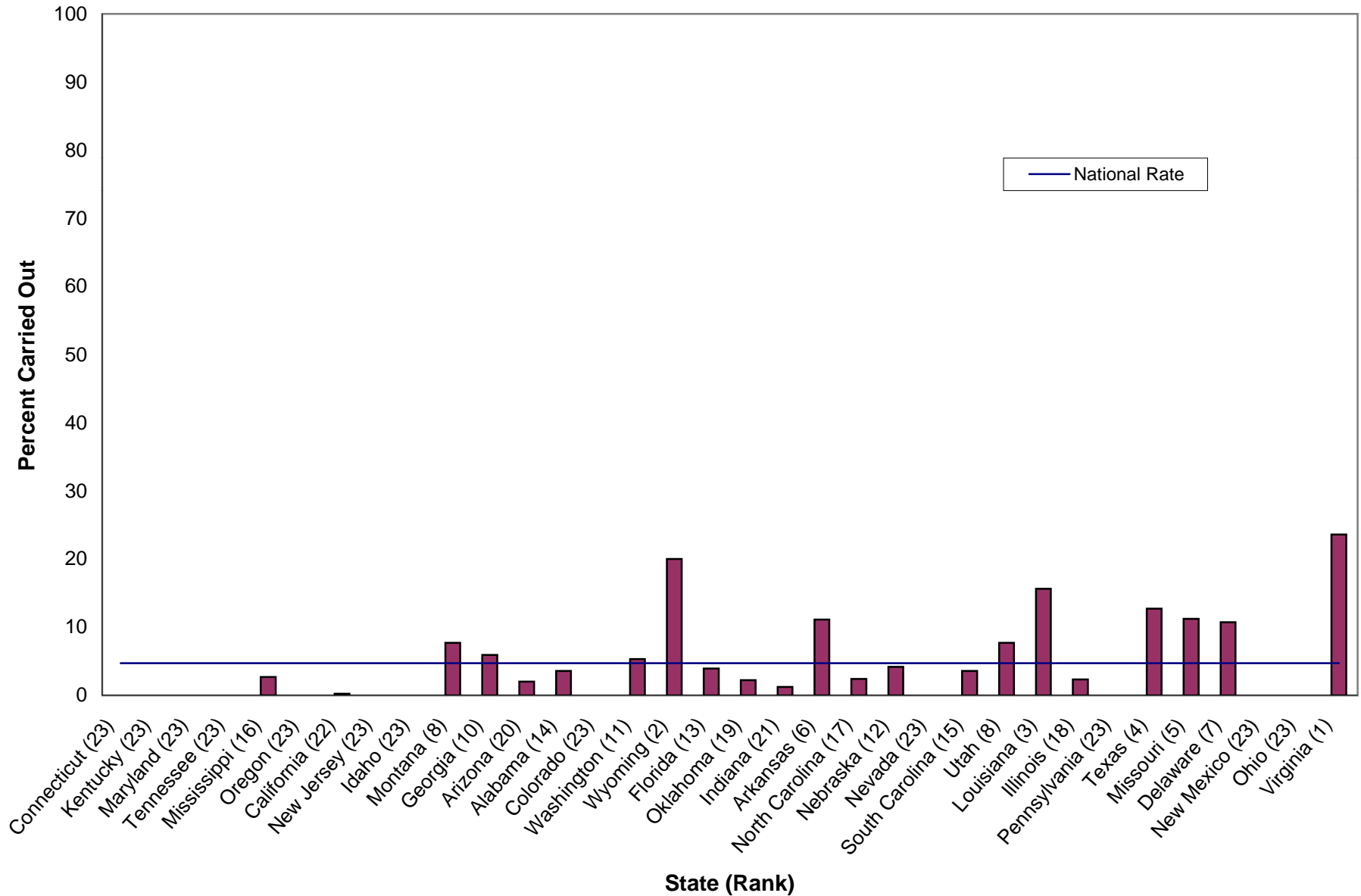
citizens and taxpayers measure the success of the death penalty that they for the most part support and pay dearly to operate²²⁴ is to ask how often imposed death sentences are carried out. As a result of chronic error, that number is lower than most citizens think, and lower than we have suggested so far:

- Of death sentences fully reviewed during the 23-year study period, 68% were overturned and sent back for a retrial—meaning **only 32% of the death verdicts fully reviewed during the period were found fit to be carried out.**
- **But nothing like 32% of the nearly 6000 death verdicts imposed during the period were carried out.**
 - The 32% applies only to death verdicts fully reviewed in the study period. But the arduous review process required by high error rates—and the stifling review burden on those courts—means that **cases proceed through the courts slowly: over 9 years on average from death sentence to execution during the entire study period; just under 11 years on average in the latter half of that period; and around 12 years for executions occurring in the last few years.** At any given time, therefore, most death verdicts are stuck in the review process, meaning **the 32% of fully reviewed verdicts approved for execution are a much smaller proportion of all verdicts.**
 - As we note above, we include in the 32% of so-called successful verdicts—ones approved for execution by all three levels of review—some that were subsequently never carried out because it was discovered by others that the prisoner was innocent.²²⁵ Thus, **a significant number of even the 32% of fully reviewed verdicts that our study counts as error-free are in fact seriously flawed and never carried out.**
- Thus, **32% is not a valid measure of the system’s success rate.** Instead, our data show that:
 - **Fewer than 8% of all death verdicts known to have entered the review process from 1973 to 1995—358 out of 4546—were approved for execution by all three sets of reviewing courts.** The rest were overturned or got stuck in the system.
 - **Only 6% of all death sentences imposed during that period—358 out of 5826—were approved for execution.** The rest either were overturned, mired in the review process or waiting to enter that process.
 - **Only 5% of all death verdicts imposed during the period—313 out of 5826—were carried out in the period.** Figure 6, p. 73 below, compares the states on this most basic measure of success: Success is in black, failure in white. Figure 6 speaks for itself.

- As Table 1 and Figures 7A-7C show (pp. 74-75 below), the result of a capital system that for decades sentenced between 250 and 325 people to die annually, that requires an elaborate and overburdened review process to catch its many mistakes, and that can make only a tiny fraction of those sentences stick, is **a huge death row population (now about 3700), and an ability to execute at most only 2.7% of those inmates in a given year—with an average of only 1.2% per year since 1981, and with only 1.8% of death row being executed in 2001.**

- **States with the death penalty executed about *two-thirds of one percent* of all their homicide offenders during the 23-year study period.**

**Figure 6. Percent Death Verdicts Carried Out
(Nonconsensual Executions), 1973-95***



* States are listed in descending order of overall reversal rates (see Figure 1B).

**Table 1: Death Row Population, Executions
and Percent of Death Row Executed, 1973-2001**

Year	Death Row Population	Executions	Non-Consensual Executions	% of Death Row Executed	% of Death Row Executed, Non-Consensual Executions
1973	376	0	0	0	0
1974	283	0	0	0	0
1975	542	0	0	0	0
1976	721	0	0	0	0
1977	557	1	0	0.18	0
1978	610	0	0	0	0
1979	635	2	1	0.31	0.16
1980	769	0	0	0	0
1981	926	1	0	0.11	0
1982	1131	2	1	0.18	0.09
1983	1327	5	5	0.38	0.38
1984	1499	21	21	1.40	1.40
1985	1689	18	14	1.07	0.83
1986	1888	18	17	0.95	0.90
1987	2089	25	23	1.20	1.10
1988	2255	11	10	0.49	0.44
1989	2374	16	14	0.67	0.59
1990	2484	23	16	0.93	0.64
1991	2610	14	14	0.54	0.54
1992	2755	31	30	1.13	1.09
1993	2866	38	31	1.33	1.08
1994	3037	31	27	1.02	0.89
1995	3212	56	49	1.74	1.53
1996	3381	45	37	1.33	1.09
1997	3516	74	70	2.10	1.99
1998	3613	68	58	1.88	1.61
1999	3625	98	87	2.70	2.40
2000	3711	85	78	2.29	2.1
2001	3705	66	N/A	1.78	N/A

Sources: BJS 2001 Cap. Pun. Study (death row pop.); Death Row U.S.A., Fall 2001 (executions).

Figure 7A. Persons on Death Row by Year, 1974-2001

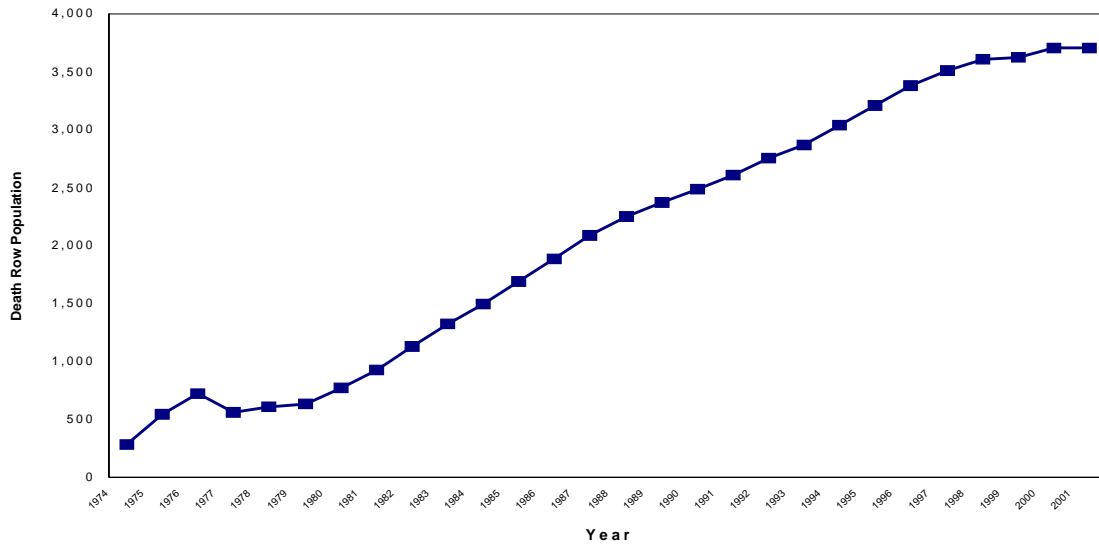


Figure 7B. Number of Executions by Year, 1974-2001

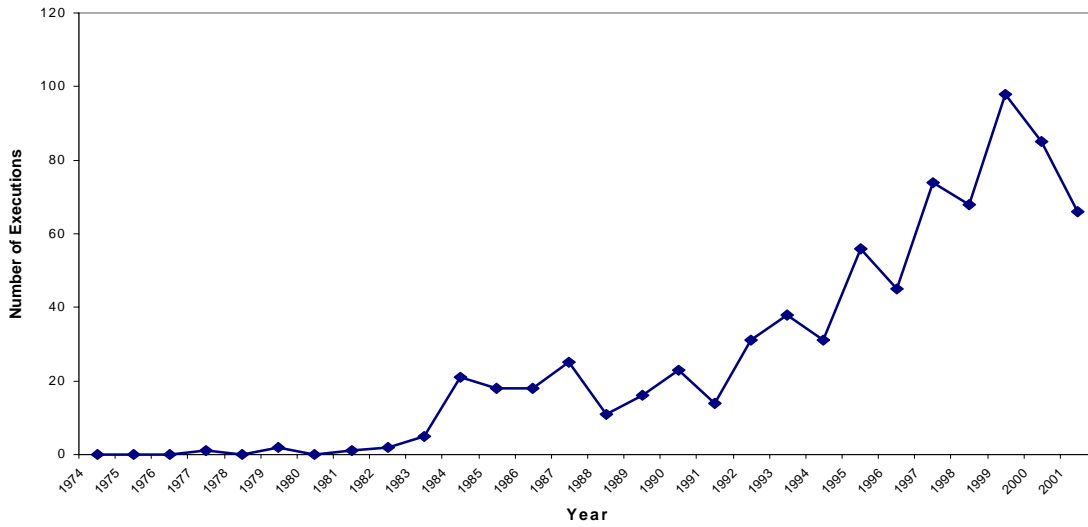
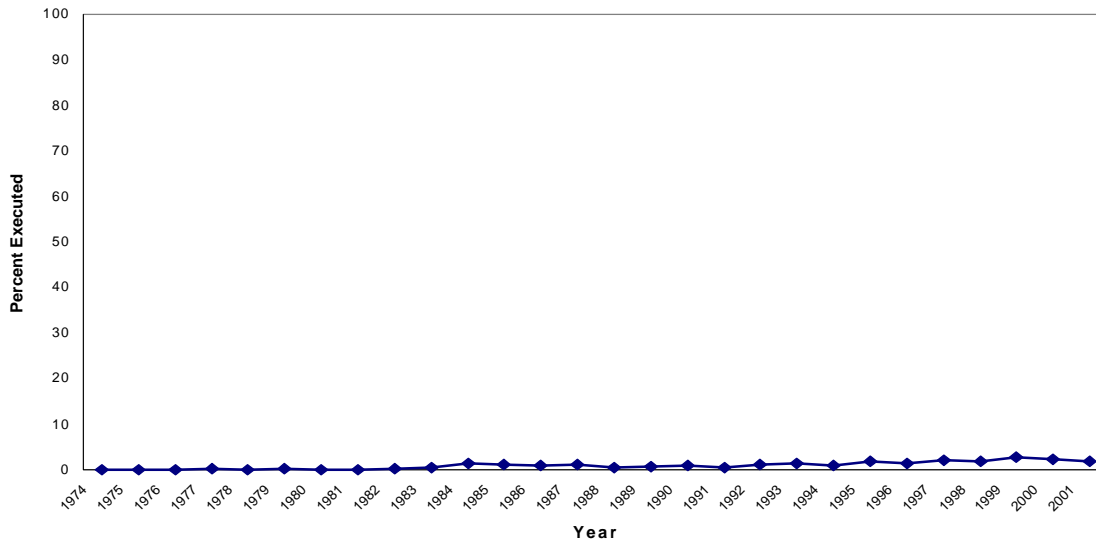
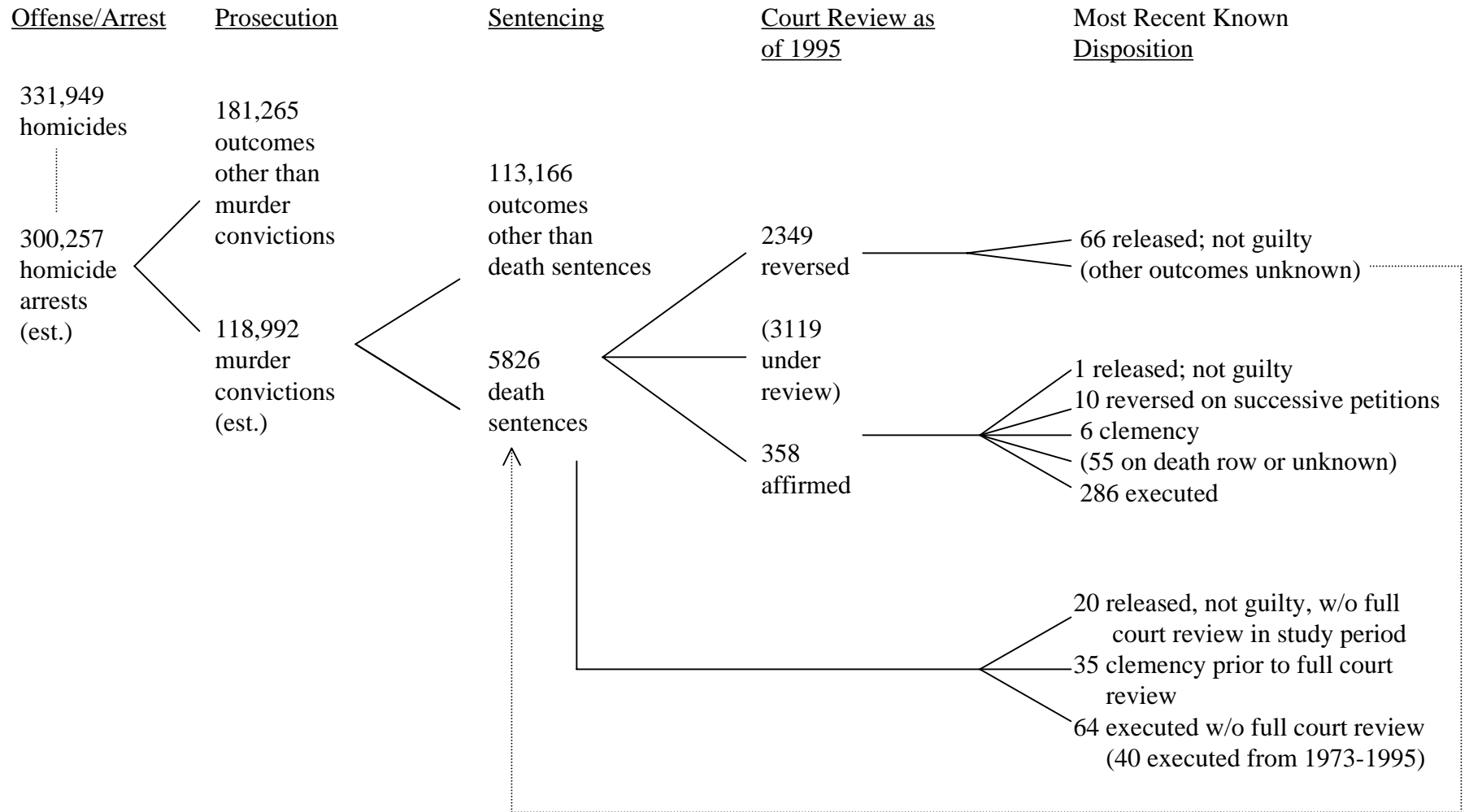


Figure 7C. Percent of Death Row Executed by Year, 1974-2001



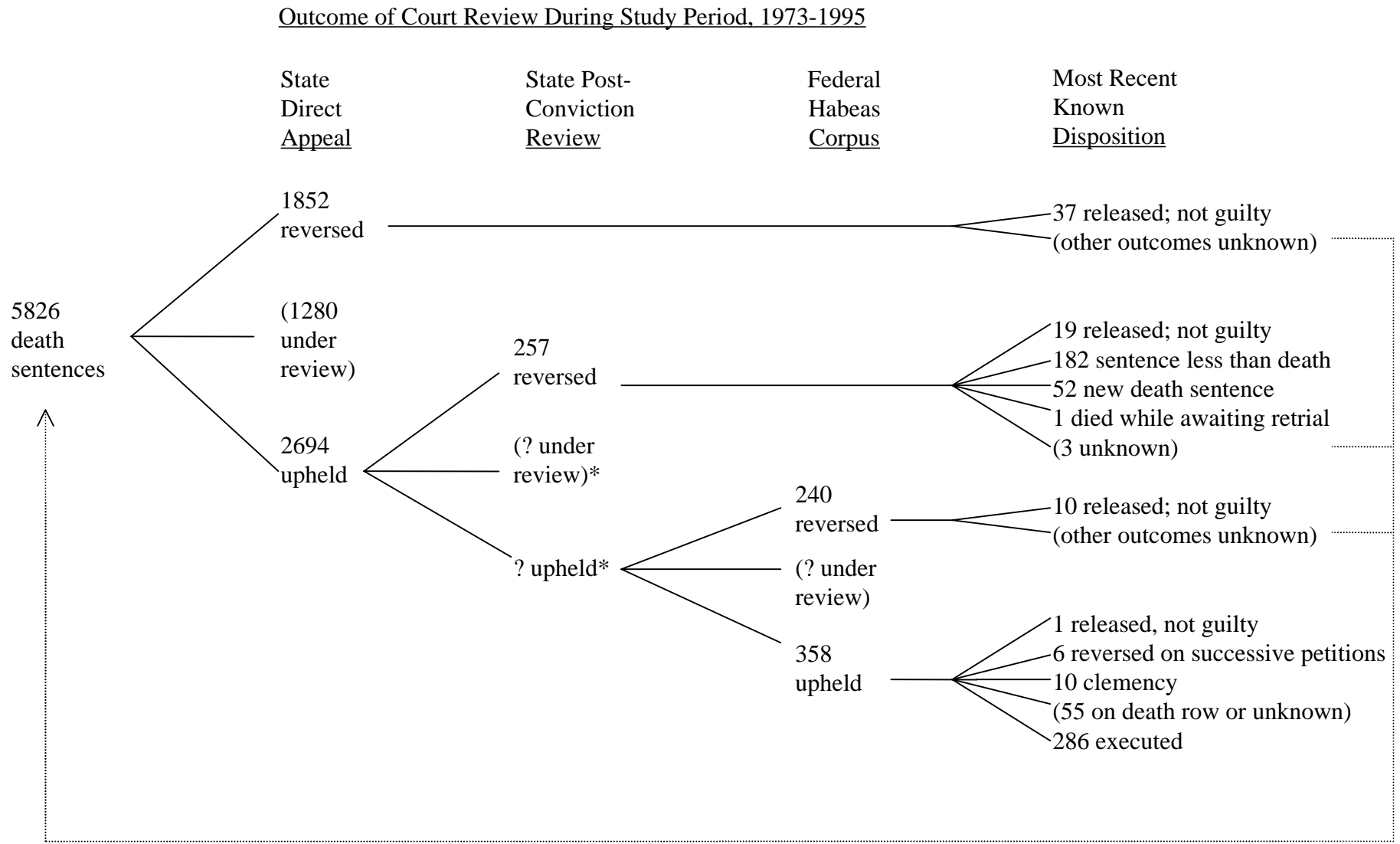
The capital system's functional success rate thus is between 1 and 8%. Indeed, as Figures 8A and 8B further illustrate, **the only possible sense in which the modern death penalty system can be said to “work” is as a costly Rube Goldberg contraption for making serious errors and then trying, but failing, to fix them.** Although intended to move cases quickly and smoothly from death verdict to execution, the main momentum of the system is in the opposite direction: After moving slowly and haltingly through a review process clogged with error-laden cases, most cases flow backwards to retrials, then out of the system entirely as non-capital sentences and acquittals. Among the trickle of cases getting through the review process to the execution stage are some in which the condemned prisoner is innocent, and more where the prisoner is guilty of a crime but not one for which the law allows to the death penalty to be imposed.

Figure 8A: Outcomes Following Arrest for Homicides Committed from 1973 to 1995 in States with the Death Penalty*^{225A}



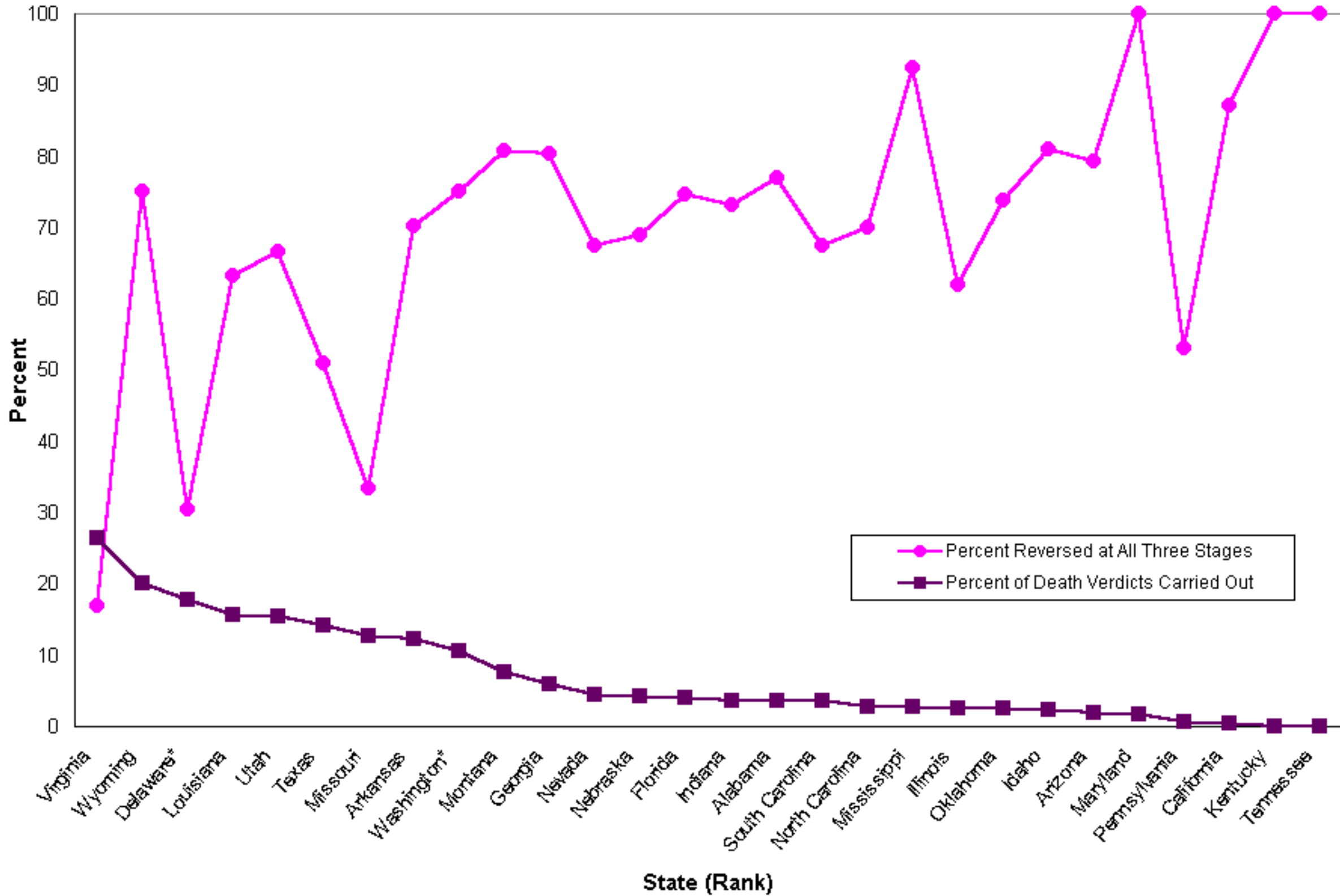
* See Figure 8B for more information on the three stages of court review and dispositions. Court review is for the direct appeal, state post conviction and initial federal habeas proceedings.

Figure 8B: Known Outcomes, Three Stages of State and Federal Court Review^{225A}



* For estimates see note 103 above.

Figure 9: Overall Error Rate and Percent of Death Verdicts Carried Out, 1973-1995⁺



The contribution of serious, reversible error to the capital system's dismal success rate is immense. Figure 9, p. 79 above, compares states with at least one fully reviewed death verdict based on (1) the percentage of death verdicts they imposed that were carried out during the study period (the dark grey line) and (2) their overall reversal rates (the light grey line). Figure 9 shows that:

- **More than 70% of the states executed fewer than 8% of the people they sentenced to die. All but one of those states had error rates over 60%; most of their error rates were over 70%; nearly half were 80% or more.**
- The two states with error rates below 30% (Delaware and Virginia) are the only two states that executed 18% or more of the men and women they condemned.
- **The correlation between high error rates and low rates of death sentences carried out is strong and significant.**²²⁶

It is hard to imagine another public or private operation being allowed to continue at all, much less for decades, with this record of error and failure. This is especially so, given the **cost**:

- Nearly 100 men and women spent years on death row for crimes they did not commit, or for which they eventually were acquitted. Many others were condemned for crimes for which the law does not permit death as a punishment.
- We cannot say how many innocent men and women have been executed, in part because officials with the information needed to answer the question won't release it. **But the risk that this has happened, and will happen again absent reform, is high.**
- While innocent people have languished on death row, actual killers have gone free, in some cases raping and killing again.²²⁷
- Murder victims' families have repeatedly had their expectations shattered, their grievous losses replayed, and their excruciating fears rekindled as a result of chronic reversals and retrials, altered outcomes and exonerations.²²⁸
- Requiring the same courts and judges to review growing numbers of seriously flawed death verdicts has caused a drastic pile-up of cases awaiting review.²²⁹ **During the 23-year study period, over 9 years passed on average between the death verdict and execution. In 1981, an average of about 5 years passed from death verdict to final habeas review. By 1995, the figure was about 12 years.**²³⁰
- Millions of dollars have been wasted on flawed trials and lengthy appeals.²³¹

- Retribution and deterrence have given way to uncertainty, mistake and delay.²³²
- Public faith in the criminal justice system has plummeted.²³³

The death penalty system documented here does not work. Instead, it plays a cruel hoax on taxpayers, the judicial system, innocent defendants and families of murder victims.

E. A Research Imperative: Seeking the Causes of Reversible Error in Death Cases

Serious, reversible error permeates the existing death penalty system and threatens to destroy it. It puts innocent lives at risk, heightens the suffering of victims, leaves killers at large, wastes tax dollars, and fails citizens, the courts and the justice system. Deciding whether the system is salvageable and, if so, how requires a better understanding of the causes of reversible error in capital trials and verdicts. The statistical analyses discussed in the rest of this Report address that question. Part III discusses study methods and data. Parts IV-VI report study results. Part VII distills those results into overall conclusions that then are the basis for a set of policy options outlined in Part VIII.

III. Research Questions, Methods, Data Sources and Format for Reporting Results

A. Research Question: What Accounts for Variation in the Amount of Serious, Reversible Capital Error from One State, County, Year and Case to the Next?

Some states have higher rates of reversible capital error than others. Some counties in those states have higher error rates than other counties in the same and in different states. And death verdicts imposed in some years are reversed more often than verdicts imposed in other years. These differences pose one of the central questions addressed in the remainder of this Report: **What factors tend to be present when capital reversal rates are especially high or low?** In addition, some verdicts are reversed at a particular stage of court review while other verdicts are approved at that stage. In regard to the federal habeas stage we ask a related question: **What factors tend to be present when capital verdicts are reversed, and when they are affirmed?**

These questions invite educated guesses, or hypotheses, about particular conditions that logic or experience suggests might have an effect on the existence and amount of reversible error. One might predict, for example, that poorly funded courts are more likely to impose flawed verdicts than better funded courts. Such hypotheses can be tested by answering the following question: **Do the existence and amount of reversible error change from one state, county, year or case to the next when particular conditions are and are not present, or are present to a different degree?**

Although one can examine each condition separately, more is learned by examining all of them simultaneously. Doing so helps identify a stronger overall explanation based on multiple conditions, and also takes into account relationships among the conditions as well as between each of them and amounts and rates of capital reversals.²³⁴ For example, crude initial analyses might reveal that changes in each of two conditions coincide with changes in capital reversal rates: reversal rates go up (1) when

the speed with which relevant jurisdictions decide all court cases goes down, and (2) when spending on courts goes down. This might seem to suggest that both decision delays and underfunded courts “cause” high reversal rates. Suppose, though, that poor funding leads to both delay in deciding cases and flawed death verdicts, so decision delays and flawed verdicts follow somewhat similar patterns of change—not because they are causally related to each other but because both are affected by funding changes. In this event, simultaneously examining the effect on reversal rates of both funding *and* decision time would likely reveal that only funding has a strong relationship to reversal rates.

Revised in light of the importance of analyzing multiple factors at the same time, our central research question is this: **What set of potentially explanatory conditions tends to be present, and to increase in amount or intensity, in those places, years and cases in which serious capital error is present and increases in amount?** Answering this empirical question may help answer two policy questions posed by chronic capital error:

- **What reforms might keep serious capital error from occurring as often in the future as they have in the past?**
- **Can such reforms reasonably contain the risks and costs that currently overwhelm the capital system?**

This Part discusses the methods and data used to answer our central research question. Parts IV-VI discuss the results of 19 statistical analyses applying these methods to our data on capital reversals. Based on those results, Part VII reaches a comprehensive set of conclusions about the factors associated with high numbers and rates of serious capital error. Then, based on those conclusions, Part VIII addresses the two policy questions above, listing reform options for moderating the problem of serious capital error.

B. Why it Is Helpful to Use Regression Analysis to Identify Statistically Significant Relationships Between Changes in Potentially Explanatory Conditions and Changes in Capital Reversal Rates

1. The chronic nature of capital error makes it important to look for explanations for reversals beyond the reasons courts give in each case.

Errors that occur at capital trials or in investigations leading up to them are typically discovered in capital appeals whose purpose is to inspect trials for serious error and, if it is found, to reverse the verdict and send the case back for a new trial at which the flaw is avoided. In this sense, the question, *Why do reversals occur?*, can be answered with lists like those at pp. 40-43 and notes 167, 169 of the reasons courts give for reversing death verdicts. By this measure, **reversals most often occur at the review stages about which information is available for study because of egregiously incompetent defense lawyers, the withholding by police and prosecutors of important evidence that the defendant did not commit a capital offense, instructions to jurors that badly misstate the law, and judge and juror bias.**²³⁵

Although there is a temptation to accept this information as a complete answer to the question of why error occurs, it actually raises much more fundamental questions. **Although the public and policy makers have not always been kept informed, key actors in the death penalty system—state’s attorneys, the capital defense bar and trial and appellate judges—have known for a long time (1) that most death verdicts are overturned because of serious errors, and (2) what those errors are.**²³⁶ Yet, knowing this has not led those actors to reform the system.²³⁷ **Why? Are high amounts of capital error inevitable? Or are there reasons—more basic than the particular flaws leading to each reversal— why the same flaws occur again and again, causing the entire system to fail,**²³⁸ **and why participants cannot or will not fix it? And if those reasons are**

discovered, can they help the public, policy makers and others outside the system to reform it and avoid its worst mistakes?

Rather than assuming from the start that serious capital error is inevitable, and that the public must either resign itself to chronic flaws or abolish the penalty, this Report pursues rigorous and systematic answers to the research questions set out above, which we boil down to these two: **Why are some states, counties, periods and cases more prone to capital mistakes than others? What conditions are present when such mistakes are common, and what factors prevail when they are rare?** Regression analysis provides a useful way to answer these questions because it enables us to:

- identify more deep-seated conditions than the actions that led to reversals in each case that are associated with an increased probability that reversible error will be found in capital cases;
- explain why error chronically recurs despite frequent reversals; and
- sort out the relationships between factors that are individually associated with higher rates of serious error by testing them simultaneously to see if the relationship remains after controlling for other relevant factors.

2. The uses of statistically significant results.

The goal of our regression analyses is to identify conditions that tend to be present, and to increase in amount or intensity, when reversible error is present and increases, *where that pattern is statistically significant*. Statistical significance means the probability is small—5% or less—that the pattern appears in the data purely by chance.²³⁹ A statistically significant finding poses an additional question: Is there a cause and effect relationship between reversible error and conditions that are linked to reversible error in a statistically significant way?

Answering this question requires more than statistical analysis. One must use common sense based on other information and experience. Suppose delay in processing court cases is high in places

where capital reversal rates are high. This might imply a cause-and-effect relationship between the two conditions, but it does not prove it or show which way causation runs. Courts bogged down with many cases might generate faulty death verdicts. Or having to review many faulty death verdicts might delay other cases. Or both conditions might result from a third factor such as poor funding.

Even so, discovering significant relationships between capital reversal rates and other conditions on which we have data reveals much more than we knew before, and may strongly suggest causal hypotheses and policy options not previously known. In some contexts, relationships between events of possible importance are relatively obvious based on everyday observation, as are their implications for action—*e.g.*, the relationship between clouds and rain, and what it tells us about when to grab an umbrella before heading out the door. But this is not true of a system as complex and removed from most citizens' everyday experience as the death penalty. In that context, recognizing significant relationships between conditions we can change (*e.g.*, high caseloads or poor funding) and outcomes we want to avoid (*e.g.*, unreliable death verdicts, costly errors and retrials and executing the innocent) requires supplements to everyday observation. Providing those supplements is the goal of the statistical analyses we describe below.

C. Study Methods

1. Six traits of the capital system and available data that shape analysis.

The nature of a system being studied, and the information available about it, shape the way researchers study it. Six such considerations shape our study.

a. A lack of variation, given nearly uniform bottom-line failure.

As we note above, few death verdicts pass inspection and are carried out. Although the 34 states with active death penalties during the 1973-1995 period imposed 5826 death verdicts during

that period, only 358 (6%) of the verdicts were approved for execution by all three sets of courts that review death verdicts. And only 313 (5%) of the verdicts were carried out—about half of which were from only two states (Texas and Virginia), with the rest sprinkled among about 20 other states.²⁴⁰ Thus, although it is possible to define executions in this context as success and everything else as failure, success is too rare and failure too uniform to provide a useful basis for analyzing the factors associated with either.

This calls for another measure of failure besides the across-the-board failure at the bottom line. The measure we use is *court reversals based on findings of serious error*, which are highly correlated with a failure to carry out death sentences.²⁴¹ When all three review stages are considered, reversals and their opposite (affirmances) number in the thousands and vary considerably across place and time—although, as we have noted, most of the variation is above a disturbingly high level of reversals that is common to most places and years.²⁴²

b. Different dates when states began imposing death verdicts under valid capital statutes.

After the U.S. Supreme Court invalidated all existing capital statutes and sentences in 1972,²⁴³ the 34 states in our study moved at different speeds to adopt revised capital statutes. Then, in 1976, the Supreme Court invalidated a substantial number, but not all, of the new statutes because they made the death penalty mandatory for entire classes of murder, in violation of the U.S. Constitution.²⁴⁴ States that had adopted mandatory death penalties and wished to continue imposing capital sentences had to draft revised statutes a second time, which they again did at different speeds. For these reasons, the 34 study states began imposing the death verdicts we study here—those imposed under valid modern statutes²⁴⁵—at times spread over about a decade. The fact that different study states started

capital sentencing under valid statutes in different years is a complication, because it means that the states have capital-sentencing experiences of different durations, and potentially of a different character based on when they began.

To permit simultaneous comparison of states that began using the death penalty under valid statutes in different years, we include time in most of our regressions. To begin with, we measure reversal rates not only by state but also by year. Making the unit of comparison each state's capital experience in each year when it imposed at least one death verdict under a valid statute, rather than each state's overall capital experience during the entire study period, helps account for the different duration of each state's capital-sentencing experience, and for differences among states tied to particular years when some were and others were not applying valid capital statutes. Second, we typically include year as a random effect, an analytic strategy which assumes that verdicts imposed in the same year (even if they are imposed by different states) are probably more alike than verdicts imposed in different years. Third, we include the time trend as a potential explanatory factor, to identify patterns of reversal rates over time that are not accounted for by other factors in the analysis.

As we point out below, including time adds complications of its own in interpreting the effect on reversal rates of time, and especially the time trend.²⁴⁶ For that reason, and also to gauge the importance of time and to provide policy makers with a direct comparison of the 34 states' overall capital-sentencing experiences under valid statutes during the 23-year study period, we remove time from consideration in a small number of supplemental analyses.²⁴⁷

c. A choice between reversal rates for *reviewed* and *imposed* verdicts.

Deciding to compare reversal rates across place and time poses another question: Should we try to explain differences in the number of death verdicts reversed during the study period as a

proportion of the number of verdicts that were *finally reviewed* during the period, or as a proportion of the number of verdicts that were *imposed* in the period? Arithmetically, the question is:

$$\text{number reversed} \div \text{number reviewed} \quad \text{vs.} \quad \text{number reversed} \div \text{number imposed} \quad ?$$

As we note above, the first of these measures is the true *error* rate—the proportion of verdicts that were inspected and found to be flawed.²⁴⁸ In contrast, the latter ratio gauges error *and delay*, because the rate of imposed verdicts that were reversed as of a given time is affected by both conditions: It may be that many verdicts were inspected and only a few were found to be flawed. In that event, a low error rate leads to a low reversal rate. Or it may be that the review process was delayed, so only a few verdicts were reviewed, and thus only a small proportion of imposed verdicts were reversed. In the latter event, even if *all* of the verdicts that were reviewed were found to be flawed, the reversal rate (as a proportion of all *imposed* verdicts) will still be very low, because most verdicts got mired in the review process and did not generate reversals *or* affirmances. In the latter event, it is delay in the review process that leads to a low reversal rate as a proportion of imposed verdicts.

If the ratio of reversed verdicts to imposed verdicts is used to measure error, therefore, it is necessary to “control for” delay. This means identifying one or more factors known to be related to delay in reviewing death verdicts, and measuring their relationship—along with the relationship of conditions thought to be associated with error, not delay—to rates of imposed death verdicts being reversed. When all such factors are studied together, the factors known to be associated with delay can be used to capture that influence on reversal rates, leaving other conditions associated with differences in reversal rates to be linked to error.

We have identified two factors that control for the proportion of imposed verdicts that have not been reversed as of a given time because of delay. The first factor is the year of the death verdict.

The more recently a verdict was imposed during the study period, the less likely it is that the verdict will have been reviewed by the end of that period. Because review takes time, the later a death verdict enters the queue of capital verdicts awaiting review, the later it is likely to be reviewed, and the more probable it is that it will not have completed that process by the time the study period ended. As we develop in more detail just below, this means that in studies of reversals as proportions of imposed verdicts, time trend—the year a verdict was imposed—is likely to be negatively associated with reversal rates, not because later verdicts are less prone to error, but simply because later verdicts are less likely to have completed the review process.²⁴⁹

A second measure of delay—the number of capital verdicts awaiting review at any given time—is sensitive not only to how far back in the queue of capital appeals a case is located, but also to how slowly the queue is moving. Thus, the number of backlogged capital cases measures both the number of capital cases experiencing appellate delays, and the severity of a key condition that causes delay—namely, capital litigation itself. Every capital appeal requires a huge commitment of court time, given the factual and legal complexity of capital cases and given the frequency with which capital verdicts are marred by reversible error.²⁵⁰ In addition, all capital appeals must pass once, and usually twice, through a small bottleneck in all states. Nearly all capital cases are reviewed in the first, and usually a second, instance by a single state high court with approximately the same small number of judges from one state to the next—often five or seven. The higher the number of capital appeals pending at any given time, the more clogged the appellate bottleneck is likely to be with these difficult cases, and the more slowly capital appeals are likely to move.

d. Uncounted reversals of death verdicts imposed in later years.

As we have just noted, comparing states based on the proportion of *imposed* verdicts that were

reversed as of the end of the study period makes it difficult to gauge the relationship of the passage of time to *error*, as opposed to its relationship to unfinished or delayed review. Suppose we find that the proportion of death verdicts imposed in 1993 that were reversed as of the end of 1995 is smaller than the proportion of death verdicts imposed 10 years earlier that were reversed by that point. This result could mean that death verdicts imposed in 1993 were freer of error than those imposed in 1983. But it could also mean that verdicts imposed in 1993 were just as error-ridden as ones imposed in 1983 (or were worse) but that reviewing courts only had two years, not *12*, to find all the errors, leaving many errors still to be discovered by the time the study ended. A decline in the rate of imposed verdicts that were reversed over time thus is not a useful measure of the trend of *error* over time.

One way to improve the power of the time trend (the year verdicts were imposed) to gauge whether error is increasing or decreasing over time after controlling for other factors is to compare rates of *reviewed* verdicts that were reversed, instead of comparing rates of imposed verdicts that were reversed. Unfortunately, comparing reversal rates for reviewed cases does not entirely avoid the link between recent verdicts and unfinished appeals, because at the third, federal habeas stage of review, flawed verdicts take longer to review than verdicts without reversible error. Figure 10, p. 93 below, shows that:

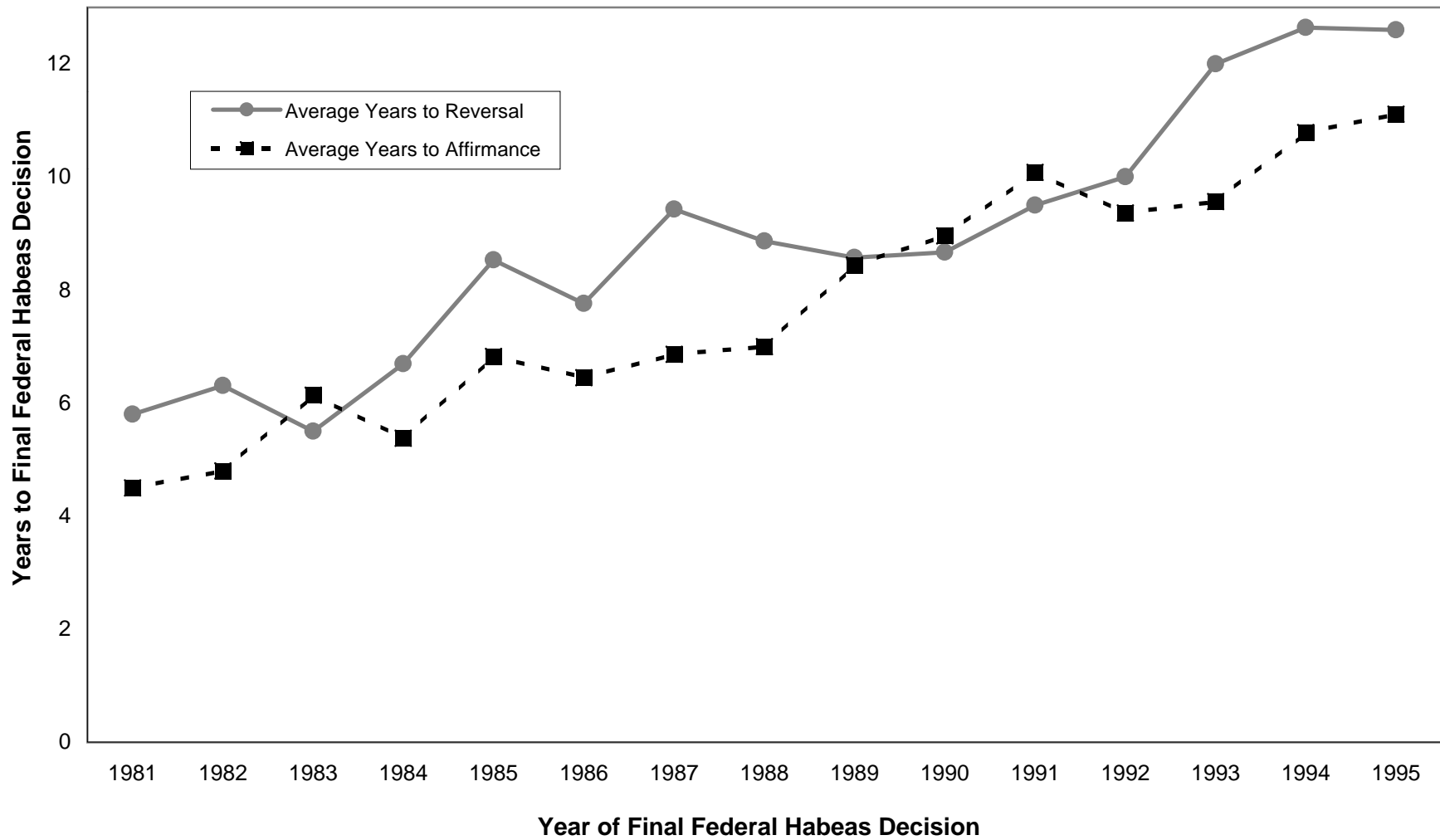
- **All verdicts finally reviewed on federal habeas during the study period spent much more time under review in state and federal court in later years than in earlier years—rising from about *5½ years* on average from sentence to final habeas review for verdicts finally reviewed in 1981, to *12 years* for verdicts finally reviewed in 1995.**
- **Of greater interest here, the amount of time taken to complete federal habeas review of cases in which relief was granted has generally been longer—by the end of the study period it was about *two years* longer (averaging about 13 years)—than for cases in which habeas relief was denied (averaging about 11 years by the end of the study period).**

Given the latter fact, our time-limited study systematically **understates reversal rates over**

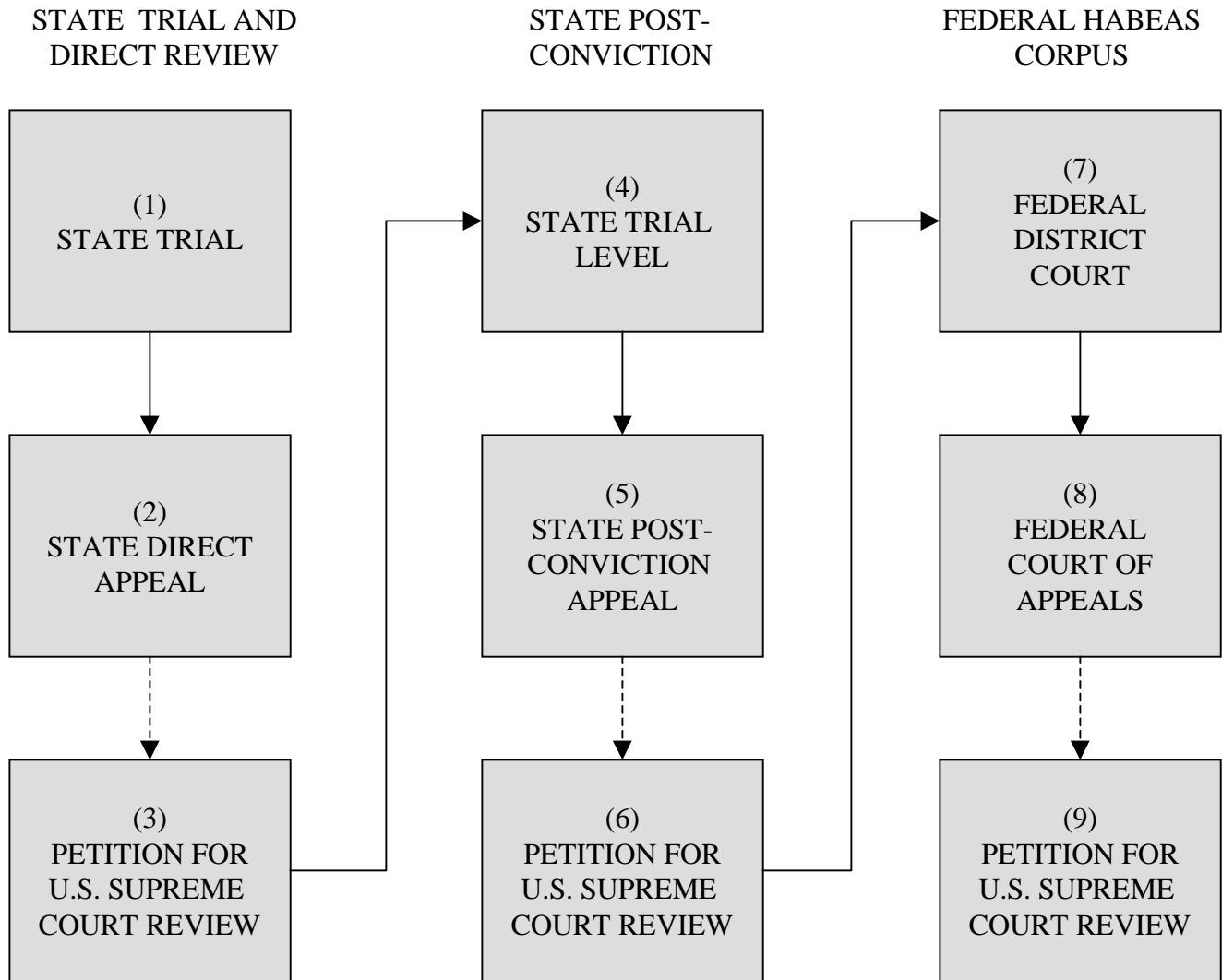
time at the federal habeas stage: Death verdicts without reversible error are over-represented among the verdicts imposed in any given year that were finally reviewed on federal habeas by the end of the study period, because those verdicts take less time to review. Conversely, death verdicts *with* reversible flaws are under-represented among verdicts finally reviewed on habeas by the end date, because they take longer to review. Because the number of unreviewed verdicts rises as the sentencing year gets more recent (fewer 1989 death verdicts were finally reviewed as of 1995 than 1988 ones; fewer 1988 verdicts were reviewed as of then than 1987 ones, and so forth²⁵¹), the impact of the bias against counting reversible error as of 1995 that eventually will be discovered and reversed grows with each successive sentencing year.

The result is a false impression that later death verdicts are cleaner than they are. What instead is happening is that cleaner verdicts move to the front of the line of cases getting finally reviewed, shoving flawed verdicts to the back of the line. As the sentencing year gets later, the proportion of verdicts awaiting review as of the cut-off date gets larger, as does the proportion of *flawed* verdicts towards the back of the line that have not yet been reversed—and, so, are not counted in our study.

Figure 10. Average Time to Final Federal Habeas Reversals and Affirmances, by Year of Final Decision, 1981-1995



THE CAPITAL CRIMINAL PROCESS: TRIAL THROUGH STATE POST-CONVICTION AND FEDERAL HABEAS



e. Large number of verdicts trapped in a multi-stage review process.

Analyzing only *reviewed* death verdicts also poses a choice about the reviewed verdicts to study. One might study only verdicts fully reviewed at all three review stages that are depicted in the stylized flow chart on p. 94 above—state direct appeal in state high courts, state post-conviction review in state trial and high courts, and federal habeas review in federal trial and appellate courts. But of the 5826 death verdicts imposed during the study period, only 598 were reviewed at all three stages. Studying only those verdicts limits analysis to state differences, because only a few counties have fully reviewed verdicts, and ignores information from thousands of court decisions reviewing death verdicts at earlier stages. And the state differences that this approach explores are limited to a subset of active death penalty states in which cases had progressed through the entire three-stage review process as of 1995.

Or one could combine all final decisions at each of the three review stages, even if the verdict never made it to a later review stage. But any reversal at the second review stage necessarily follows an affirmance of the same verdict at the first review stage; and any reversal at the third review stage necessarily follows an affirmance of the verdict at two prior stages. As a result, any condition found in a case that leads to a second-stage reversal will also necessarily be found in a case with the opposite outcome, affirming the verdict—*i.e.*, the case reviewing the same verdict at the first review stage. And any condition found in a case that leads to a third-stage reversal will also necessarily be found in *two* cases that affirmed the verdict—the cases reviewing the same verdict at the first and second stages of review. Studying reversals as a proportion of court decisions at all three stages combined thus might dilute the effect of forces that account for reversals in proportion to how late in the review process the reversal occurred.

The only way to analyze reversals at all three stages combined, therefore, is to consider reversals as a proportion of *imposed* death verdicts, not *reviewed* verdicts. Then, each verdict only counts once, as either a reversal or a non-reversal (the latter meaning either an affirmance at some or all stages, or that the case was delayed and not decided at all). If analysis of only reviewed verdicts is desired, the data must be divided into three clumps—one for reversals and affirmances issued at each of the three review stages.

f. Smaller number of cases at second and third review stages.

The value of the latter approach—separately examining *reviewed* verdicts at each review stage—is moderated by the successively smaller number of cases reaching the second and third review stages. Stage-by-stage analysis generally can only compare states, because there too few cases spread among too many counties to reach useful conclusions at the county level of analysis.²⁵² And even when comparing states, the small number of cases at the latter two stages makes it more difficult to reach comprehensive conclusions.

As we discuss above, moreover, we do not know how many cases were *reviewed* at the second, state post-conviction stage, but only how many were *reversed* at that stage. This information enables us to conservatively estimate state post-conviction reversal rates based on the assumptions that no delays occurred at the second review stage and that every case clearing the first stage was immediately and fully reviewed at the second stage.²⁵³ But these same assumptions do not permit us, at this second stage of review, to achieve the advantages of comparing reversal rates among verdicts that were actually reviewed, as opposed to those that were imposed and were merely available for review.

2. A prudent strategy for reaching conclusions: the results of a principal analysis, tested by the results of 18 follow-up analyses.

These theoretical and empirical considerations call for our main analysis—Analysis 1—to use **over-dispersed binomial logistic regression analysis**²⁵⁴ to study:

- **state-level factors,**
- **that explain differences from state to state and year to year**
- **in capital reversal rates calculated as the number of reversals at all three review stages combined as a proportion of all *imposed* verdicts,**
- **using time trend and the number of backlogged death verdicts awaiting review**²⁵⁵ **to capture the relationship between unfinished and delayed review and lower reversal rates, leaving other significant factors to explain differences in capital *error* rates that lead to differences in capital reversal rates.**

We include these components in our main analysis because they make the best use of all of the detailed information we have collected to identify conditions associated with serious capital error.

To begin to test whether our main results are robust, we first follow-up our main binomial logistic regression with an otherwise identical analysis that uses a different kind of regression—Poisson logarithmic regression—with different assumptions about patterns of capital reversal rates. Robust results are ones that consistently reveal similar relationships between capital error and the explanatory factors the analyses identify, regardless of the type of regression used.

As further tests of the robustness of our main results, we conduct 16 additional follow-up analyses, each altering one or more components of the main analysis to see if its results depend on that particular aspect of the study design. Substantial similarity between the results of the main and follow-up analyses would be a very good indication that our results reflect actual relationships in the data and are not sensitive to the details of the statistical method being used. Each follow-up study substitutes

one or more of the following components for the corresponding aspects of the main analysis:

- analysis of reversal rates at each separate capital review stage, as opposed to reversal rates at all three stages combined, as the outcome to be explained;
- analysis of reversal rates as a proportion of verdicts reviewed, as opposed to verdicts imposed, during the study period;
- different assumptions about the importance of time and the year death verdicts were imposed;
- inclusion of county-level explanatory factors for state reversal rates;
- use of county-to-county (rather than, and sometimes simultaneously with, state-to-state) differences in reversal rates as the outcome to be explained, with a variety of assumptions about the effect of states on counties and vice versa;
- use of the results of individual appeals of capital verdicts at a particular review stage (as opposed to reversal rates from the aggregate of all such appeals) as the outcome to be explained, calling for a different type of regression analysis; and
- different tests of the relationship between delay and rates of reversal and error.

This strategy is prudent because it limits conclusions to relationships:

- **present in 23 years worth of carefully compiled and conservatively measured information about the existence and amount of capital outcomes in this country;²⁵⁶ and**
- **confirmed by multiple overlapping results using different sets of explanatory factors, different levels of aggregation of data, different definitions of the outcome being explained and different probability measures.**

The next section describes our main analysis and 18 follow-up analyses. The analyses are grouped by those examining differences in:

- capital reversal rates across states (main Analysis 1; follow-up Analyses 2-6, 14, 15);
- capital reversal rates across counties (and, sometimes, states) (Analyses 7-13, 16-18); and
- outcomes of federal habeas appeals reviewing capital verdicts (Analysis 19).

3. Detailed description of the main and 18 follow-up analyses.

a. Main analysis and seven follow-up analyses explaining differences in rates of serious capital error across states.

i. Main Analysis 1: over-dispersed binomial logistic regression analysis of the probability of reversal at all three review stages combined. Our main analysis, Analysis 1, explains variation in the number of reversals at all three review stages, as a proportion of the total number of death verdicts imposed in each of the 34 study states in each of the 23 study years in which the state imposed at least one death verdict. The total number of combinations of states and years compared in this analysis (*i.e.*, the sum of the 34 study state times the number of years out of the 23 studied in which that state imposed at least one death verdict) is 519. (Although 34 states times 23 years establishes a maximum of 782 possible “state-years,” not all states imposed death verdicts under valid death-sentencing statutes in each of the 23 years.) The analysis uses an over-dispersed binomial logistic regression technique designed to explain conditions (here, rates of serious, reversible error) with a known range of possible outcomes (here, values ranging from 0 to 100%).²⁵⁷

This analysis tests the explanatory power of a variety of specific factors and conditions that might potentially explain variations in reversible error across time and place (*e.g.*, court funding and caseloads). One of those factors is time as a linear trend, which asks whether a pattern of increasing or decreasing amounts of error over time explains changes in reversal rates. As is discussed above, time trend, as well as backlogs of cases awaiting review, are included to isolate the effect of unfinished review and delay, as opposed to error, on reversal rates: By lowering the number of final decisions reviewing imposed death verdicts, unfinished appeals and delay decrease the number of reversals without corresponding gains in the quality of death verdicts.²⁵⁸

Analysis 1 treats each of the 34 study states and each of the 23 study years as random effects. That gives these generalized factors the maximum ability to explain variance in reversal rates. Doing so helps assure that the specific explanatory factors we examine (*e.g.*, court caseloads or funding) do not get credit for explaining variance that instead is attributable to conditions we have not studied but that vary from state to state (in which case each of the states is likely to get credit for explaining the variance that the missing factor causes) or that vary over time (meaning years may get credit for the variance the missing time-dependent factor causes).

ii. Analysis 2: *over-dispersed Poisson logarithmic regression analysis of the probability of reversal at all three review stages combined*. Analysis 2 uses a different statistical technique, over-dispersed Poisson logarithmic regression, which is used to explain counts of events that are relatively rare.²⁵⁹ Because we deflate reversal rates in this analysis (as in Analysis 1) by calculating them as a proportion of all imposed death verdicts, not all reviewed verdicts,²⁶⁰ the distribution of the condition we are explaining (serious, reversible errors) might reasonably be explained by an over-dispersed Poisson regression. Another advantage of the Poisson regression is that its results provide a somewhat more easily interpreted description than binomial regressions of how much one expects reversal rates to rise or fall based on a specified change in a significant factor (*e.g.*, given an additional \$100 per capita in public spending on the courts). Otherwise, Analysis 2 is similar to Analysis 1.

iii. Analysis 3: *binomial regression analysis of the probability of reversal on direct appeal*. Analysis 3, another follow-up inquiry, explains variation in the number of reversals at a single stage of review—state direct appeal. Limiting analysis to one stage allows us to identify factors related to reversals as a proportion of death verdicts that were fully *reviewed* during the study period.²⁶¹ Analysis 3 (like Analyses 4 and 6 and, to some extent Analysis 5, below) thus enables us to see if:

- reversal rates at one review stage behave differently from those at all three stages combined;
- Analysis 1 and 2's results for all three review stages combined miss factors operating only at particular review stages; and
- results differ when the reversal rates being studied are calculated as a proportion of reviewed, not imposed, verdicts.

The total number of combinations of states and years compared in Analysis 3 (the sum of the 34 study states times the number of years out of the 23 studied in which each state imposed at least one death verdict that was fully reviewed at the state direct appeal stage during the study period) is 453. This analysis compares fewer state-years than the prior analyses because it explains variation in reversal rates only for state-years in which at least one death verdict was finally *reviewed* at the direct appeal stage during the study period, rather than focusing on the larger number of state-years in which at least one verdict was *imposed* during the period even if the verdict was never finally reviewed during that period. In other respects, this analysis is similar to Analysis 1.

iv. Analysis 4: Poisson regression analysis of the probability of reversal on direct appeal. Analysis 4 also explains differences in the number of reversals of death verdicts at the direct appeal stage as a proportion of fully reviewed verdicts. It is similar to Analysis 3 in other respects, as well, except that it uses a Poisson regression, rather than a binomial regression, for reasons discussed above in regard to Analysis 2.²⁶²

v. Analysis 5: Poisson regression analysis of the probability of reversal on state post-conviction review. Analysis 5 explains variation in the number of reversals of death verdicts at the state post-conviction stage. As is discussed above, we do not know how many verdicts were finally reviewed at this stage, but we do know how many were *available* for review in that they had been approved at the direct appeal stage that immediately precedes the state post-conviction stage.²⁶³

Analysis 5 accordingly considers the number of state post-conviction reversals of death verdicts as a proportion of the number of verdicts available for state post-conviction review after being approved on direct appeal. Analysis 5 includes 26, not 34, states—only those in which death verdicts completed state post-conviction review during the study period for which reversal data are available. The total number of combinations of states and years in this analysis is 359. Analysis 5 uses Poisson regression because of the relatively large proportion of values less than .5 being explained, given relatively low reversal rates at the state post-conviction stage and the use in this analysis of a large base number, or denominator—the number of verdicts available for review, not the number actually reviewed—which as we have shown depresses the reversal rates being studied.²⁶⁴

vi. Analysis 6: *binomial regression analysis of the probability of reversal on federal habeas review*. Analysis 6 explains differences in the number of reversals of death verdicts at the federal habeas stage as a proportion of the number of death verdicts that were fully reviewed at this stage. Analysis 6 includes 28 states, all those in which one or more death verdicts completed federal habeas review during the study period. The total number of combinations of states and years examined in this analysis is 161. The smaller number of observed reversal rates to be explained—161 state-years, compared to 519 for Analyses 1 and 2, 453 for Analyses 3 and 4 and 354 for Analysis 5—makes it more difficult for explanatory values to achieve statistical significance, especially given the treatment of states and years as random effects. Because Analysis 6 examines reversal rates on habeas that range fairly evenly from 0 to 100% (given relatively high reversal rates and the use here of the smaller of the two denominators, *i.e.*, reviewed verdicts, not imposed verdicts), we use a binomial regression.²⁶⁵

vii. Analysis 14: *binomial regression analysis of the probability of reversal at all review stages combined, with state but not year as a random effect*. Analysis 14²⁶⁶ (a binomial regression)

modifies main Analysis 1 in an important respect. Whereas Analysis 1 treats both the state and year in which death verdicts were imposed as random effects, Analysis 14 treats only state as a random effect. Analysis 14 thus assumes that reversal rates for all years in each state are relatively responsive to the same set of factors, without making the same assumption about reversal rates for all states in each of the 23 study years. In other words, Analysis 14 clusters the 519 observed reversal rates (one for each relevant state and year) into 34 groups based on the state where the verdicts were imposed and attempts to explain differences among the clusters. But it does not create or attempt to explain differences among cross-cutting clusters based on the year in which death verdicts were imposed. Analysis 14 thus is designed to explain differences in the 34 study states' experiences with capital reversals over the 23-year study period as a whole, with each state's experience being the composite of its reversal rates in all study years in which it imposed death verdicts.

Removing time as a consideration and comparing each state's 23-year experience with capital reversals to that of the other 33 study states makes Analysis 14 useful to policy makers who want to know how much, and why, their state's capital success and failure rates in the modern capital period as a whole differ from the success and failure rates of other capital states in the same period. A comparison of Analysis 1 and Analysis 14 also gauges the importance of time by gauging how much results differ when reversal rates are and are not clustered based on death-sentencing year.

viii. Analysis 15: Poisson regression analysis of the probability of reversal at all review stages combined, with state but not year a random effect. Analysis 15 is the same as Analysis 14, except that it uses a Poisson regression to make sure that results are not tied to the binomial method of analysis.

b. Ten analyses explaining differences in rates of serious capital error across counties as well as states.

The analyses in this section consider whether results change, or whether more is learned about

factors related to capital reversal rates, when county-level as well as state-level reversal rates are studied and when county-level as well as state-level explanations for reversal rates are tested. Because these analyses are designed to identify conditions associated with court *reversals* of capital verdicts, and not the conditions that produced the verdicts themselves, we analyze only those counties that imposed one or more death verdict during the study period.

i. Analysis 7: Poisson regression analysis of county explanations for county reversal rates at all review stages combined. Analysis 7 begins our county-level inquiries by comparing county reversal rates and county-level explanations for those rates among the 967 counties within the 34 study states that imposed at least one death verdict during the 23-year study period, where the year of that verdict is known.²⁶⁷ This analysis considers only potential county-level explanations, as well as time, but not state-level explanations, for differences in *county* reversal rates. Because Analysis 7 omits states altogether, even as categories within which counties are grouped for purposes of analysis, it treats each death-sentencing county in the U.S. as an entirely separate unit on a par with all other counties, regardless of whether or not the counties are in the same state. This approach is only a starting point, to identify the full range of conditions operating at the county level that might have an effect on reversal rates. Very possibly, however, introducing states back into the analysis—as we do in our subsequent county analyses—will show that states or state-level explanations turn out to explain variation that a county-only analysis at first seems to attribute to county-level conditions.

Analysis 7 explains variation in the number of reversals at all three stages of review, as a proportion of the total number of death verdicts imposed in each of relevant counties and years. The total number of combinations of counties and years compared in this analysis (the 967 study counties times the number of years out of the 23 studied in which each imposed at least one death verdict) is

3054. (Not all counties imposed death verdicts under valid capital statutes in each of the 23 years.) Analysis 7 tests the explanatory power of county-level conditions comparable to those operating at the state level that are evaluated in Analyses 1-6. It uses Poisson regression analysis and treats time as both a random and a fixed effect.²⁶⁸

ii. Analysis 8: *county-within-state binomial regression analysis of county and state explanations for county reversal rates at all review stages combined*. Like Analysis 7, Analysis 8 explains differences in county capital reversal rates, calculated as the number of reversals at all three stages of review as a proportion of the total number of death verdicts imposed in each of the 967 study counties²⁶⁹ and 23 study years. The total number of combinations of counties and years compared is, again, 3054. Unlike Analysis 7, Analysis 8 gauges the effect of state, as well as county, factors. In recognition of the fact that state and county explanations might operate differently from each other given their different jurisdictional levels, Analysis 8 treats county level factors as random effects and state level factors as fixed effects. In addition, each of the 967 counties is treated as a subject variable nested within the state among the 34 studied where the county is located. This analysis thus assumes that counties in the same state are more like each other than counties in different states. Analysis 8 is a binomial regression study.

iii. Analysis 9: *county-within-state Poisson regression analysis of county and state explanations for county reversal rates at all review stages combined*. Analysis 9 is like Analysis 8, except that it uses Poisson regression analysis.

iv. Analysis 10: *county-within-state Poisson regression analysis of county and state explanations for county reversal rates on direct appeal*. Analysis 10 is like Analysis 8, but studies reversals as a proportion of death verdicts actually reviewed at the single, direct appeal stage. Analysis

10 compares reversal rates for death verdicts in each of 851 counties in each of the 23 years in which at least one death verdict was imposed and was fully reviewed on direct appeal, generating 2472 observations.

v. Analysis 11: *binomial regression analysis using predicted values from state analyses and other county factors to explain county reversal rates at all review stages combined.* Analysis 11 is a binomial regression designed to determine how well state-level factors explain county reversal rates, while also testing county-level explanations for county reversal rates. Analysis 11 uses Analysis 1's binomial analysis of factors related to state-level reversal rates to generate predicted values for each death-sentencing county in each of the 34 study states. The predicted values are the capital error rates the state analysis predicts for each county given the number of death verdicts the county imposed. The predicted values then are examined along with other county-level factors to see which are significantly associated with county reversal rates. The predicted values are derived from 519 observed state reversal rates (34 states times each of the 23 study years in which the state imposed at least one death verdict) and are used along with other county-level factors to explain 3054 observed county reversal rates (967 capital counties times each of the 23 years in which the county imposed at least one death verdict). Reversal rates are the proportion of death verdicts *imposed* in the relevant county that were reversed at one of the three review stages.

There are two ways in which Analysis 11 is a more demanding test than Analyses 7-10 of the explanatory power of specific state and county-level factors:

- In the analysis used to derive predicted values, state and year are treated as random effects. As a result, predicted values are based only on factors that significantly explain variance in reversal rates that is not explained by the state and year in which the verdict was imposed.²⁷⁰
- In the analysis used to identify county-level factors that significantly explain county reversal

rates, the county imposing the death verdict is treated as a random effect. As a result, specific county-level factors (including the predicted values derived from state-level factors) can achieve significance only by reliably explaining variance in county reversal rates that is not explained by the variation among the counties themselves.

vi. *Analysis 12: Poisson regression analysis using predicted values from state analyses and other county factors to explain county reversal rates at all review stages combined.* Analysis 12 is the same as Analysis 11 except that it is a Poisson regression technique, and it uses Analysis 2 (also a Poisson regression analysis) to derive predicted reversal rates for death-sentencing counties based on significant state-level explanations for state reversal rates.

vii. *Analysis 13: county-within-state Poisson regression analysis of county reversal rates at all review stages combined, with county and state explanations as fixed effects.* Analysis 13 is a Poisson regression analysis of 3054 observed county reversal rates for each of 967 counties in the nation that imposed at least one death verdict during the study period in each of the 23 study years in which the county imposed a death verdict. Reversal rates are proportions of *imposed* death verdicts reversed at one of the three review stages in the 23-year study period. By nesting counties in states, this study (like Analyses 8-10) assumes that counties in the same state behave more similarly than counties in different states. Analysis 13 treats potential state- and county-level explanations for county reversal rates as fixed effects and has no random effects.

viii. *Analysis 16: county-within-state binomial regression analysis of county and state explanations for county reversal rates at all review stages combined, averaged over time.* Like Analysis 8, Analysis 16 is a binomial regression analysis of explanations for differences in county reversal rates, in which county-level explanations are treated as random effects and state-level explanations are treated as fixed effects, and in which reversal rates are the proportion of each

county's imposed verdicts that were reversed at one of the three review stages. Unlike in Analysis 8, the county reversal rates being explained in Analysis 16 are each county's total number of reversals during the entire 23-year study period (not the number reversed in each death-sentencing year) divided by its total number of death verdicts. Analysis 16 clusters counties into 34 groups based on the state where each county is located. This technique assumes that counties in the same state behave more similarly than counties in different states. As a result of these steps, Analysis 16 can be understood as examining differences in the 34 study states' experiences with capital reversals during the entire 23-year study period, with each state's capital experience being the composite of its counties' experiences during the period.

By taking a single picture of each state's 23-year experience with capital reversals, Analysis 16 (like state-only Analyses 14-15 above²⁷¹) allows a more direct examination of differences in the 34 states' experiences in the modern capital era. Because Analysis 16 omits time as a consideration, it includes 35 additional counties that imposed death verdicts in the study period but in an unknown year. This increases to 1002 the number of observed county reversal rates being explained.

ix. Analysis 17: county-within-state Poisson regression analysis of county and state explanations for county reversal rates at all review stages combined, averaged over time. Analysis 17 is like Analysis 16, except that it uses a Poisson regression analysis.

x. Analysis 18: Poisson regression analyses of county reversal rates at all review stages combined in Florida, Georgia and Texas. Analysis 18 is comprised of three Poisson analyses—one each for Florida, Georgia and Texas—of county-level factors that explain variation in reversals among each state's death-sentencing counties in each year in which the county imposed at least one death verdict. We chose Florida, Georgia and Texas because they are the three states with the highest

number of death verdicts that underwent at least one stage of review in the 23-year study period and because each imposed death sentences in all 23 study years. County and year are treated as random effects, and county-level explanatory factors are treated as fixed effects.

c. Analysis 19: case-level logistic regression study of factors associated with capital federal habeas reversals.

Analysis 19 supplements the prior analyses' explanations of differences in reversal rates among states and counties with a logistic regression study of differences in the outcomes of particular capital federal habeas cases. Using a data base containing hundreds of items of data we collected on each of the nearly 600 capital cases reviewed on federal habeas between 1973 and 1995, Analysis 19 examines circumstances about defendants, victims, lawyers, judges, court procedures, evidence and timing to see whether they predict the probability that capital verdicts will be reversed at that stage.

4. Eleven inter-related tests of factors' success in explaining variation in rates of serious, reversible error.

a. Three tests applied to individual explanatory factors.

We already have mentioned two tests we apply to any factor or condition that might help explain increases or decreases in the amount of reversible capital error across states and counties or over time: statistical significance and validation by multiple analyses.

i. Statistical significance. The more times we observe how two conditions relate to each other (say, a high number of death verdicts per 1000 homicides and a high probability that any given death verdict is seriously flawed), the more confident we can be that any relationship between the conditions we think we observe is consistent and cannot be explained simply as a chance product of the variation among states or counties. The same is true, the more closely changes in one condition track changes in the other. Statistical significance is a test that uses these criteria—number of observations, clarity

and closeness of the observed relationship—to calculate the degree of confidence we can have that the observed relationship could not have occurred by chance. We follow the usual practice of starting to pay attention if the probability that the observed relationship could have occurred by chance is less than 10%, and using results as bases for conclusions when that probability is less than 5%.

ii. Consistency of significance across analyses. Different statistical analyses assume different things about events being compared across place and time—*e.g.*, that they are rare or common; that they have a yes-or-no form (as is true of the outcomes of capital habeas cases, which are either approved or not) or have a range of possible outcomes (as is true of reversal rates); that the range of possible outcomes is bounded or infinite; and that the regression relationship can be expressed on the logistic or logarithmic scale (based on different expected distributions of the outcomes being explained). Where, as is usually true, the relevant events do not follow any of these patterns perfectly, it helps to use multiple analyses to determine whether factors that in one analysis have a significant relationship to the condition under study reveal the same relationship when a different analysis is used.²⁷² The more ways researchers analyze data, the more confidence they have in results supported by most or all analyses. On the other hand, relationships that are not robust—*i.e.*, that only occasionally appear— are less convincing.

As we note above,²⁷³ we begin by identifying the empirically and theoretically soundest and most comprehensive methods for analyzing our detailed data about state and county capital reversal rates (Analysis 1, discussed at pp. 99-100 above and pp. 146-91 below). Then, to provide a strict test for robustness, and for whether our results reflect actual relationships in the data between significant explanatory factors and capital reversals and reversal rates and are not dependent on particular study methods, we conduct 18 follow-up analyses that:

- **use different statistical techniques with different assumptions and capacities;**
- **vary the power of general factors (e.g., state, time), and specific factors (e.g., homicide rates and death-sentencing patterns) to explain variance;**
- **calculate rates using all possible base numbers (all death verdicts, only those available for review at a particular stage or only those actually reviewed);**
- **examine decisions made at different stages of the review process (direct appeal, post-conviction, federal habeas and all three combined);**
- **consider counties, as well as states, as the possible location of explanatory factors;**
- **treat counties as independent, and as substantially influenced by the relevant state, and treat states as independent, and instead, as collections of counties;**
- **compare states in each relevant year and as composites of all years; and**
- **treat time as the unit for measuring states' and counties' activities, as sporadically important (to see whether particular events have a big effect on the amount of serious error found by the courts), and as continuously important (to see whether the amount of serious error increases or decreases over time).**

For each major set of analyses, we conclude by arraying all outcomes in a summary table showing which factors are usually significantly related to serious error and which are only occasionally related to error.²⁷⁴ We emphasize the former factors in making findings and reform proposals. Sometimes, however, a finding of only one analysis is logically explained by the condition being studied in that, but no other, analysis that distinguishes it from all others. In that case, we note the explanatory factor and the evident reason for its significance in one context but not others.

iii. Consistency of significance within analyses. As we note above, reversal rates and a potentially explanatory factor may have about the same pattern of increases and decreases over place and time and yet not be causally linked. Both conditions may be independently reacting to a third factor.²⁷⁵ To probe relationships that appear to be significant, we test the effect—in each of our

separate analyses—of many different possible explanatory factors and combinations. In eight analyses, we carry the process a step further, simultaneously testing the effect not only of a variety of different factors but also the effect of the *same* factor measured at state and county levels. For instance, homicide rates might have some effect on reversal rates; if so, one may wonder whether local or state homicide rates (or both) have that effect. In those eight studies, we examine both.

Some factors are significantly related to capital reversal rates, whether or not other factors or combinations of them are included in the study. Other factors only sporadically appear to be significant, depending on the other factors being tested. When factors follow the latter pattern, we drop them from consideration. We also drop factors that are never significant. The factors we consider are discussed on pp. 135-40 below and described more fully in Appendix E.

b. Six tests applied to overall sets of explanatory factors.

The tests listed above consider the strength of individual explanatory factors. As we have noted, it is important to examine more than one factor at a time.²⁷⁶ Doing so tests the strength of the relationship between a factor and reversal rates by revealing whether the factor is significant when other factors are also considered. Doing so also reflects the fact that harmful conditions usually have several interrelated explanations—which must be studied together to be understood. We do not stop, therefore, with tests of the consistent significance of individual explanatory factors. Instead, we add four overlapping tests of the strength and consistency of *sets* of explanatory factors.

i. Five within-analysis tests: fit and explained variance compared to a baseline and to other sets of explanations. The first five tests compare each set of explanatory factors to all other sets analyzed *within* a particular analysis (*i.e.*, all others analyzed using the same statistical technique). The set of factors that does best overall on the five tests is the best set of explanations the study or

statistical technique can provide when applied to the available data.

These tests first ask if there is enough variation among places and times to study. If so, they ask additional questions about the overall explanation for reversal rates that the set of factors provides:

- **Does the explanation fit the data better or worse than the explanation provided by the place (state, county) and year in which the relevant death verdict was imposed—*i.e.*, when no other, more specific factors are analyzed?** Lack of fit is gauged by the combined distance between the amounts of reversible error the set of explanatory factors predicts would occur in each place and year, and the amounts actually observed there and then. (If more places and times are studied, lack of fit will be higher, because there are more “distances” between each actually observed value and each predicted value to be added up. As a result, fit can only be compared within, not across, analyses.) When two sets of explanatory factors using the same statistical technique are compared, the set that overall is closer to the actually observed outcomes is better. This fit comparison starts with a baseline analysis of the effect on reversible error of (1) the state where the death verdict was imposed, (2) the year it was imposed and (3) any trend in reversal rates over time. The test asks whether the amount of reversible error predicted by a given set of specific factors is, overall, closer to the observed amount of error than the amount predicted by the baseline analysis of only state, year and the trend of reversal rates over time and, if so, whether the improvement is statistically significant.²⁷⁷
- Using a different statistical measure of a related condition, as explained further in the accompanying note, we ask: **Does the explanation account for more variation among reversal rates across place and time than the explanation given by the baseline analysis of state, time and trend?**²⁷⁸
- **Does the explanation fit the data better or worse than the explanations provided by other sets of specific explanatory factors?** Here, the fit analysis is the same as above but the explanation generated by each set of factors is compared, not to the explanation generated by the baseline inquiry, but to the explanations generated by other sets of specific factors.
- **Does the explanation account for more of the variation across place and time than the explanations provided by other sets of specific factors?**

ii. A cross-analysis test: consistently favorable fit and explained variance. Our confidence in the explanation for reversible error provided by a given set of factors is further enhanced if the various fit and explained variance measures indicate that the group is one of the best overall explanations not only within a particular analysis (*i.e.*, in a comparison of sets of explanatory factors using the same

statistical technique) but also among multiple analyses (*i.e.*, in comparing sets of factors using more than one statistical technique).

c. A tenth test: gauging the size of the effect of each significant explanatory factor, holding other factors constant.

We focus only on statistically significant relationships between serious, reversible error and potentially explanatory factors²⁷⁹—and only then if confidence in their importance is confirmed by the other tests described above. But even if an explanatory condition is significantly related to error rates—meaning an increase in one tends to coincide with increases or decreases in the other—the *size* of the effect may be too small to warrant attention. If, for example, a 500% increase in per capita funding of courts is associated with a 1% decrease in serious capital error, the relationship between funding and error is not interesting, even if the relationship is highly significant (in the sense that it is highly unlikely the relationship could appear by chance).

The regression techniques we use allow researchers to estimate effect size, which answers the following question: Taking into consideration all explanatory factors tested in an analysis, how much of an increase or decrease in reversal rates is expected to occur if a given factor is increased or decreased by a specified amount? The analyses we use generate estimates of the increase or decrease in reversible error associated with each measurable increase or decrease in the explanatory factor.²⁸⁰

i. Effect-size estimates for binomial analyses. About half our analyses are binomial regressions.²⁸¹ Those analyses predict that, for each increase of one unit in the value of an explanatory factor—taking all other factors in the analysis into account—the “odds” that a death verdict imposed in that jurisdiction will be reversed change by a factor of x , where x is the amount of the effect-size estimate reported in our results. If that estimate is greater than 1, the analysis predicts that the odds

of reversal increase as the value of the factor increases; if the estimate is less than 1, the analysis predicts that the odds of reversal decrease as the value of the factor increases. As an example, consider a binomial analysis in which: increases in homicide rates are significantly associated with increases in capital error rates; homicide rates range from 1 to 10 homicides per 100,000 residents in different states and years; and the effect-size estimate for the homicide rate is 1.4. In that event, the analysis predicts that for each increase of 1 in the number of homicides per 100,000 residents, the “odds” that a death verdict will be reversed increase by a factor of 1.4. An example is given in the accompanying endnote.²⁸² The formula differs for explanatory factors whose values have been logged.²⁸³

To help readers interpret effect-size estimates, we often graph the predicted reversal rate associated with each of the range of values for particular explanatory factors that different states in our study have. See, *e.g.*, Figures 22A, 22B, p. 175 below. In graphing the effect size of homicide rates, for example, the range of homicide rates in all study states and years is indicated on the horizontal (x) axis, and the range of possible reversal rates from 0 to 100% is indicated on the vertical (y) axis. For binomial analyses, each point on the line of the graph represents the predicted reversal rate indicated on the vertical axis, for a state and year in which the homicide rate is the value indicated on the horizontal axis—assuming all other explanatory factors are held constant at their average value.²⁸⁴ Another way to say this is that each point on the graph identifies the predicted probability that a death verdict will be reversed, as indicated on the vertical axis, in a state that has the homicide rate indicated on the horizontal axis and otherwise is average in all respects. If a point on the graph corresponds to 4 on the horizontal axis and 49 on the vertical axis, the predicted capital reversal rate for states with 4 homicides per 100,000 residents is 49%. Or, put the other way, the predicted probability that a death verdict will be reversed in a state with 4 homicides per 100,000 residents is

49%, other factors equal.

ii. Effect-size estimates for Poisson analyses. The effect-size estimates our Poisson regression analyses generate are interpreted differently. Here, the analysis predicts that for each increase of one in the explanatory factor—and taking all other factors into account—the *rate* (not the odds) of reversal increases by a factor of x , with x being the effect-size estimate. Assume, again, that an analysis—now a Poisson analysis—finds that increases in homicide rates are significantly associated with increases in capital error rates, where homicide rates range from 1 to 10 homicides per 100,000 residents in different states and years, and where the effect-size estimate for the homicide rate is 1.4. In that event, the analysis predicts that for each increase of 1 in the number of homicides per 100,000 residents, the capital reversal rate will increase by a factor of 1.4. An example is given in the accompanying endnote.²⁸⁵ The formula is different for explanatory factors whose values have been logged.²⁸⁶

Sometimes, we graph Poisson effect sizes. See, e.g., Figures 22C and 22D, p. 175 below. These graphs have a different interpretation from the binomial graphs, as is indicated by the different label of the vertical (y) axis. On these graphs, it is not possible to link a particular value for the relevant explanatory factor as indicated on the horizontal (x) axis (say a particular state's homicide rate) to a particular reversal rate indicated on the vertical axis. Instead, these graphs are interpreted by comparing two points on the horizontal axis and calculating the percent change in the associated points on the vertical axis.²⁸⁷ Suppose the graph indicates that a homicide rate per 100,000 residents of 5, indicated on the horizontal axis, has a corresponding value of .2 on the vertical axis, and a homicide rate per 100,000 residents of 6 (indicated on the horizontal axis) has a corresponding value of .4 on the vertical axis. The graph shows that the predicted reversal rate increases by 100% as the homicide rate per 100,000 residents increases from 5 to 6—assuming other factors hold steady at their

averages.²⁸⁸ Or, one could say that the predicted probability of reversal of any death verdict increases by 100% as the homicide rate per 100,000 residents rises from 5 to 6, holding other factors constant.

d. Analogous tests for case-level analyses.

The diagnostic tests described above apply to binomial and Poisson regression analyses of factors explaining differences in capital reversal rates from one place (state or county) to another. Analysis 19 is instead a logistic regression study of factors associated with decisions reversing, as opposed to approving, death verdicts in 600 federal habeas cases. In presenting Analysis 19 below, we use measures of fit and effect size that are analogous to but not the same as the tests described above. The analogous tests are discussed in connection with Analysis 19 below.²⁸⁹

e. A final test: does the explanation square with common sense and experience?

Statistical studies aid judgment, but they are no substitute for it. Even if explanations for reversible error do well on the tests listed above, they still do not qualify as bases for firm conclusions if they don't jibe with common sense and experience. Even if explanatory factors have a significant, robust and sizeable statistical relationship to serious, reversible error, therefore, we do not rely on that relationship unless we can give a reasoned and practical account of how the two are related.

D. Stages of Analysis and Factors Considered

Our analysis proceeds in stages based on the capital reversals being explained. First, we study factors explaining differences in *state* rates of reversed capital verdicts, starting with our main binomial Analysis 1 and an otherwise similar Poisson analysis (Analysis 2), then conducting several follow-up analyses. Still more follow-up analyses identify explanations for differences in *county* capital reversal rates, starting with explanations operating at the county level, then testing factors

operating at both the state and county level. Finally, using a large data set from all capital federal habeas cases decided between 1973 and 1995, we identify traits of capital cases that increase the probability of federal habeas reversals.

In this section, we explain why state, county and case-level factors, and time, are all worth considering as explanations for serious capital error. Then we describe the range of factors considered. Finally, we discuss our sources of information about those factors.

1. Why study state, county and case-level explanations for serious capital error?

In identifying explanations for reversible capital error, we consider conditions operating (1) throughout each of the **34 states** that imposed and reviewed death verdicts in the study period, (2) in each of about **1000 capital-sentencing counties** in those states and (3) in **nearly 600 capital federal habeas cases** decided in the period. Examining factors at multiple levels is useful for several reasons.

- Some factors operate at only one level and may be missed if only another level is studied.
- Even if a factor operates at all levels, data on it may be available at only one level. For example, the amount spent on individual trials and counties' aggregate spending on courts is generally not known, but figures on *state* spending are available. Conversely, states and counties do not rate the "average aggravation levels" of their homicides, but juries make findings on that subject in each capital trial. To see if court spending and the aggravation level of crimes affects reversal rates, it thus is necessary to study both the state and case level.
- The effect of conditions that operate at one level may be detected at, and misattributed to, another level if only the latter is studied. Error rates may appear to be reacting to county homicide rates, for example, if only county-level effects are studied. But that effect may disappear if the effect of state homicide rates is simultaneously considered and is significant. A way to identify the source and extent of effects that may arise at two levels is to study both.
- Data for entire counties and states are relatively easy to obtain from public agencies and private researchers, but their aggregate character may obscure subtleties. In contrast, case-level data may reveal a lot about each event but are far more difficult and expensive to gather for all cases. Taking both a wide view of aggregate data blanketing entire jurisdictions and periods, and a close look at hundreds of details about a smaller set of cases, achieves the

advantages of both kinds of data.

a. Why study the effect of state conditions?

It is sensible to expect that conditions at the state level might affect capital error rates because

the existence and operation of the death penalty is largely a result of state-level policies:

- State legislators decide whether to adopt a death penalty law giving local prosecutors and juries the authority they would otherwise lack to seek and impose death sentences.
- State legislators define the offenses for which death is a legal punishment, including by listing the aggravating and mitigating circumstances that prosecutors may or must consider, and by stating which individuals are and are not subject to the death penalty (retarded individuals?, juvenile offenders?, co-participants who were not directly involved?, etc.). Some states have broad death-sentencing laws; others have narrow ones. Some states give D.A.s and jurors broad discretion over whether to seek and impose death sentences; others give little discretion.
- The court system that administers the death penalty is by and large a state system. That system uses procedures set by the state constitution and laws, which apply throughout the state.
- The state's single supreme court has the final say on the meaning of the state's death-penalty law. Its rulings bind prosecutors, judges and capital trials all over the state. The law in many states requires that court to assure statewide uniformity in capital practices and outcomes.
- Some states fund most or all costs of prosecuting and defending capital cases at local trials. Statewide public defenders or capital resource centers represent many or all capital defendants and appellants in a minority of states, and the laws of other states govern who may be appointed to represent capital defendants and how much they can be paid. These states may be contrasted to states that require localities to fund all capital litigation, at least for trials, and that leave it to localities to decide whom to appoint to represent capital defendants and how much to pay them.
- In many states, the governor, on behalf of the entire state, nominates judges subject (in most states) to voter approval. In many cases—including in the selection of most state supreme court justices—the electorate to whom state judges answer is statewide, not local.
- State attorneys general set policies that bind or affect decisions by local officials on whether to bring capital cases and how to try them. In most states, the state attorney general defends all capital verdicts on appeal and in state post-conviction and federal habeas proceedings, giving that official considerable power to define acceptable local as well as state capital charging, conviction and sentencing policies, by deciding whether, and how vigorously, local policies will be defended when attacked on appeal.

- Governors and state boards of pardons and parole establish criteria for executive clemency in capital cases that apply throughout the state.
- The public and political processes through which state legislators, judges, attorneys general, governors and other state officials make these decisions create **a common statewide approach to the death penalty** that differentiates each state from every other. **State actions and policies lead officials and jurors throughout the state to use the death penalty differently from actors in other states.**

As a matter of observable fact, moreover, states vary widely in how they use the death penalty, and in how their death verdicts are perceived by reviewing courts—providing further justification for studying possible state-level explanations for serious capital error. To start with, as Figure 11, p. 121 below shows, **states differ greatly in how often they impose death verdicts** per 1000 homicides. **States also differ greatly in how often serious error led to reversal of their death verdicts during the 23-year study period** when their verdicts were reviewed by state supreme courts on direct appeal (see Figure 12, p. 122 below), all state courts combined at both the direct appeal and post-conviction stages (Figure 13, p. 123 below), or federal courts on habeas corpus (Figure 14, p. 124 below).

Figure 11. Death Verdicts per 1000 Homicides, 1973-95⁺

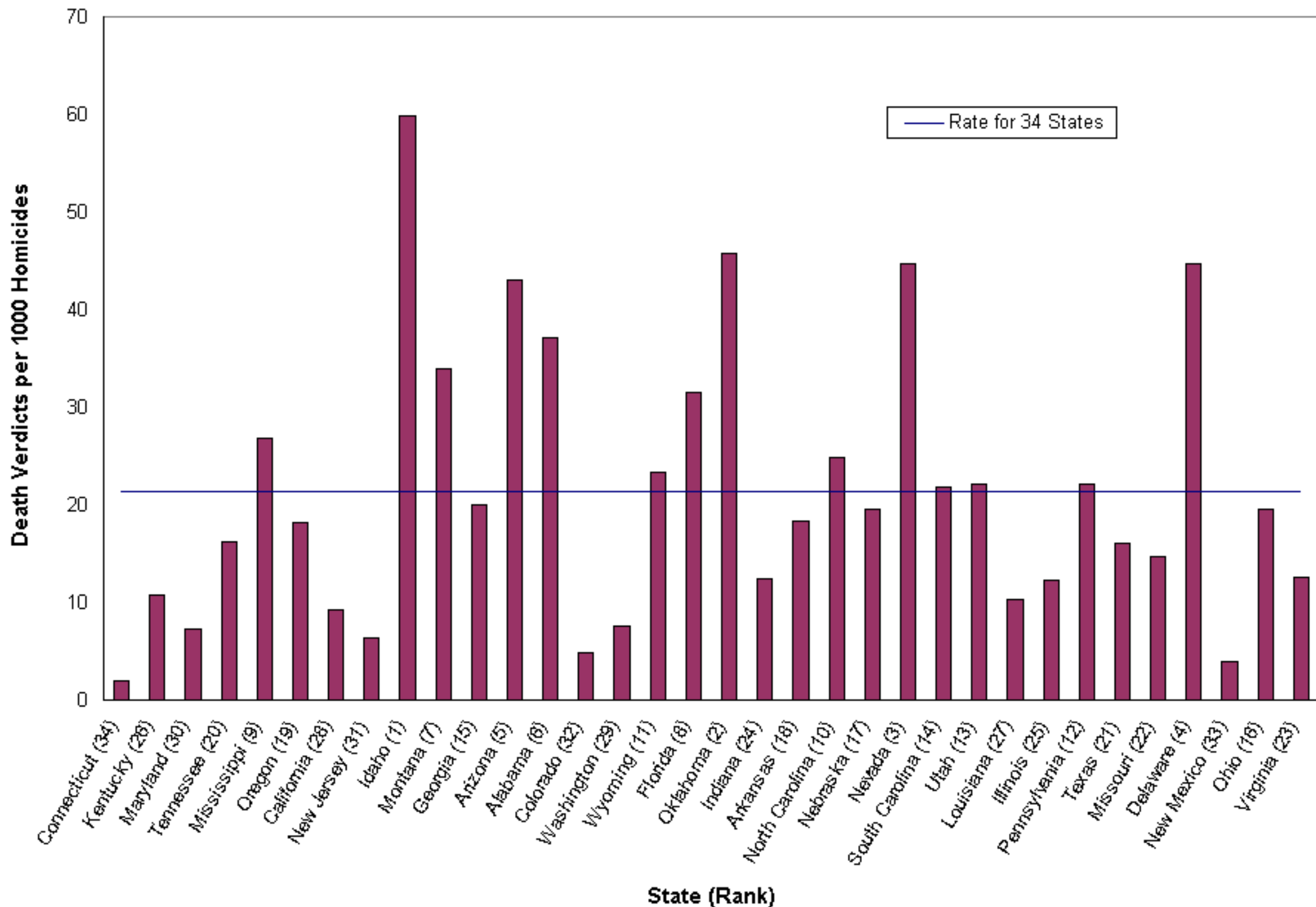
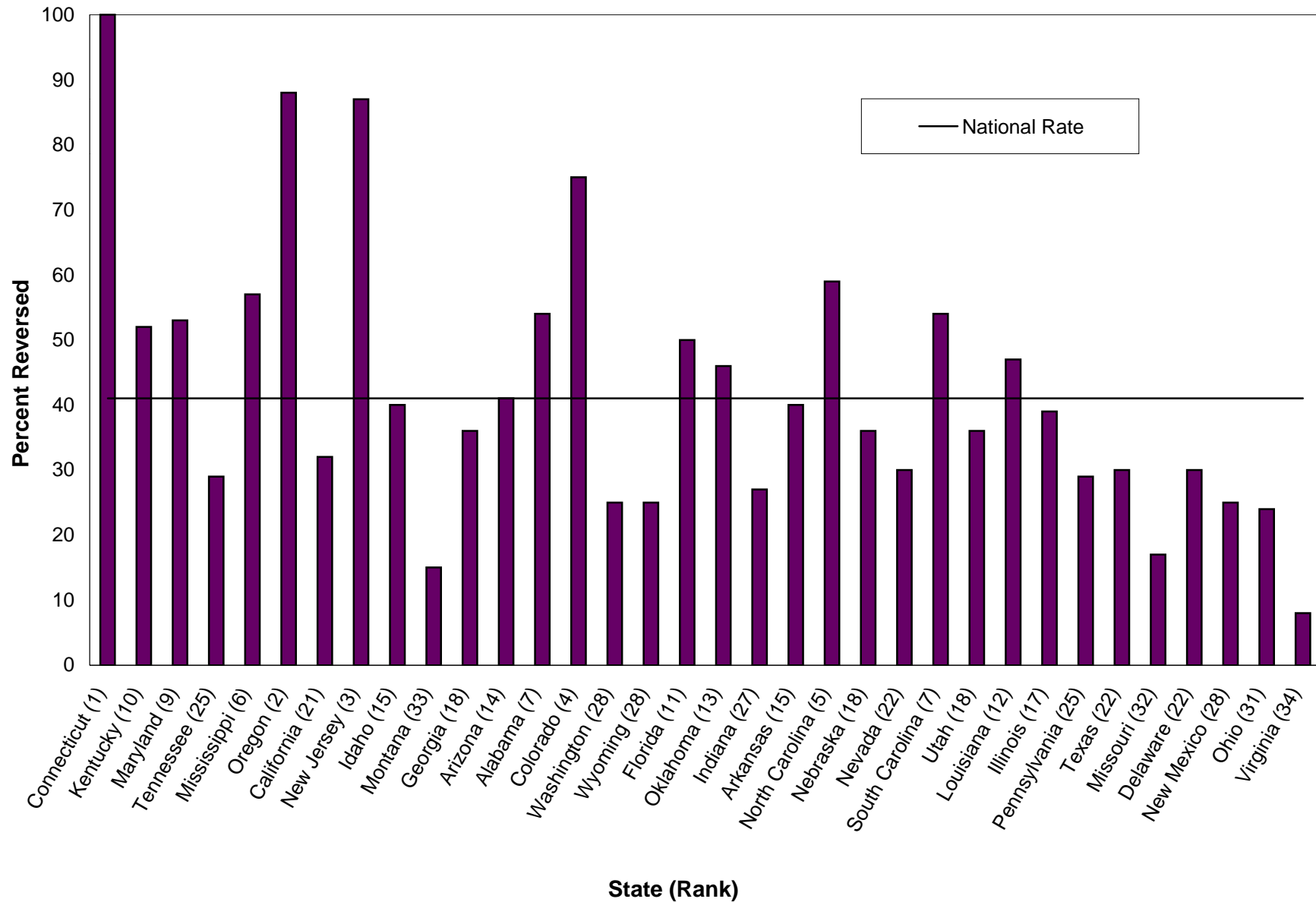
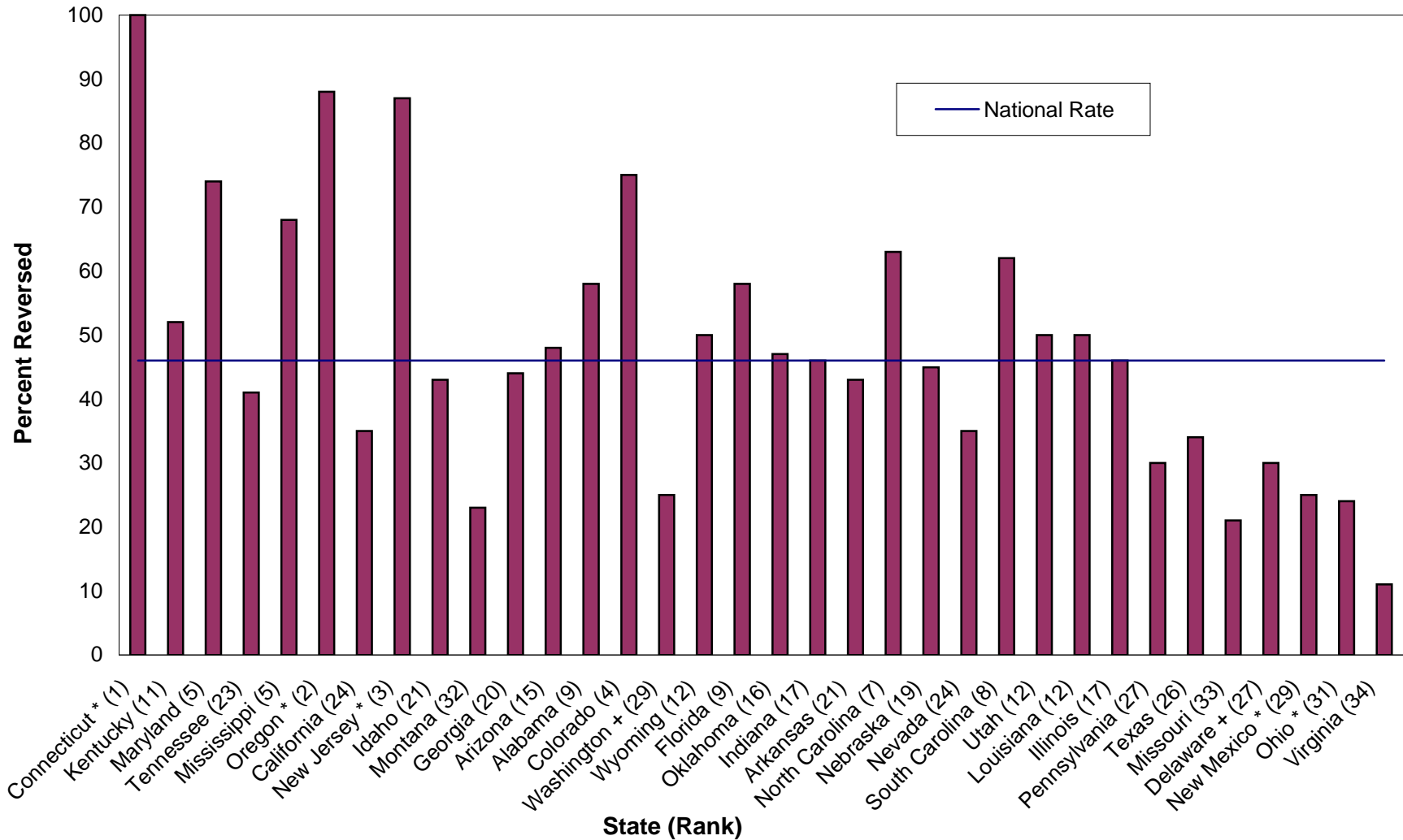


Figure 12. Percent of Death Verdicts Reversed on State Direct Appeal, 1973-95*



* States are listed in descending order of overall reversal rates (see Figure 1B).

Figure 13. Percent of Death Verdicts Reversed on State Direct Appeal or State Post-Conviction, 1973-95#

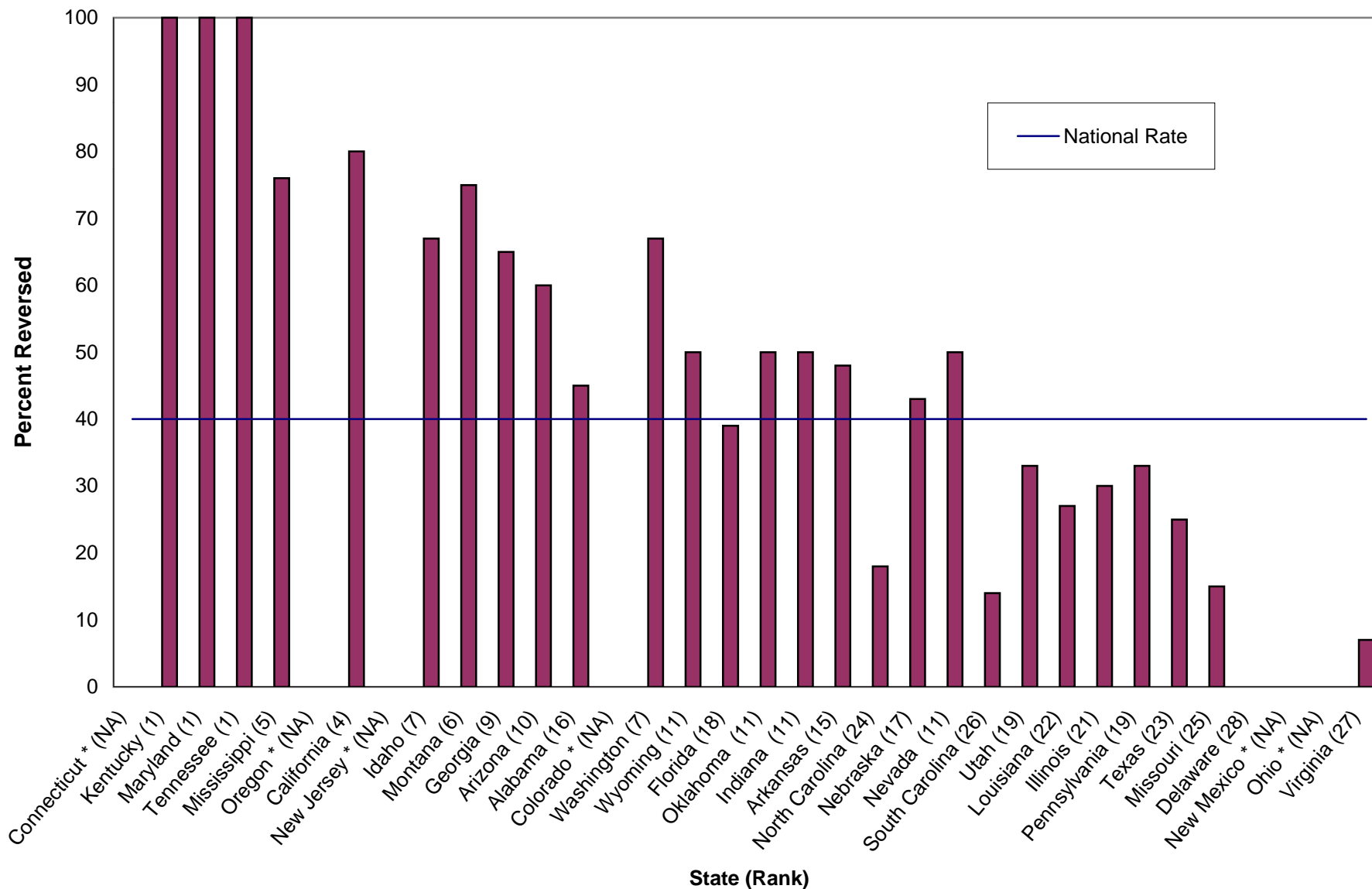


States are listed in descending order of overall reversal rates (see Figure 1B).

* States where capital verdicts were only reviewed at first (state direct appeal) stage.

+ States where number of state post-conviction reversals is unknown.

Figure 14. Percent of Death Row Verdicts Reversed on Federal Habeas Review, 1973-95⁺



+ States are listed in descending order of overall reversal rates (see Figure 1B).

* States where capital verdicts were only reviewed at first (state direct appeal) stage.

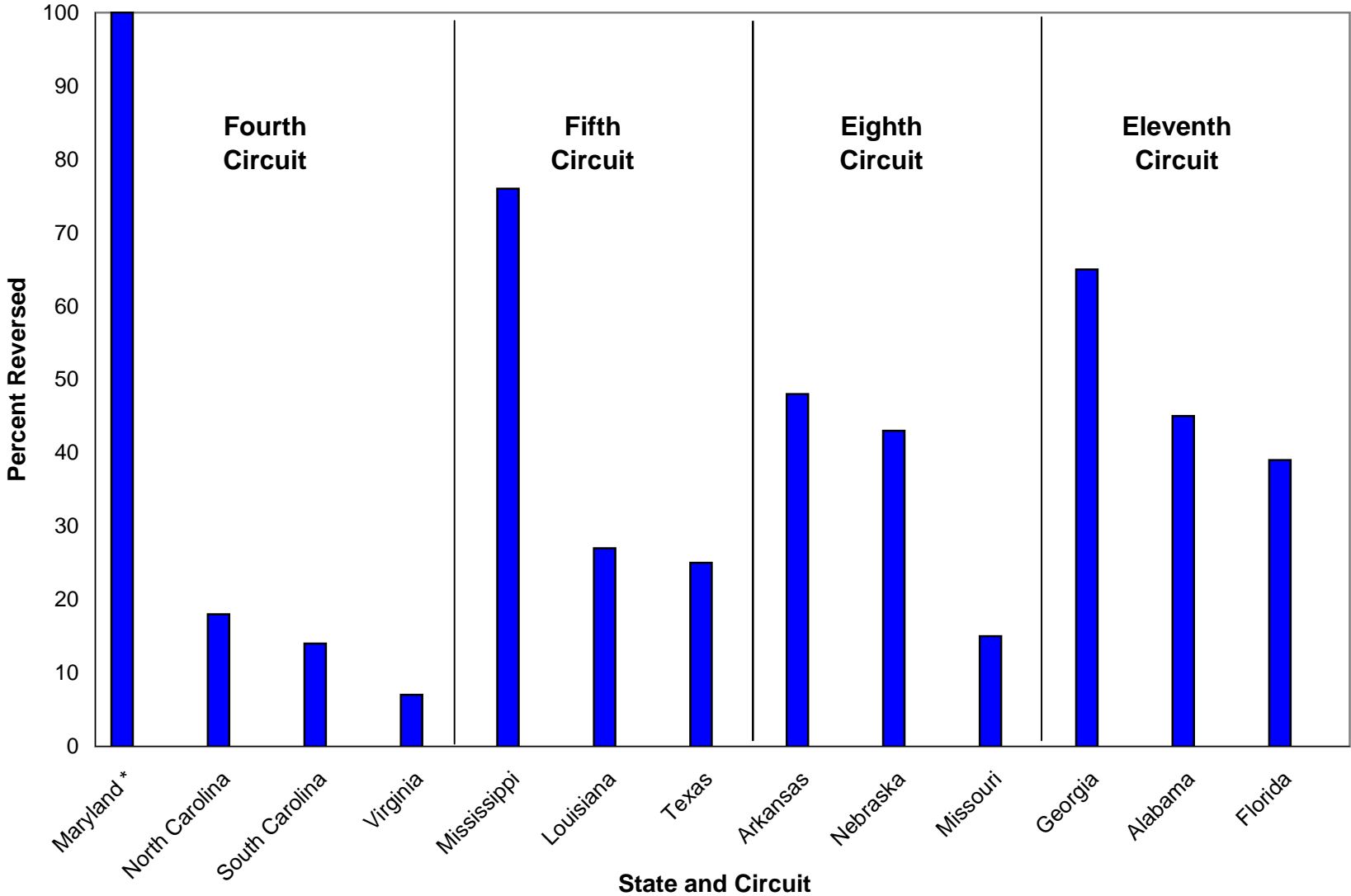
As Figure 15, p. 126 below shows, capital verdicts from different states have different rates of serious error even when the *same* judges inspect them. Federal court of appeals judges are assigned to circuits with responsibility for cases from several adjoining states. Figure 15 compares federal habeas reversal rates for capital verdicts from different states in the same circuit, revealing **large differences in the amount of error the same judges find when reviewing death verdicts from different states**. Figure 15 supports a point made above: **There evidently is something about capital verdicts—and especially about the states that impose them—and not simply something about particular judges that determines how often the judges reverse capital verdicts.**²⁹⁰

When all serious, reversible error found at all stages of review completed by at least one death verdict from each state are combined, the resulting overall error rates again vary dramatically from the top to the bottom states, as Figure 16, p. 127 below, illustrates.²⁹¹ States also vary dramatically in:

- how long it took them to get from a first death verdict to a first non-consensual execution (Figure 17, p. 128 below), and how long it took on average for an imposed death verdict to be carried out (Figure 18, p. 129 below)—roughly gauging how long capital appeals take;
- the size of their backlogs of unreviewed death verdicts (Figure 19, p. 130 below);
- how many people states non-consensually execute for every 1000 homicides—ranging from .02 in California to 4.8 in Delaware (240 times greater) (Figure 20, p. 131 below); and
- the proportion of each state’s death verdicts imposed during the study period that it carried out in the period (Figure 21, p. 132 below).²⁹²

Comparing Figure 11 (p. 120 above) to Figures 20 and 21 (pp. 131-32 below) reveals no evident, positive relationship between states that impose, and ones that carry out, high proportions of death verdicts per homicide. In some cases, the relationship appears to be negative: Arizona, Idaho, Mississippi, Nevada, North Carolina and Pennsylvania are among the most likely to impose death verdicts per homicide and the least likely to carry them out. Louisiana, Missouri, Texas and Virginia imposed relatively few death verdicts per homicide but were among the most likely to carry them out.

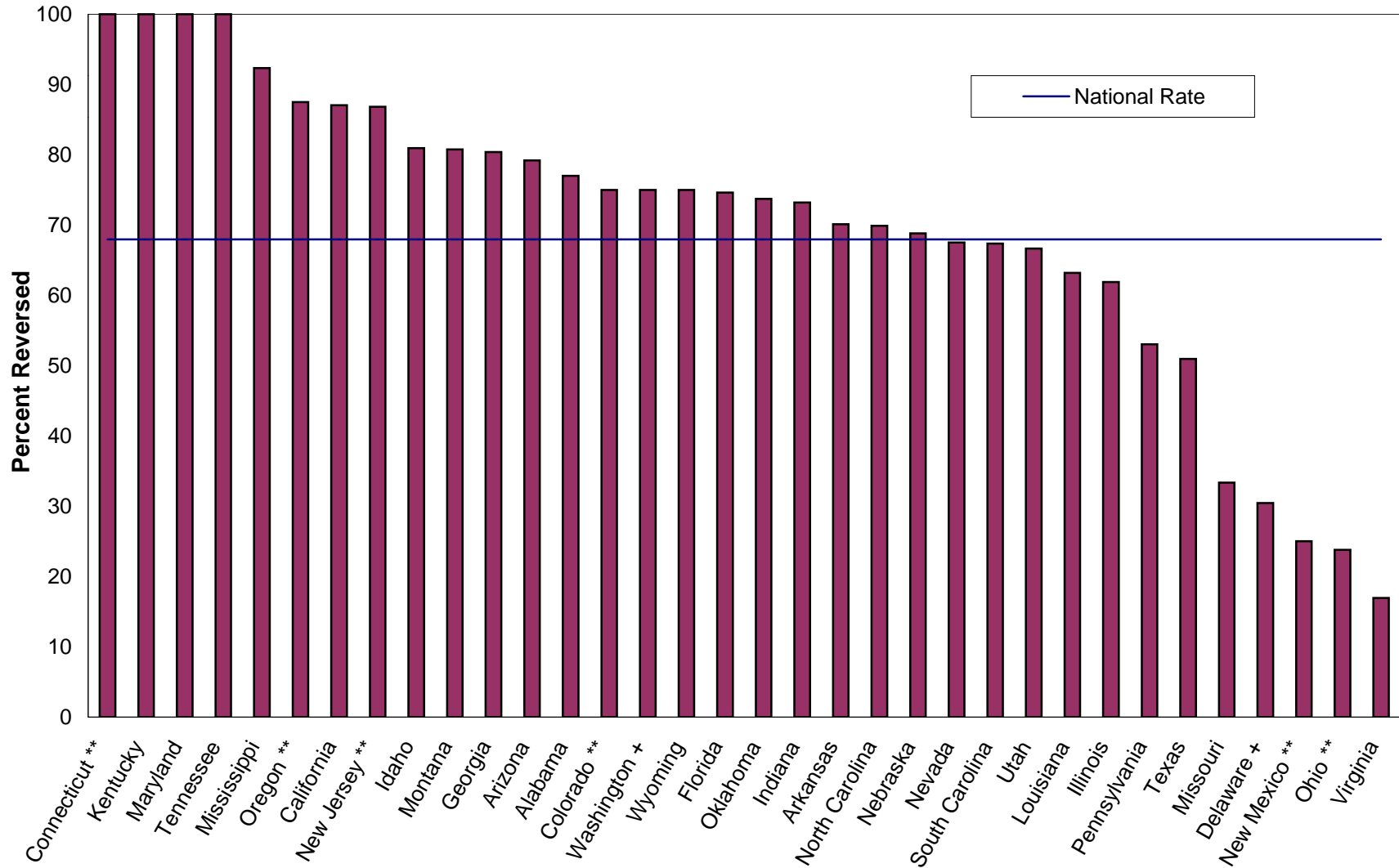
Figure 15. Percent of Death Verdicts Reversed on Federal Habeas by Selected Circuits and Their States, 1973-95⁺



+ The selected circuits decided 88% of all federal habeas cases during the study period.

* Maryland had only three federal habeas cases during the study period.

Figure 16. Combined Reversal Rate for Completed Stages of Review, 1973-95*

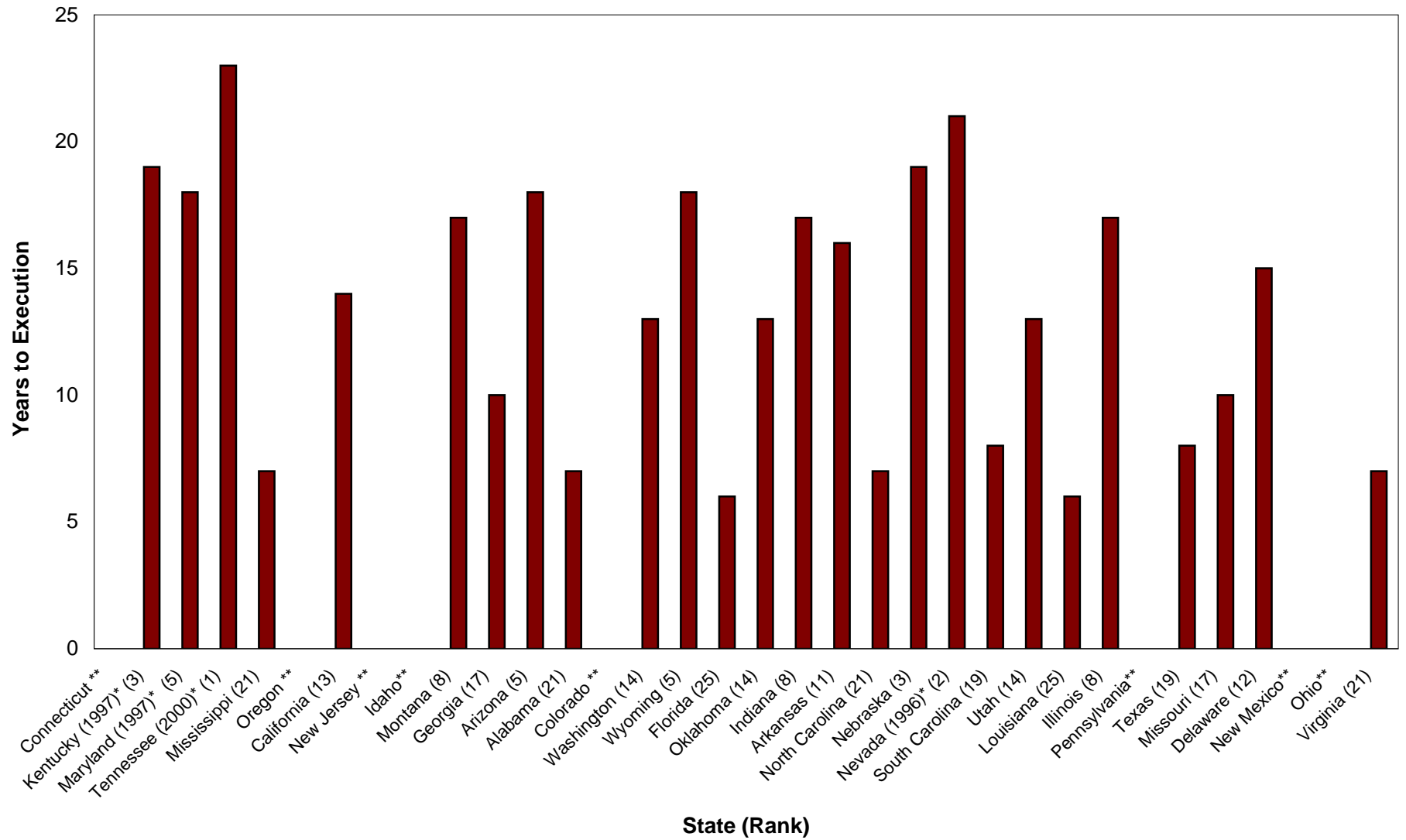


* States are listed in descending order of overall reversal rates.

** States where capital verdicts were only reviewed at first (state direct appeal) stage.

+ States where number of state post-conviction reversals is unknown.

Figure 17. Years from First Death Verdict to First Nonconsensual Execution, 1973-2000⁺

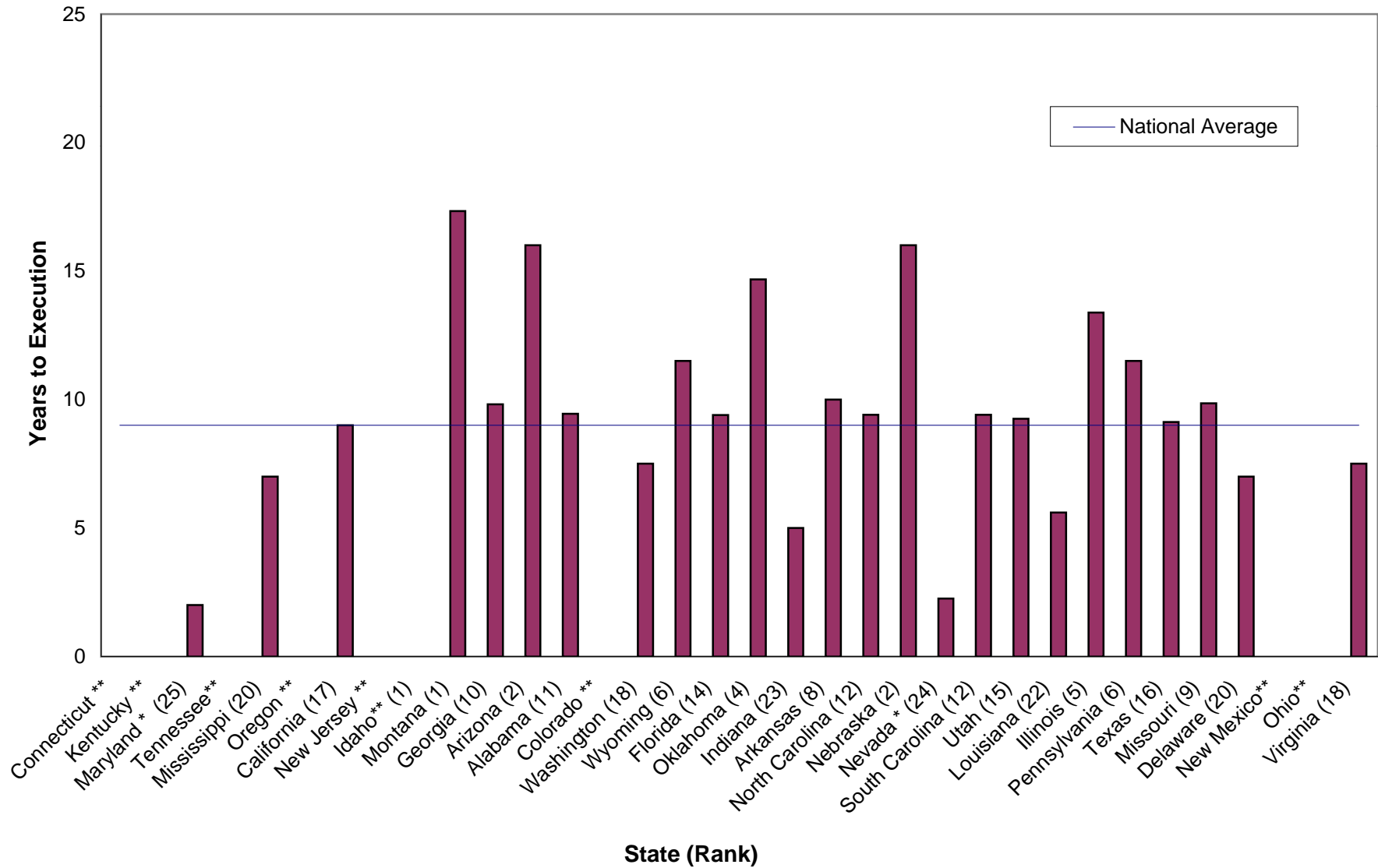


+ States are listed in descending order of overall reversal rates.

* The first nonconsensual execution occurred after 1995, in the year indicated.

** As of June 2000, there had not been a nonconsensual execution.

Figure 18. Average Years from Death Verdict to Execution, 1973-95⁺

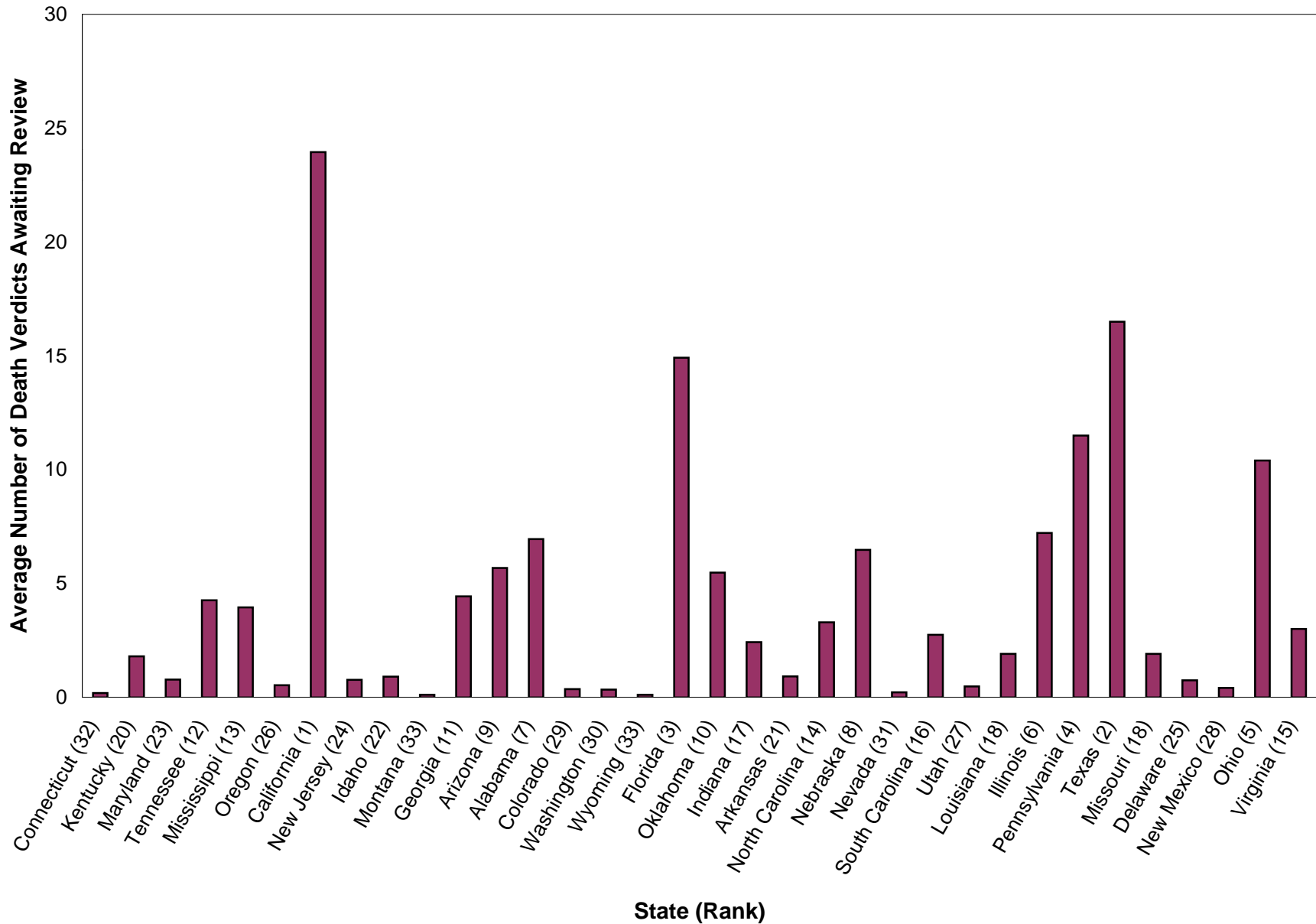


+ States are listed in descending order of overall reversal rates (see Figure 1B).

* States with only consensual executions during the study period.

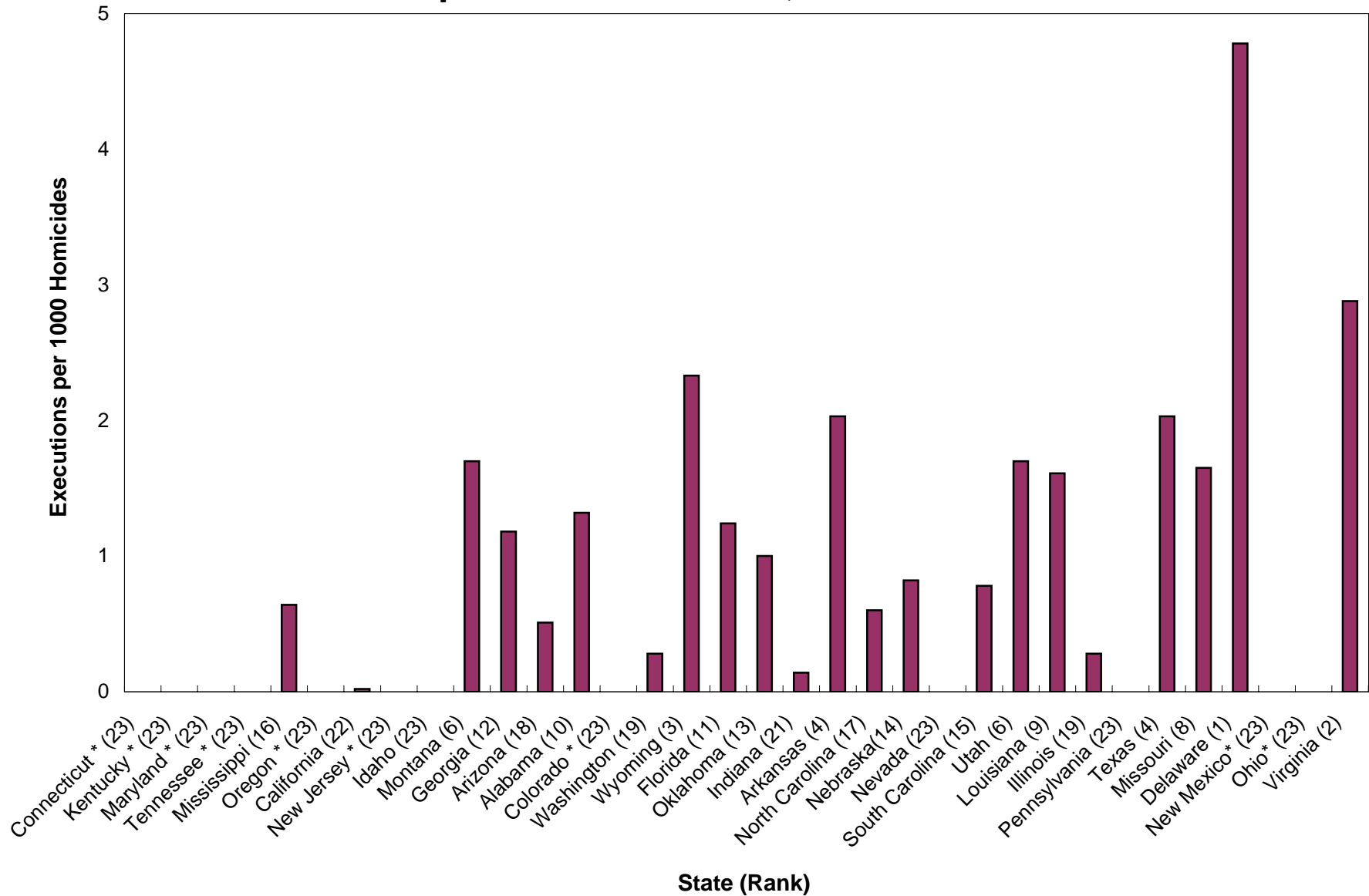
** States with no executions during study period.

Figure 19. Average Number of Death Verdicts Awaiting Review, 1973-95*



* States are listed in descending order of overall reversal rates (see Figure 1B).

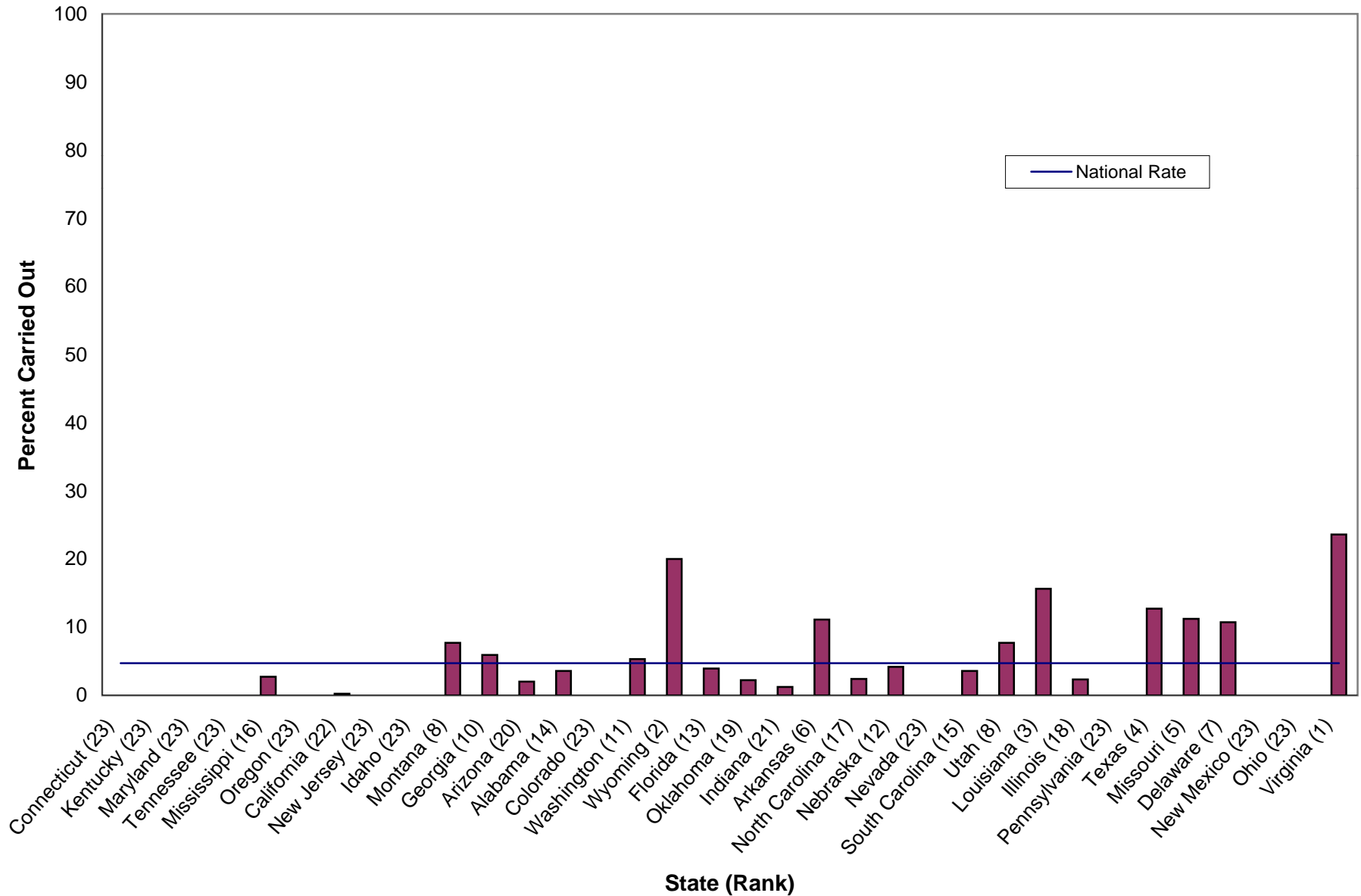
**Figure 20. Nonconsensual Executions
per 1000 Homicides, 1973-95⁺**



+ States are listed in descending order of overall reversal rates (see Figure 1B).

* States with no nonconsensual executions during study period.

**Figure 21. Percent Death Verdicts Carried Out
(Nonconsensual Executions), 1973-95***



* States are listed in descending order of overall reversal rates (see Figure 1B).

b. Why study the effect of county conditions?

Conditions at the **local level** also affect capital sentencing and may affect capital error:

- In some states, local prosecutors are free to decide whether to use the death penalty at all. Even where they lack that power, they usually have broad discretion over whether to charge cases capitally and whether to accept plea bargains that avoid the death penalty.²⁹³
- When state statutes and supreme court decisions are ambiguous, local trial judges must interpret the law as best they can—sometimes defining different views of the law in different places. Local judges often are elected locally, which puts different political pressures on them and their interpretations, rulings and instructions to jurors.
- The quality of the criminal defense bar differs from county to county. So do policies about whom to appoint to represent defendants in states where local officials decide that question.
- The ultimate decision maker in capital cases is the jury (or, in a minority of states, the trial judge). Local conditions may affect how jurors (and trial judges) approach that decision.

States differ in how they define the areas over which each district attorney and trial court has responsibility, and from which potential jurors in capital cases are drawn. In many states, the area of responsibility for each set of decision makers is different. A district attorney may be responsible for prosecuting capital cases in several adjoining judicial districts, while jurors in resulting trials might come from only a subdivision of one of those districts. The relevant areas change over time, and there is no national or even state repository of information about their boundaries. The closest approximation to these areas of responsibility with stable and well known boundaries are counties. Most districts over which district attorneys and local courts have responsibility, and from which jurors are chosen, are defined in relation to county boundaries (very often being coterminous with counties, and, if not, splitting or combining them). Like states, moreover, counties are good bases for comparison because public and private agencies often organize information they collect by county.

States influence counties and are themselves aggregations of counties. In a number of analyses,

therefore, we use analytic techniques that either treat counties as “nested” within states (meaning counties within states are assumed to be more similar to each other than to counties outside the state) or define state conditions as a composite of conditions in all their death-sentencing counties.

c. Why study the effect of case-level conditions?

Above we explain why understanding capital error requires knowing more than the error found in each case.²⁹⁴ We have just explained why identifying state- and county-level factors related to high capital error rates can help fill out our knowledge of conditions associated with error. Additional information can be provided by traits of particular cases—beyond the error found in each case²⁹⁵—that are significantly related to a lower or higher probability that the verdicts will be reversed. As a result, the trial-level conditions appellate judges describe based on the trial transcripts and other records before them, and the circumstances of the appeals, are worth studying for clues to why errors arise in some cases but not others and, indeed, in some counties, states and periods but not others. Studying traits of particular capital cases supplements state and county analyses in a number of important ways:

- Case-level analysis may identify explanations for capital error that cannot be tested at the jurisdictional level, because states and counties don’t differ much in that respect, but cases do. For example, rates of homicide victimization among women may be similar from state to state, making it difficult at that level to study how the victim’s gender affects capital error. But the victim’s gender does vary from case to case, so the factor can be tested at the case level.
- There may be better data about particular conditions at the case as opposed to the jurisdiction level. Not much can be learned about the attributes of appellate lawyers at the county and state level, because no relevant records are kept at those levels. By examining court decisions in each case, however, one can learn the names of the lawyers who represented capital defendants, whether they are private or publicly employed, and where their offices are located.
- As we note below in discussing racial characteristics related to high capital error rates, a case-level analysis can provide important information about factors that *were* effectively tested at the jurisdiction level. The racial makeup of a state’s population may have an important relationship to high capital error rates, though the racial makeup of the state’s death row, or the race of particular defendants or victims, is not significantly related to the probability of

error.²⁹⁶ The same may be true for other traits such as the seriousness of crime: The fact that states with high rates of serious crime such as homicide, or at least high rates affecting influential residents, have high capital error rates does not assure that death verdicts imposed for especially serious homicides are more likely to be reversed. That may be true—and, if so, it says something about how serious crime affects error rates. But it also may not be true, or the opposite may be true—that verdicts for more serious offenses have less capital error. Either of the latter findings would say something else about how the seriousness of a jurisdiction’s crime problem affects capital error rates.

2. Explanatory factors studied.

In the 10 years since this study began, we compiled **the largest data base ever assembled on factors at the state, county and case level that might lead to the occurrence and discovery of reversible error in capital verdicts.** We believe it is the largest data base ever assembled on factors contributing to the existence and discovery of error in any type of judicial proceeding.

Our choice of factors to study was guided by this question: **What types of conditions at the state, county and case level might plausibly affect how much serious error trial actors make in imposing capital verdicts and how much error appellate courts discover when they inspect those verdicts?** To answer this question, we reviewed the literature on capital sentencing and judicial decision making and consulted widely among lawyers, judges, criminologists and other social scientists. We also examined available sources of data on states, counties and court cases, asking in each case if there were plausible reasons to think that the category of available data might provide clues to the causes of reversible capital error. Below we list the categories of explanatory factors we considered in our state and county-level analyses and in our case-level analysis. For a more comprehensive discussion of the explanatory factors we considered, see Appendix E.

a. State and county-level factors.

Table 2 lists categories of state and county factors we considered in our state and county

analyses. Unless indicated, we considered state and county-level factors of each type. With some exceptions, we were able to collect data in each of these categories on all or nearly all states and counties in each of the 23 study years.

**Table 2: Categories of Factors Considered in Analyses
Explaining Differences in State and County Capital Reversal Rates**

- I. Capital Judgements and Appeals
 - A. Number and Rates (per homicide) of Capital Verdicts
 - B. Number and Results of Direct Appeals
 - C. Number, Results and Retrial Outcomes of State Post-Conviction Proceedings
 - D. Number and Results of Federal Habeas Review
 - E. Number and Rates of Undecided Appeals
 - F. Presence or Absence of Death Penalty Resource Center (state only)
 - G. Post-Reversal Capital Re prosecution Rates
 - H. Index of Timing and Frequency of Use of Death Penalty

- II. Death Row Population (state only)
 - A. Race (including relative to state population)
 - B. Race of Victim (including relative to state population)

- III. Functioning of the Judicial System Generally (state only)
 - A. Caseloads
 - B. Dispositions
 - C. Backlogs
 - D. Expenditures

- IV. Political Pressure on the Judiciary (state only)
 - A. Selections and Retention Methods
 - B. Party and Ideological Influences

- V. State Demographic Characteristics
 - A. Population
 - B. Race
 - C. Population Density and Urbanization
 - D. Median Age

**Table 2 (cont'd): Categories of Factors Considered in Analyses
Explaining Differences in State and County Capital Reversal Rates**

- VI. Crime and Victimization
 - A. Crime Rates (all crimes, homicides, violent crimes and FBI Index Crimes)
 - B. Victims (racial composition in general and relative to population)
 - C. Incarceration (rates and new admissions)

- VII. Political, Ideological and Religious Characteristics (state only)
 - A. Relative Strength of Two Major Political Parties
 - B. Religious Affiliation of Population
 - C. Extent of Use of Criminal Sanctions

- VII. Social Characteristics
 - A. Poverty Rates
 - B. Welfare Recipients and Expenditures
 - C. Income Distribution
 - D. Unemployment
 - E. Divorce Rates

b. Case-level factors.

Table 3 below lists the traits of federal habeas cases examined by [Analysis 19](#) to see if they predict when reversible error is and is not found at that review stage. We could not collect every type of information for every case because most of our data come from published state and federal court decisions reviewing death verdicts, and because reviewing judges do not always know, and their decisions do not always discuss, every trait of the crime, defendant, victim and lawyers in the case, or those of the judges who decided the case.

**Table 3: Categories of Factors Considered in Analysis 19
Explaining Outcomes of Capital Federal Habeas Cases**

- I. Sentencing State and County
 - A. Offense
 - B. Trial

- II. Timing
 - A. Offense
 - B. Arrest
 - C. Conviction
 - D. Death Sentence
 - E. Filing of Each Level of Appeal
 - F. Decisions at Each Level of Review
 - G. Execution

- III. Offense Characteristics
 - A. Offense Charged and Convicted
 - B. Location
 - C. Circumstances
 - D. Weapons Used
 - E. Accompanying Offenses
 - F. Accomplices and Disposition of Their Cases
 - G. Evidence
 - H. Defendant's Level of Participation
 - I. Aggravating Circumstances
 - J. Mitigating Circumstances

- IV. Defendant Characteristics
 - A. Age
 - B. Race
 - C. Gender
 - D. Mental Status
 - E. Child and Sexual Abuse
 - F. Drug and Alcohol Use
 - G. Economic Status
 - H. Employment History
 - I. Criminal Record
 - J. Connection to Community
 - K. Other Aggravating Circumstances
 - L. Other Mitigating Circumstances

**Table 3 (cont'd): Categories of Factors Considered in Analysis 19
Explaining Outcomes of Capital Federal Habeas Cases**

- V. Victim Characteristics
 - A. Number
 - B. Age
 - C. Race
 - D. Gender
 - E. Traits Indicating Vulnerability
 - F. Relationship to Defendant
 - G. Economic and Social Status
 - H. Connection to Community
 - I. Method of Death, Wounds, Suffering

- VI. Defense and State Lawyers at Trial and on Appeal
 - A. Public or Private (for defense lawyers)
 - B. Appointed or Retained (for defense lawyers)
 - C. Whether or Not Employed by Capital Case Resource Center (for defense lawyers)
 - D. Out of State or In State (for defense lawyers)
 - E. Employer
 - F. Location
 - G. Local or State Responsibility (state lawyer)
 - H. Years of Experience
 - I. Education
 - J. Specialty

- VII. Judges
 - A. Trial Judge
 - B. State Direct Appeal Judges
 - C. State Post-Conviction Judges

 - D. Federal Habeas Judges
 - 1. District or Circuit
 - 2. Name and Party of Appointing President
 - 3. Education
 - 4. Service as Prosecutor or Other Government Official
 - 5. Years on Bench
 - 6. Size and Composition of Panels and Majority
 - 7. Caseloads

**Table 3 (cont'd): Categories of Factors Considered in Analysis 19
Explaining Outcomes of Capital Federal Habeas Cases**

VIII. Procedural History

A. Trial Procedures

1. Length of Trial
2. Guilt Determination by Trial or Plea
3. Jury or Judge Sentencing

B. State Appellate and Post-Conviction Procedures

1. Votes and Outcomes
2. Was Evidentiary Hearing Requested, Held

C. Federal Procedures

1. Was Evidentiary Hearing Requested, Held
2. Lower Court Outcomes
3. Appeals

IX. Legal Claims and Defenses

- A. Number and Types of Claims Raised by Defendant, and Court's Response
- B. Number and Types of Objections to Relief by State, and Court's Response

c. Time.

Time is a potentially important factor considered in all our analyses except four—Analyses 14-17, which explain differences in states' and counties' reversal experiences as a 23-year whole. Studying time may identify particular years in which important events affected reversal rates (*e.g.*, major elections or court decisions) or may suggest that reversal rates have steadily improved or worsened over time. But, as we note above, the bearing of time may be difficult to determine.²⁹⁷ For one thing, time may be a stand-in for other factors that change over time. For example, court backlogs may increase over time. If backlogs affect reversal rates but are not separately studied, the passage of

time will be given credit for that effect. This explains why the effect of time may diminish from a baseline analysis in which only the effect of state and time is considered to analyses in which other, more specific factors are considered.

The effect of time may also be distorted in studies of ongoing processes, where outcomes of interest almost inevitably continue to occur after the study cut-off date. We already have identified two effects of the time-limited nature of our study, which together lead changes in reversal rates over time to be a better indicator of the influence of *delay* in reviewing death verdicts than of changes in the amount of capital *error*:

- The condition most of our analyses explain is the proportion of death verdicts *imposed* in a given year that later court review found to be seriously flawed. Because the three-stage review process takes time—12 years, on average, as of the last year of our study—it is inevitable that fewer verdicts imposed in later study years will have been reversed by the study’s cut-off date than is true of verdicts imposed in earlier study years, because many fewer verdicts will have completed review by then. This creates an artificial downward trend in the most recent years in reversals as a proportion of imposed verdicts, not because there are fewer errors in later death verdicts but because fewer were reviewed for error by the time the study ended.²⁹⁸
- At the federal habeas stage, seriously flawed death verdicts take more time to be finally reviewed and reversed than do unflawed verdicts.²⁹⁹ Inevitably, therefore, a study cut-off date at a point when some verdicts imposed in the study period were not yet fully reviewed assures that the verdicts whose review outcomes we *do* count—ones finally reviewed *before* the cut-off date—include a disproportionately high number of the unflawed death verdicts, while verdicts whose review outcomes we don’t count—because they were still under review as of the cut-off date—include a disproportionately high number of the flawed verdicts. Thus, **a cut-off date automatically understates the actual rate of error by causing more flawed than unflawed verdicts to go uncounted.** Because this problem is especially acute in years with large numbers of verdicts yet to be reviewed as of the cut off date, and because—as Table 4 below shows—the later the study year, the larger the number of verdicts yet to be reviewed as of the cut-off date, **the bias against counting seriously flawed verdicts is especially a bias against counting seriously flawed verdicts imposed *later* in the study period.**

Table 4: Percent of Death Verdicts Still Under Review at Some Review Stage as of the Study Cut-off Date (Sources: DRCen, DADB, SPCDB, HCDB)

Year of Verdict	Tot. Number of Verdicts	Percent Still Under Review
1973-1978	694	14%
1979-1981	704	23%
1982-1985	1297	45%
1986-1987	665	57%
1988-1991	1087	66%
1992-1993	537	87%
1994-1995	426	99%

For these reasons:

- Where, after taking other factors into account, we find that the passage of time increases rates of reversible error (as is true for death verdicts reviewed at the state direct appeal stage of court review), we almost certainly have understated that effect.
- Where, after accounting for other factors, we find that the passage of time is not significant (as is true in some of analyses of county reversal rates at all three stages), this may be because our use of a cut-off date has artificially depressed reversal rates for verdicts imposed in later years, neutralizing what otherwise would appear to be an increase in reversal rates over time.
- Where, after accounting for other factors, we find that reversal rates decrease over time, and where we have measured reversal rates as a proportion of imposed death verdicts—and in all studies of the federal habeas stage of review, where the second effect discussed above occurs—the most we can say is that the actual effect of time is unknown. This is because a drop in reversal rates over time is at least partly the effect of the successively lower number of more recent verdicts that are finally reviewed and thus available to be reversed (or affirmed). Overall, as we note above, the inclusion of time trend in studies such as the ones described here serves to control for the effect on reversal rates of *unfinished and delayed appeals*, and is of little or no use as a measure of changing *error* rates over time.³⁰⁰

E. Sources of Data

Three data bases original to this study were assembled by the authors from information in court decisions available mainly through the Westlaw and Lexis legal search engines:

- Direct Appeal Data Base (DADB). This data base contains information on state direct appeals of capital verdicts imposed in all years during the 1973-1995 period in which the relevant state

had a valid post-*Furman* capital statute. The 4546 appeals in this data base include all those that we identified as having been finally decided during the 1973 to 1995 period. Data on each appeal include year of death verdict; year of decision; outcome of decision (whether the verdict was affirmed or reversed); subsequent judicial history on rehearing in the state system and certiorari to the U. S. Supreme Court; and citation. The methods used to identify capital cases that were decided on direct appeal and at other stages of review is described at pp. 14-16 above. This information is used in regression Analyses 1-4, 7-18 of factors associated with differences in state and county capital reversal rates.

- State Post-Conviction Data Base (SPCDB), reproduced in major part in Appendix C to this Report). Appendix C and SPCDB contain a list of capital verdicts that were imposed during years when the relevant state had a valid post-*Furman* capital statute and were *reversed* on state post-conviction review between 1973 and April 2000. For each reversal, information was collected about year of decision; outcome of decision; basis for reversal; stage of trial affected by reversal; outcome on retrial; timing; and citation. A fuller description of the manner in which these data were collected is in Appendix C, pp. C-1 to C-2. In addition, SPCDB includes the county in which the death verdict was imposed and the year of the death verdict. This information is used in regression Analyses 1, 1R, 2, 5, 7-9, 11-18 of factors associated with differences in state and county capital reversal rates.
- Habeas Corpus Data Base (HCDB). This data base contains information on the 598 final decisions of initial (non-successive) capital federal habeas corpus cases between 1973 and 1995 that reviewed death verdicts imposed during years in the 1973 to 1995 period in which the relevant state had a valid post-*Furman* capital statute. Included are hundreds of items of information (insofar as it was available in the court decisions reviewed for each case) in each of the nine categories and approximately 75 subcategories listed in Table 3, pp. 138-40 above. This information is used in regression Analyses 1, 1R, 2, 6-9, 11-18 of factors associated with differences in state and county capital reversal rates. It also is used in Analysis 19, a regression study of factors associated with the probability of affirmance or reversal of capital verdicts on federal habeas review.

Our Death Row Census Data Base (DRCen) is a compilation of death verdicts imposed between 1973 and 1995 organized by state and by the name of the defendant who was sentenced to death. We assembled this data base starting with the information used to produce the NAACP Legal Defense Fund's quarterly death row census, *Death Row U.S.A.*. We expanded that information to include every death verdict we could identify that was imposed in any year during the study period in which the relevant state had a valid post-*Furman* capital statute, and to include information on

sentence year, county of death verdict, county census designations, and other information. Data include name of all individuals on a state death row between 1973 and 1995; state and county in which death verdict was imposed; year death verdict was imposed. *Death Row U.S.A.* is also our source of information about executions—when and where they occurred and whether they were consensual or non-consensual. This information is used in all 19 of our regression analyses.

State and county reversal rates—the condition studied in regression Analyses 1-18—are calculated based on:

- the number of death verdicts that were imposed in each state and county during the study period (see DRcen), or that were reviewed at each of the relevant capital review stages (DADB, HCDB), or that were available for review at the state post-conviction stage because they had completed direct appeal review and been affirmed (DADB); and
- the number of death verdicts from the relevant state or county that were reversed at each of the three review stages or at all three of them (DADB, SPCDB, HCDB).

State and county characteristics—the factors operating at the state level that are examined by regression Analyses 1-6, 8-17, and the factors operating at the county level that are examined by regression Analyses 7-18, to determine whether they are associated with differences in state and county capital reversal rates—are from the following public sources:

- State population and racial composition are from the United States Census (USCen).
- Crime data are from the FBI Uniform Crime Reports (UCRDB).
- State and county homicide and homicide victimization data, including by race, are from the Vital Statistics of the United States or other data sources generated by the Centers for Disease Control and Prevention National Center for Health Statistics (HomVic).
- Annual state prison population data are from the Source Book of Criminal Justice Statistics (PrisCen).
- General court caseload data are from State Court Statistics (CtCaLd).

- Court expenditure data are from Expenditure and Employment Data for the Criminal Justice System (CtExpen).
- Welfare recipients and expenditures are from The Statistical Almanac of the United States (FacWelf).
- Other county-level data are from a data set created by Professor Steven F. Messner and his colleagues at the University of Albany and the University of Illinois, as described at p. 170 & n.371 below. We are extremely grateful for the use of these data.

Our indexes of the political pressure on state judges from judicial selection techniques are original to this study and based on provisions of the 34 study states' constitutions and codes governing judicial selection, supplemented by information from the National Center for State Courts (PolPres).³⁰¹

F. Format for Reporting Results

The text and notes to this Report discuss the results of our 19 analyses. For each separate analysis in each study, we show the amount of unexplained variance left by the baseline analysis compared to that of each analysis of more specific factors; the fit of the baseline analysis compared to that of each analysis for specific factors; factors that were significantly associated with reversal rates; the factors' significance levels; and the factors' effect size (sometimes given numerically, other times displayed in graphs). Appendix E defines the explanatory factors included in one or more of our analyses, and Appendix F contains correlation matrices of all those factors as well as the minimum, mean and maximum values and standard deviations for each factor. A table with the detailed results of each analysis is collected in Appendix G.

IV. State-Level Factors Related to Serious, Reversible Error in Capital Trials and Verdicts: Aggressive Use of the Death Penalty, Ineffective Law Enforcement, Politics and Race

A. Differences in How States Use the Death Penalty and How Often Their Verdicts Are Reversed

As we point out in discussing Figures 11-21, pp. 118-32 above, our data reveal wide variation among states on many aspects of death sentencing, including how often their death verdicts are reversed due to serious error. Statistical analysis can sometimes identify factors that explain such differences across place and time. Those factors might include the other differences among states noted above—*e.g.*, in how often they impose death verdicts and how often verdicts get bogged down in the courts—or a variety of other conditions. We turn now to the results of the regression analyses we designed to identify factors that are significantly related to capital reversal rates.

B. Results of Main Analysis 1 and Analysis 2 of Reversal Rates at All Review Stages Combined: Aggressive Death Sentencing, Ineffective Law Enforcement, Politics and Race

1. Significant differences across state and time permit analysis of factors related to rates of serious, reversible error.

Main Analysis 1 and our first follow-up analysis (Analysis 2) consider differences in reversal rates as a proportion of death verdicts imposed by each of the 34 states that used the death penalty in the 23-year period between 1973 and 1995 in each of the years when the state imposed at least one death verdict. Each year in which a particular state imposed one or more death verdicts is an observation—because we can observe the reversal rate, over the remainder of the study period, for death verdicts imposed by the state in that year. If a state imposed death verdicts in 18 of the 23 study years, it provides 18 observations, or observed rates of reversible error. All told, the 34 states contributed 519 observed error rates to Analyses 1 and 2. That rate, in these analyses, is the proportion

of the death verdicts imposed in the relevant state and year that, during the rest of the 23-year study period, were reversed at one or another of the three stages of review—direct appeal in the state supreme court, state post-conviction review in a state trial or appellate court or federal habeas review.

As we note above, Main Analysis 1 and Analysis 2 differ in their assumptions about patterns of capital reversal rates.³⁰² Analysis 1 uses binomial logistic regression to explain differences among rates that range fairly evenly from 0% to 100%. Analysis 2 uses Poisson regression to explain differences among rates that clump towards the low end of the spectrum. As we will see, the results are nearly the same no matter which assumption is made.

In our first test of the usefulness of these analyses,³⁰³ we conduct a baseline inquiry to see how much of the variation in reversal rates among states and years is explained by three general traits of each of the 519 observed reversal rates—(1) the state in which the death verdicts subject to review were imposed, (2) the year in which they were imposed and (3) the general trend of reversal rates over the 23-year period. The second trait identifies changes in reversal rates that are associated with particular years; the third trait identifies any linear trend in those rates over all years.³⁰⁴

State, year and time trend almost certainly appear to explain differences in reversal rates that in fact are related to more specific conditions that happen to vary from state to state, year to year or over time. Even so, we proceed with confidence to search for those specific explanations only if state, year and trend are *weak* proxies for more specific explanations for error rates, leaving significant differences to be accounted for. The baseline inquiries for Analyses 1 and 2³⁰⁵ reveal that even after state, year and time trend are considered, a significant amount of variance in reversal rates remains among states (as indicated by statistically significant random intercepts, or variation in the state coefficients³⁰⁶), and among years (as indicated by statistically significant slopes, or variation in the

year coefficients³⁰⁷).

2. Main Analysis 1 and Analysis 2 identify sets of factors that explain differences in reversal rates and fit the data better than the baseline analyses and other sets of factors.

Having found that differences in reversal rates across state and time are significant and are not fully explained by the state and year in which the death verdicts were imposed or the trend of reversals over time, we put Analyses 1 and 2 to four more tests: Whether, based on what is known about the capital system, as well as experience and common sense, we can identify specific explanatory factors that, individually, (1) are significantly related to reversal rates; and that, as a group, (2) do a better job than the baseline analysis of matching the actual reversal rates to those the explanatory factors predict; (3) explain more of the differences across state and time than the baseline analysis; and (4) provide a better explanation than other sets of explanatory factors (again, given their fit with the data and ability to explain differences across state and time).

a. Statistical significance.

Analyses 1 and 2—the former using a binomial regression analysis, the latter using a Poisson regression—identify similar sets of explanatory factors that are significantly related to reversal rates. In conducting each analysis, we identified two overlapping but not identical sets of factors that are significantly related to reversal rates—referred to here as Analyses 1A, 1B, 2A and 2B. By significantly related, we mean that:

- even when all explanatory conditions are considered at once, **changes in each condition track changes reversal rates from state to state and year to year**—*e.g.*, when the condition increases in amount or intensity, reversal rates also tend to increase; and
- **it is unlikely that this congruence between changes in each explanatory condition and changes in capital reversal rates could occur by chance, based on typical state-to-state and year-to-year variation.**³⁰⁸

Before discussing the significant explanations for reversal rates identified by Analyses 1 and 2, we ask whether, beyond each individual condition's statistically significant relationship to reversal rates, there are reasons for confidence in the overall set of explanatory factors the analyses identify.

b. Improved fit.

Our first basis for confidence in the overall explanation Analyses 1 and 2 supply is that each set of factors they generate—Analysis 1A, 1B, 2A and 2B—fits the data significantly better than the explanation provided by the two analyses' baseline inquiries. Fit is measured by the combined distance between each of the 519 actual reversal rates being studied and the reversal rates the set of explanatory factors predicts for each state and year. The higher the measurement, the poorer the fit.³⁰⁹ Fit helps us compare different sets of explanatory factors within the *same* analysis—each using the same type of regression to explain the same real world condition (here, the 519 particular reversal rates)—to see which provides the closest match to the known reversal rates. We first compare the baseline analysis to each set of specific factors to see if the explanation for reversal rates the set provides fits the 519 reversal rates by state and year better than the baseline analysis. Then we compare each set of factors to all others to see which matches the actual outcomes the best.

Fit measurements change from one type of regression analysis and one topic of study to another and are sensitive to the number of data points being explained. They thus are not assessed on an absolute scale and cannot be directly compared across analyses. A 3000 fit score in one analysis, using one type of regression analysis to study one set of reversal rates, is not necessarily worse than a 2000 score in another analysis, using another type of regression analysis or analyzing a different set of error rates. But if the degree and significance of any improvement in fit as one moves from a

baseline analysis to a “best” analysis of multiple factors is greater than is true of another analysis, that may suggest that the former analysis is a better method of explaining the differences being studied (here, variation in capital reversal rates) than the latter.

We first ask whether the groups of specific explanatory factors identified by Analyses 1 and 2 fit the data better than the baseline inquiry,³¹⁰ in that they predict reversal rates closer to the actual ones than do analyses based solely on the state and year in which the death verdicts being studied were imposed and the trend of reversals over time. **For Analyses 1A, 1B, 2A and 2B, the improvement in fit over the Analysis 1 and 2 baseline inquiries is highly statistically significant.**³¹¹

c. Less unexplained variance.

As we note above, the Analysis 1 and 2 baseline analyses of state, year and time trend leave significant unexplained variation in reversal rates from state to state and year to year.³¹² We next consider whether the sets of specific explanatory factors identified by Analyses 1A, 1B, 2A and 2B leave *less* unexplained variation in reversal rates than both the Analysis 1 and 2 baseline inquiries and other sets of potentially explanatory factors. Statistical analysis is not expected to eliminate all significant unexplained variance. The goal is to eliminate as much as possible.

Table 5 compares the coefficients indicating the amount of variance left unexplained by the baseline inquiries for Analyses 1 and 2 to those indicating the amounts of unexplained variance left by Analyses 1A-2B. Smaller coefficients are better, indicating that less variance remains unexplained. Significance levels are in parentheses.³¹³

Table 5. Analyses 1 and 2: Coefficients Indicating Amount of Unexplained Variance, Baselines vs. Analyses 1A, 1B, 2A, 2B (less is better; significance levels are in parentheses)

	<u>variance from state to state</u>	<u>variance from year to year</u>
<u>Analysis 1</u> (binomial)		
<i>Baseline Analysis</i>	.52 (.0006)	.29 (.01)
<u>Analysis 1A</u>	.14 (.01)	.14 (.03)
<u>Analysis 1B</u>	.15 (.01)	.15 (.02)
<u>Analysis 2</u> (Poisson)		
<i>Baseline Analysis</i>	.14 (.0006)	.30 (.009)
<u>Analysis 2A</u>	.030 (.02)	.22 (.01)
<u>Analysis 2B</u>	.033 (.03)	.21 (.01)

As Table 5 reveals, **Analyses 1A, 1B, 2A and 2B** achieve the desired drop in the amount of unexplained variance that had remained after accounting for only state, year and time trend:

- Analyses 1A and 1B diminish by nearly 75% the amount of unexplained state-to-state variance left by the baseline inquiry, and by about 50% the unexplained year-to-year variance.
- Analyses 2A and 2B decrease unexplained state-by-state variance by nearly 80% and year-to-year variance by about 25%.

d. Better combination of significance, fit and explained variance than other groups of conditions.

Tables 2 and 3, pp. 136-40 above, list the categories of potentially explanatory factors we considered in studying state and county reversal rates, including in Analyses 1 and 2. Appendix E more fully defines the specific factor we discuss in the findings below. **The groups of explanatory factors in Analyses 1A, 1B, 2A and 2B improved fit and diminished unexplained variance more successfully than other groups of factors we considered.** The next section discusses the explanations for capital reversal rates identified by Analyses 1A-2B.

3. Factors that as a group do the best job of explaining differences in serious, reversible error across state and time.

a. Non-error-related explanations for low rates of review and reversal: court delay and year of death verdict.

Analyses 1 and 2 compare the rates at which death verdicts imposed in given states and years between 1973 and 1995 were reversed at one of the three phases of state and federal court review during the period. As we note above,³¹⁴ there are two reasons why reversals as a proportion of imposed death verdicts—the reversal rates studied in Analyses 1 and 2—might be lower than otherwise:

- The death verdicts imposed in particular states and years were reviewed at one or more of the three phases of court review and found to be free of reversible error.
- Or, the verdicts were *not* reviewed at any of those phases, and thus could not have been reversed, no matter how erroneous they were. In fact, about 54% of the death verdicts imposed during the 1973-1995 study period were *not* fully reviewed at all three review stages by the end of the period, either (1) because they were imposed too late to make it through some or all of the review process by the end of the period, or (2) because court congestion delayed final review until after the period ended.³¹⁵

These two distinct reasons for lower or higher reversal rates require that we include factors in our analysis that control for the effect of:

- *the time required for verdicts to finish being reviewed by the courts in the normal course of appeals—i.e.*, factors that account for verdicts that, although flawed by reversible error, were not reversed during the study period because they were imposed relatively late in the period and thus were still moving through the review process when the study period ended; and
- *court delay—i.e.*, factors that account for verdicts that, although marred by serious error, were not reversed because court delay impeded their progress through the review process. (Unlike the first phenomenon, this one can affect all, not simply more recent, death verdicts.)

Controlling for these *non-error-related* influences on reversal rates lets us link other significant factors to the presence or absence of serious *error*.

As we discuss above,³¹⁶ we use the year death verdicts were imposed to control for the non-

error-related effect on reversal rates of unfinished appeals. Here, we use three factors to control for the similar effect of court delay: the number of death verdicts awaiting review³¹⁷ and two measures of court cases of all kinds awaiting decision. As predicted, these delay-related factors are negatively associated with rates of imposed verdicts that were reversed by the end of the study period. In other words, they effectively measure the downward effect of unfinished and delayed review on reversal rates calculated as a proportion of imposed verdicts. We discuss delay first, then unfinished appeals.

i. Court delay in reviewing capital verdicts. To control for the effect of delay in the review process, we first used a count of death verdicts imposed in each year by each state that had not been finally reviewed in the three-stage appeals process by the end of the study period. As predicted, this non-error-related explanatory factor was highly significant and negatively related to reversal rates in all four analyses (1A, 1B, 2A and 2B).³¹⁸ **States and years with more death verdicts backlogged in the courts awaiting review had fewer verdicts reversed as a proportion of imposed verdicts.**

This does not suggest delaying review of death verdicts in order to avoid reversals. States and years with many backlogged, and thus unreviewed, death verdicts had fewer verdicts *affirmed* as well as fewer *reversed*, because fewer were *reviewed*. Recall that Analyses 1 and 2 measure reversals as a proportion of imposed death verdicts. This creates three possible outcomes: the verdict was reversed; the verdict was affirmed; or the verdict was still under review and so was neither reversed nor affirmed. Because of this last option, a drop in reversal rates does not necessarily indicate a rise in affirmance rates. If the number of delayed verdicts rises, affirmance as well as reversal rates will both fall. **If the goal is to carry out legally appropriate death verdicts while overturning unreliable ones, delaying review of all verdicts is the worst outcome.**

It is also probably true, however, that **less serious, reversible error in death verdicts almost**

certainly will mean fewer delays on appeal. This increases the importance, in conducting our analyses, of controlling for the non-error-related effects of court delay and time, so we can be relatively confident that other conditions related to capital reversals are also related to capital *error*.

ii. Delays in all court cases. Although delays in capital appeals certainly keep death verdicts from being reviewed, artificially depressing reversal rates as a proportion of imposed verdicts, it also may be that delays in court cases of all kinds independently impede the progress of death verdicts through their appeals, with the same artificial effect on reversals. To test that possibility, we used two alternative measures of court congestion. In Analyses 1A, 1B, and 2A, we combined four individual indicators of court congestion—civil, criminal, felony and all cases filed and awaiting decision by state courts as a proportion of the state’s population—into a single aggregate measure of court congestion.³¹⁹ In Analysis 2B, we used one of the four individual indicators—the total number of filed cases awaiting decision by state courts as a proportion of the state’s population.³²⁰

As predicted, states with high rates of court congestion tend to have lower rates of death verdicts making it through the courts and being reversed, even after controlling for the effect of delays in capital appeals themselves.³²¹ In Analyses 1A, 1B and 2A, the combined (four-part) measure of each state’s rate of general court congestion was negatively related to reversals and significant at the .05 level, or at just slightly above that level.³²² In Analysis 2B, the single measure of rates of court congestion (total cases awaiting decision per the state’s population) was also negatively and significantly related to reversal rates.³²³

iii. Year of death verdict. It takes time for courts to discover flaws in death verdicts: By the end of the study period, the average federal habeas reversal was occurring 13 years after the flawed death verdict was imposed.³²⁴ As a result, the later a death verdict was imposed, the less likely it is

that the verdict was fully reviewed—and thus that flaws in it had been discovered—as of the study’s cut-off date. More recent verdicts thus will automatically have lower rates of reversal within the study period than earlier-imposed verdicts, not because later verdicts are less error-prone, but because later verdicts had less time to be screened for error.³²⁵

Additionally, reversals take longer on average than affirmances at the federal habeas stage. As a result, a disproportionate number of verdicts yet to be reviewed on federal habeas as of the study cut-off date are flawed. The effect is to artificially depress the reversal rate among all verdicts, but especially among *more recent* verdicts, because a higher proportion of them had yet to be reviewed on federal habeas as of the cut-off date than is true of earlier verdicts.³²⁶

In both the baseline inquiries and Analyses 1A-2B of specific factors, we included the year death verdicts were imposed.³²⁷ Doing so controls for the non-error-related effect of unfinished appeals, which are more probable in proportion to how recently the verdict was imposed and which, as we have just seen, depress review rates and thus reversal rates as a proportion of imposed verdicts. As predicted, the later a verdict was imposed—meaning the less time the verdict had to be reviewed for error by the study end date—the less likely it was that the verdict was reversed during the period.

Comparing the effect of time in the Analysis 1 and 2 baseline inquiries and in Analyses 1A, 1B, 2A and 2B of specific factors tends to confirm our judgment that the effect of time in the latter analyses is mainly the dampening effect of delay on *reversal* rates, not the effect of the passage of time in lowering *error* rates. Recall that we included the passage of time in both our baseline analyses and our analyses of more specific factors. Recall also that in the baseline analyses, the passage of time probably serves as a stand-in for more specific conditions that tend to increase or decrease in intensity over time. One thus expects the effect of the passage of time (the linear time trend) to diminish as

specific factors are added to the analysis—ideally leaving only the non-error-related effect of the study cut-off date to be captured by the time factor. As predicted, the size of the effect of the time trend dropped substantially from the baseline analysis to each of our four analyses of specific factors.³²⁸ This tends to suggest (as do the results of Analyses 3, 4 and 10 below³²⁹) that **the effect of the passage of time is mainly the *non-error-related* effect of the study cut-off date —decreased levels of *review over time*—not the effect of higher quality death verdicts over time.**

* * * * *

The above efforts to isolate the non-error-related effects on reversal rates of court delay and unfinished appeals prepare us to explore the effect of conditions, discussed below, that logic and experience suggest are primarily associated with the amount of serious error. All those factors were significantly associated with reversal rates, improved the fit between predicted and actual outcomes, and reduced unexplained variance.

b. Racial factors related to higher capital-error rates.

The effect of race is commonly studied in American criminological research. In the death penalty context, studies suggest that the race of the defendant and especially that of the victim influences the decision whether a death sentence will be *imposed* for any given homicide—and particularly for homicides in the medium range of aggravation, where neither a life sentence nor a death sentence is the obviously appropriate punishment under law.³³⁰ What has not been studied is whether the racial makeup of the *population generally* and of the *class of all homicide victims* (as opposed to the race of particular defendants and victims) is associated with rates of capital *error* (as opposed to rates of capital-sentencing). Analyses 1 and 2 fill that void, finding that the racial makeup of both the state’s population and its pool of homicide victims—and also the combination of the

two—is associated with high rates of capital error.

To be clear, we are not attempting to interpret our regression results on demographics causally. Rather, we are attempting to maximize the amount of information we can derive from a study of reversal rates in the 34 states that actively used the death penalty during the study period. With that finite number of states to study, it is useful to compare them in a number of ways that are strongly suggested by prior research, to get an idea of the factors associated with high capital reversal rates.

i. Higher proportion of African-Americans in the state population. Controlling for other factors, **states with higher proportions of African-Americans in their population tend to have higher rates of capital error.**³³¹ **The relationship is highly significant** in binomial Analyses 1A and 1B, and Poisson Analyses 2A and 2B.³³²

This result deserves attention. A threshold question is, The race of whom? Is it the proportion of blacks in the *population generally* that is related to higher capital error rates, as Analyses 1 and 2 suggest? Or, is the crucial factor the proportion of African-Americans among the *people the state sentences to die*? In other words, do states that sentence African-Americans to die more often than other states have more serious error? Or, alternatively, is the crucial factor the race of the *actual defendant in each case*? Each of these three relationships might masquerade as the other in a statistical study, and it is important to sort out their effects, because each has different implications.

To test for the middle possibility—that there is a relationship between rates of reversible error and the racial makeup of the people states sentence to die or the race of their victims—we conducted subsidiary Analysis 1R. Analysis 1R uses Analysis 1A as a starting point and tests a number of additional factors relating to the race of individuals sentenced to die during the study period, and the race of the victims of the offenses for which the penalty was imposed. Although we tested a number

of such factors (listed in the Appendix E), none improved the fit of the predicted and actual results achieved by Analysis 1A³³³ nor appreciably diminished the amount of unexplained variance. **Nor was the percentage of African-Americans among states' death row populations significantly related to error rates. Nor did adding factors based on the racial makeup of death row populations undermine the statistical significance of the percentage of African-Americans in the state's general population.** In other words, including the racial characteristics of people sentenced to die did not improve the explanatory power of Analysis 1A itself.³³⁴

We next consider whether the race of the capital defendant in a particular case—as opposed to the racial makeup of either the state's general population or its death row—is related to the probability of error. Table 6, p.159 below, compares reversal rates—at the state direct appeal, federal habeas and both stages combined³³⁵—for death verdicts imposed on African-American defendants to reversal rates for verdicts imposed on white defendants. (The table also compares reversal rates for death verdicts involving black versus white victims, and for death verdicts involving various combinations of defendants and victims by race. For now, though, we are concerned only with the race of the defendant.) As the table reveals, there is little or no, much less a statistically significant disparity in reversal rates for death verdicts imposed on black and white defendants: At both stages and the two combined, reversal rates for death verdicts imposed on black and white defendants are identical. As our case-level study below finds, moreover, the race of the defendant has no significant relationship to reversal rates at the federal habeas stage, even when other factors are considered.³³⁶

Table 6: Percent of Verdicts Reversed at State Direct Appeal and Federal Habeas Stage by Race of Defendant and Victim

Race of: Defendant/Victim*	% Reversed at State Direct Appeal Stage	% Reversed at Federal Habeas Stage	% Reversed at Both Stages
White/White	41% of 1757	42% of 281	41% of 2038
Black/White	43% of 945	42% of 193	43% of 1138
Other/White	33% of 155	33% of 18	33% of 173
White/Non-White	45% of 404	39% of 23	45% of 427
Black/Non-White	40% of 760	33% of 70	40% of 830
Other/Non-White	44% of 154	34% of 11	43% of 165
White/All	42% of 2161	41% of 304	42% of 2465
Black/All	42% of 1705	40% of 263	42% of 1968
All/White	42% of 2857	41% of 492	41% of 3349
All/Non-White	42% of 1318	34% of 104	42% of 1422

* In multiple-victim cases, if at least one victim is white, then the victim is classified as white. Sources: DADB, HCDB.

Our finding above stands. Race and capital reversal rates are significantly related, with the key factor being the racial makeup of the state’s population. Controlling for other factors, **states with higher African-American populations relative to the total population have significantly higher rates of serious error than states with smaller African-American populations.** Before interpreting this relationship, we consider two other significant racial effects based on the victims of homicide.

ii. Relatively high risk of homicide to whites as compared to blacks. Research suggests that the race of the victims of homicides affects whether those crimes are punished with death.³³⁷ This has led observers to conclude that homicides of white victims put more pressure on prosecutors, judges and jurors to charge and sentence capitally than do homicides of black victims.³³⁸

These results prompted us to consider whether high rates of homicides against whites are also significantly related to high rates of reversible capital error. One measure of how often whites are

victims of homicide is the number of white homicide victims per 100,000 white residents in each relevant state and year—the “white homicide victimization rate.” But in states with large proportions of white residents, this rate is essentially the state’s *overall* homicide rate—the number of homicides per 100,000 residents of the state. And with minor exceptions noted below, **neither homicide rates generally, nor the white victimization rate by itself, are significantly associated with rates of reversible error in our analyses.**³³⁹

A different measure is needed to determine the extent to which the threat of homicide is concentrated on—*i.e.*, is experienced with particular force by—the white community. This led us to devise an explanatory factor that compares the number of white homicide victims per 100,000 white population to the number of African-American homicide victims per 100,000 black population:

$$\text{rate of white homicide victimization} \div \text{rate of black homicide victimization}.$$
³⁴⁰

Because the homicide risk to members of the black community is usually higher than the homicide risk to members of the white community, this factor in effect compares states and years based on how closely the homicide risk to whites approaches the homicide risk to blacks.³⁴¹

In Analyses 1 and 2, the more heavily the risk of homicide is concentrated on a state’s white community compared to its black community, the higher the state’s rate of reversible capital error.³⁴²

As with the percent of each state’s population that is African-American,³⁴³ we examined the race of homicide victims to see whether the effect we found was from the racial makeup of the state’s overall pool of homicide victims (as our results thus far would suggest), or from the race of the victims of people sentenced to death in the state (Are states that sentence more people to death for killing whites than for killing blacks more prone to capital error?), or from the race of the victims in

individual capital cases (Are capital verdicts for homicides against white victims more likely to be reversed than verdicts for homicides against other victims?)

Our results here are similar to those in the preceding section. Analysis 1R, compares states on:

- the proportion of homicides against white victims that actually resulted in death verdicts;
- that proportion relative to the proportion of the state's homicides committed against whites; and
- a factor composed of both these conditions.³⁴⁴

These conditions are only intermittently significant; when significant, they are sometimes positively and sometimes negatively related to reversible rates; and they do not improve the capacity of the analysis to fit the data and decrease unexplained variance. And in each case, adding these factors increased the significance of our original race-of-victim factor which compares state rates of white and black homicide victimization. Analysis 1R thus gives no basis for confidence that the percentage of white-victim homicides that result in death verdicts has any relationship to reversal rates, but it does support our finding that **states where whites face a higher risk of homicide relative to the risk faced by blacks have significantly higher rates of serious capital error.**

Table 6, at p. 159 above, further supports the conclusion that it is a state's overall proportion of white versus black homicide victims, and not some other category of white and black homicide victims, that is associated with high rates of reversible capital error. The table shows that **the fact that the victims of particular homicides are white does not appear to be related to reversal rates for death verdicts imposed for those homicides.** At the state direct appeal and the federal habeas stage combined, and at the direct appeal stage itself, the reversal rates for white-victim and non-white-victim homicides are identical, or nearly so. At the federal habeas stage alone, death verdicts imposed

for homicides against white victims do have a higher reversal rate (42%) than capital verdicts imposed for homicides against nonwhites (34%). But our Analysis 19 below finds that this relationship is not statistically significant when other factors are considered.³⁴⁵

iii. The interaction of a larger African-American population and a relatively high risk of homicide to whites as compared to blacks. Both the proportion of African-Americans in a state's population and the extent of the homicide risk to the state's white as compared to its black population are racial factors. But is there reason to think these two significant racial explanations for error rates are related, or are part of a single explanation for error rates? To help answer this question, we asked whether the interaction of the two factors is significantly related to rates of serious capital error. Where both racial conditions are high, are error rates especially high, and where both are low, are error rates especially low? More specifically, if we multiply the values for each of the two conditions, is the resulting value significantly associated with capital error rates, above and beyond the effect of the two factors themselves?

Analyses 1, 1R and 2 reveal that the interaction of the percent of a state's population that is black and the state's white homicide victimization rate relative to its black homicide victimization rate is significantly associated with rates of reversible capital error.³⁴⁶ **States with both a high proportion of blacks in their population and a high concentration of homicides of whites relative to blacks tend to have especially high rates of reversible capital error—above and beyond the positive effect on error rates of each component of that interaction and other significant effects.**³⁴⁷

Again, it is the **racial characteristics of the state's overall population and pool of homicide victims** that has this effect, not the racial characteristics of the state's death row population and their victims, nor the race of individual capital defendants and victims. No analogous effect was found in

Analysis 1R for the interaction of the race of offenders who were sentenced to die and the race of the victims of their offenses—although controlling for such factors did slightly increase the significance of the relationship between capital error rates and the interaction between the percent of the state’s population that is black and the state’s rate of white, relative to black, homicide victimization. And, as Table 6, p.159 above, reveals, death verdicts imposed on black defendants for homicides against white victims are not noticeably more or less likely to be reversed than other death verdicts.

iv. Preliminary interpretation: race-based pressures to punish homicides with death. A final interpretation of our results, including that capital error rates are significantly associated with the percent of the state’s population that is black, the state’s rate of white compared to black homicide victimization and the interaction of the two, must await a full presentation of all the results. A preliminary interpretation is set out here.

Three possible explanations for the relationship between capital error rates and the size of a state’s African-American population must be rejected at the outset, because available evidence rules them out. The first explanation notes that homicide defendants and victims in states with large African-American populations tend to be African-American themselves, and hypothesizes that trials with black defendants or victims are more likely to be poorly run and unreliable given the defendant’s or victim’s low status or outright discrimination against them and their community. As we have seen, however, reversible error is not more probable when the defendant is black than when he or she is white, and the same goes for the race of the victim.³⁴⁸

A second hypothesis is that large black populations mean high rates of participation in death cases by black judges, prosecutors, defense lawyers and jurors, and that they commit more error than white trial participants. Here, the evidence contradicts both the premise and the inference from it.

African-American representation among states judges, prosecutors and defense lawyers is extremely low throughout the country, including in states with high percentages of African-American residents.³⁴⁹ Black participation on juries that impose death sentences is also low, for several reasons:

- The proportion of black citizens who are called and who appear for jury duty is substantially lower than the proportion of blacks in the population.³⁵⁰
- When prospective black jurors are called, judges regularly bar more of them than whites from actually sitting on capital juries because more blacks than whites oppose the death penalty in all cases and thus are ineligible to sit on capital juries.³⁵¹
- Even blacks who are not opposed to the death penalty in all cases have more qualms about it than whites who are eligible to sit in death cases,³⁵² which partly explains why prosecutors commonly use peremptory challenges to strike more blacks than whites from capital juries.³⁵³
- When black jurors are seated on capital juries, they tend to vote against imposing the death penalty more often than white jurors,³⁵⁴ so fewer death verdicts are imposed in such cases. (Typically, unanimity is required to impose the death penalty, so a single vote against it bars it.) As a result, the juries that impose death verdicts—the juries of interest in this study of cases in which death verdicts were imposed—tend to be mostly white in *all* states, including states that seat higher proportions of black jurors in cases in which death is not imposed.

Participation by African-American judges, prosecutors, defense lawyers and jurors in imposing the death penalty—and thus in committing capital error—appears to be too uniformly small across states, therefore, to account for the large amounts of error in such cases and wide differences among states.

In addition, there is no evidence to suggest that capital trial participants of one race are any more prone to error than members of another race. On the contrary:

- The evidence presented thus far suggests that the three racial factors have an effect at the *state level where overall death penalty policy* is made, not at the trial level where policy is applied.³⁵⁵
- The evidence presented below reveals that error is associated with aggressive use of the death penalty, making it less likely that members of the racial group with the least enthusiasm for the penalty are mainly responsible for its misuse.³⁵⁶

Low black support for the death penalty also undermines a third hypothesis—that pressure

from the black community to use the death penalty accounts for high rates of capital error. Although such pressure would theoretically be higher in states with larger black populations, and might have an effect even where most trial participants are white, there is no evidence that such pressure exists, and considerable evidence that such pressures are much more likely to come from the white community. Even if blacks did press for use of the death penalty, the available evidence suggests that law enforcement officials are relatively unresponsive to pressures from the black community.³⁵⁷

The evidence contradicting these hypotheses suggests a different explanation for the significant association between capital error rates and the three racial factors identified by Analyses 1 and 2. **All three racial factors tend to generate crime fears among members of politically influential communities. And those fears may generate pressure on officials to extend their use of the death penalty to weaker, more marginal cases where the need to cut corners to obtain capital convictions and sentences is greater.**

Longstanding racial stereotypes and prejudices have created a stubborn association in the minds of some between blacks and violent crime, especially violent crime against whites.³⁵⁸ **The larger a state's African-American minority,³⁵⁹ therefore, the more fear of violent crime some members of the majority may feel, and the more pressure politically influential members of that group may generate to use the death penalty as a protective measure.**³⁶⁰ Second, a fair gauge of the threat of homicide felt by politically influential communities—and the best gauge for which evidence is available in all states—is whether and to what extent homicides committed in the state actually pose as much of a threat to the white, as to the black, community. **As the level of white homicide victimization approaches or surpasses the level of black homicide victimization, pressure to use the death penalty may increase as well.** Finally, if both racial forces have the

suggested effect, states with both forces operating at once—high black populations and high rates of white, relative to black, homicide victimization—would likely generate especially heavy pressures to use the death penalty as a protective measure. **If people with political influence feel not only that they live in a high-crime environment and are often the victims of violent crime, but, in addition, that they might be targeted for crime by members of a different group, pressure to increase the use of the death penalty might be especially high.**

This explanation is provisional, for now. If it is credible, capital error should also be associated with one or more of the following conditions:

- a state's heavy use of the death penalty;
- death verdicts imposed where the evidence is weak or marginal;
- methods of selecting capital officials that make them vulnerable to political pressure; or
- a state's relatively ineffective non-capital response to serious crime, increasing the death penalty's attractiveness as an outlet for crime fears.

We consider below whether our study results provide any evidence of these processes by which crime fears lead to increased use of the death penalty which, in turn, lead to increased rates of capital error.

c. More aggressive use of the death penalty and higher capital-error rates.

As Figure 11, pp. 121 above, demonstrates, states vary widely in how often homicides resulted in death verdicts in the study period. **Colorado, Connecticut, Maryland, New Jersey, New Mexico and Washington had 7 or fewer death verdicts per 1000 homicides; and California and Louisiana had around 10 per 1000 homicides.** By contrast, **Alabama had around 37 death verdicts per 1000 homicides; Arizona, Delaware, Nevada and Oklahoma had around 45 per 1000 homicides; and Idaho had 60 per 1000 homicides.** Above, we note that **a number of states with above average**

death-sentencing rates per 1000 homicides carry out below average proportions of the death verdicts they impose—and, conversely, that states with low death-sentencing rates often have high execution rates.³⁶¹ Together with Figure 9, pp. 79 above—revealing a correlation between high error rates and low execution rates—these results suggest a relationship between high capital-sentencing rates and high error rates.

Evidence that high rates of death verdicts per 1000 homicides may be related to high rates of reversible capital error was first detected by Cornell Law Professors John Blume and Theodore Eisenberg. Using Bureau of Justice Statistics (BJS) data for 1985-95, they found that “[t]he rate at which states impose [death] sentences strongly correlates with the rate at which relief was obtained from those sentences.”³⁶² After we replicated this two-factor correlation with the BJS data that Blume and Eisenberg used, and with our own data covering the longer period from 1973 to 1995,³⁶³ we decided to include Blume and Eisenberg’s explanatory factor in our multiple regression analyses³⁶⁴ to see if the two-factor relationship between high death-sentencing and high error rates persists when other factors are simultaneously considered.

Analyses 1A, 1B, 1RA, 1RB, 1RC, 2A and 2B all reveal a highly significant relationship between high death-sentencing rates and high rates of reversible capital error. Because the relationship is between two *rates* (death verdicts per 1000 homicides, and reversals per 100 death verdicts), it is not a foregone conclusion—as it would be if it instead compared raw numbers. Although states with larger numbers of death verdicts (*e.g.*, states with larger populations) should have larger *numbers* of reversals, because more cases to review should mean more reversals, there is no reason to presume that all death verdicts from particular states (*e.g.*, populous states) are more likely to contain reversible error than all verdicts from other states.³⁶⁵ Yet, that is what Analyses 1 and 2 find: Controlling for

other factors, **states that are more likely to impose death verdicts per 1000 homicides are more likely to have the verdicts they impose reversed due to serious error. And states that are less likely to impose death verdicts per 1000 homicides are less likely to have their verdicts reversed.** *States with a propensity to impose death sentences are prone to serious capital error.* We already have suggested a reason for this relationship:³⁶⁶ **The more homicides officials treat as capital, the more often they may sweep in marginal cases where it is necessary to cut corners and commit other kinds of errors to obtain death verdicts.** We explore this hypothesis further below.

d. Poorer record of arresting and punishing serious criminals and higher capital-error rates.

Originally, we hypothesized that states with high rates of incarcerated individuals would have high rates of serious capital error. We accordingly designed a factor comparing the number of people incarcerated in each state's prisons each year (a number influenced by how many people the state arrests, convicts and imprisons) to the number of FBI Index Crimes committed in that state and year. Index Crimes are a composite of property and violent crimes (including homicide) the FBI uses to measure crime rates.³⁶⁷

In all seven analyses (1A, 1B, 1RA, 1RB, 1RC, 2A and 2B), this factor indeed has a highly significant relationship to capital error rates. But the relationship runs in the opposite direction from what we predicted: **States with higher prison populations relative to the number of serious crimes—*i.e.*, states that apprehend, convict and imprison more of their serious criminals—have lower rates of reversible capital error than states that arrest, convict and imprison fewer serious criminals.** This suggests that **states with relatively more effective *non-capital* responses to crime—*i.e.*, arrest, conviction and imprisonment—may be under less pressure than states with**

weaker law enforcement records to use the death penalty. And that in turn may dampen the penalty's use in weak cases in which the temptation to use unreliable procedures is high.

e. Heavier political pressure on state judges and higher capital-error rates.

Is there any evidence that political pressure on capital officials influences capital error rates? To answer this question, we had to identify differences among states in the political pressure capital officials experience, to see if the differences are associated with differences in states' capital error rates. Two actors in the capital system face political pressures formal enough to measure: district attorneys³⁶⁸ and state judges. Both are subject to legally defined selection procedures that make them vulnerable to political pressure from the officials who appoint them or the voters who elect them. District attorneys do not provide a good point of comparison, however, because most are selected the same way—partisan elections every four years.³⁶⁹ This leaves too little state-to-state variation to compare to differences in capital reversal rates.

State judges, by contrast, are selected in a variety of ways that create the needed variability. In a few states, judges are appointed by other officials and never face direct elections. In other states, judges are appointed to long terms and can be removed only by recall elections triggered by fairly onerous petition requirements. In other states, judges are appointed to shorter terms, after which they face periodic retention elections. And in other states, judges are directly elected from the beginning in contested elections, for either longer or shorter terms, in either non-partisan or partisan elections. These possibilities create a continuum of selection methods ranging from those placing less to those placing more political pressure on judges to conform their rulings to the desires of politically influential groups. Using these criteria, we developed two measures of political pressure on judges

in different states to see if either has a significant relationship to rates of serious capital error.

In all analyses under discussion, the amount of political pressure on state judges is related to higher error rates, and the relationship is highly significant. The two indexes of political pressure had similar effects.³⁷⁰ **Death verdicts imposed at trials run by state judges who are subject to relatively more direct political pressure are more likely to be seriously flawed than verdicts presided over by judges facing less political pressure.**

f. Other factors related to higher capital-error rates.

We also hypothesized that demographic factors, and factors gauging the quality of the criminal justice system such as expenditures and caseloads, might affect rates of reversible error. Two factors were significant: population structure and capital and other caseload burdens on the courts.

i. Higher population size and density. In research on the spatial patterning and structural covariates of county homicide rates, Professor Steven F. Messner and colleagues at the University of Albany and the University of Illinois developed a combined measure of population structure that compares jurisdictions based on their overall population and population density.³⁷¹ This factor, adapted for use at the state level, was positively associated with error rates and significant in all seven analyses.³⁷² **In these analyses of reversal rates at all three stages of judicial review, states with larger populations that are more highly concentrated in cities tend to have higher reversal rates than less heavily and densely populated states.**

ii. Courts more burdened by capital and non-capital caseloads. As is noted above, Analyses 1 and 2 find that large numbers of capital verdicts awaiting court review, and general court congestion, are each associated with low capital reversal rates—as one would expect, given that court delay decreases the amount of court review, which in turn lowers rates of reversals per imposed

verdicts.³⁷³ We also studied the interaction of the two types of court congestion, expecting it to coincide with especially low reversal rates.³⁷⁴ The interaction is indeed related to reversal rates, and the relationship is highly significant in all analyses, but its direction is the opposite of the one we had predicted. **States whose courts are congested with large numbers of capital verdicts being reviewed *and* high rates of court filings tend to have high capital reversal rates.** The positive association between reversal rates and the interaction of capital and non-capital court congestion suggests that **a combination of many pending capital and non-capital cases may overwhelm courts, increasing the number of serious mistakes made in trying capital cases.**

4. The *size* of the effects associated with each significant explanatory factor.

a. Effect size generally.

With enough observations, even tiny relationships may be statistically significant. This leads us to ask whether the significant relationships between capital reversal rates and the explanatory factors described above are large and important enough to deserve attention in, for example, crafting reform proposals. Statistical analysis lets us do this by estimating effect size—**the predicted rise or fall in capital reversal rates associated with a given change in the amount or intensity of each significant explanatory condition.**

We use two methods of displaying effect size—one numerical, the other in graphs. The numerical estimates are reported in our detailed results in Appendix G, coded as “newestimates.” The graphs are displayed below, starting with Figure 22A, p. 175 below. The different interpretation of these effect-size estimates in binomial regression analyses (Analyses 1 and 1R) and Poisson analyses (Analysis 2) is discussed above.³⁷⁵ Recall that, when using a particular analysis to predict the change in capital reversal rates associated with a given change in the amount or intensity of the explanatory

condition under discussion, *the graphs hold all other explanatory factors in the analysis constant at their average.*

As we explain above, the reversal rates Analyses 1 and 2 predict reversals as a proportion of all *imposed* verdicts, not as a proportion of only *reviewed* verdicts. This enlarged base number of verdicts (*i.e.*, the enlarged denominator) enables our statistical analyses to make the best use of all our data.³⁷⁶ But by inflating the base number (denominator) to include unreviewed as well as reviewed verdicts, these analyses deflate predicted reversal rates (the number of reversals divided by the larger base of all imposed verdicts) below the actual reversal rates (the number of reversals divided by *actually reviewed* verdicts).³⁷⁷ Particularly given that the rates presented in these graphs are systematically depressed by their enlarged denominators, the best use of the effect size graphs is comparative. **Rather than predicting the actual reversal rates we know to have occurred—*e.g.*, 68% nationally over the 23-year study period—the graphs are best used to indicate the percent increase or decrease in reversal rates associated with specified increases or decreases in the amount or intensity of significant explanatory factors.** In this way, we can determine whether statistically significant factors are worth attention based on whether changes in them are associated with appreciable changes in capital reversal rates, holding other factors constant.

On each effect-size graph, the horizontal (*x*) axis plots the value of the explanatory factor, and the vertical (*y*) axis plots the predicted reversal rate associated with that value (for binomial analyses) or the predicted change in the reversal rate associated with that value (for Poisson analyses). The range of values displayed on the horizontal axis for each explanatory factor is the actual range of values for that factor among states and years in our study. Thus, the rise or fall in predicted reversal rates (holding other factors constant) that the graph displays is the rise or fall across the actual

spectrum of 34 capital-sentencing states and 23 years in our study. (The minimum, average and maximum values for each explanatory factor in each analysis are listed in Appendix F-1.) In most cases, the graphs divide the range of values on the horizontal axis into seven equal intervals.

As a check on the extent to which different sets of factors within the same analyses, and different analyses (binomial vs. Poisson analyses), make a difference in effect sizes, we proceed factor by factor, examining the size of its effect in all four analyses, with four graphs displayed on the same page. The steeper the line on the graph, the larger the effect size. Lines that get higher (meaning predicted reversal rates increase) as they move from left to right (as the explanatory factor increases) indicate that the explanatory factor is associated with *higher* reversal rates. Lines that get lower as they move from left to right indicate that the explanatory factor is associated with *lower* reversal rates. We do not report effect sizes for interaction effects, which are more complicated to interpret.³⁷⁸

b. Factors unrelated to the quality of death verdicts.

i. Court delays in reviewing capital verdicts. Figures 22A-D, p. 175 below, display the size of the effect on reversal rates of large backlogs of unreviewed capital verdicts in, respectively, Analyses 1A, 1B, 2A and 2B. As is noted above, this factor serves mainly to measure the non-error-related, downward effect on reversal rates (as proportions of imposed verdicts) that delay causes by depressing rates of review.³⁷⁹ Figures 22A-D indicate that, as backlogs of capital verdicts awaiting review increase, the number reviewed—and thus the number available to be and that actually are reversed—decreases sharply. At some point—about where the backlog of unreviewed verdicts reaches 20—the system appears to shut down, with virtually no cases being reviewed or reversed.³⁸⁰ **This suggests that as the number of death verdicts awaiting review increases, they so clog the appellate system that it ceases to function as a means of moving valid death verdicts forward**

to execution and for diverting flawed verdicts back for retrials. In that event, unclogging the system would require fewer death verdicts, fewer flaws demanding extended review, or both.

Displaying the effect size graphs for all four analyses on the same page reveals that they are fairly similar, as is true for most factors found significant by Analyses 1 and 2. Effect sizes for our main, binomial regressions tend to be larger than those for the Poisson regressions (Analysis 1 vs. Analysis 2), but the differences are modest, as are differences among analyses with slightly different groupings of explanatory factors (Analysis 1A vs. 1B vs. 2A vs. 2B).

ii. Delays in all court cases. Figures 23A-C, p. 176 below, show the size of the statistically significant effect on capital reversal rates of the combined four-factor measure of undecided court cases of all types, holding other factors constant. Figure 23D shows the same for the alternative measure of court caseloads that uses only one of those four measures of general court caseloads. In both cases, effect size is too small to warrant additional attention to these alternative measures of general court cases—illustrating the usefulness of effect-size inquiries in distinguishing among explanatory factors all of which are statistically significant.³⁸¹

Effect Size: Backlog of Capital Verdicts (Analyses 1A, 1B, 2A, 2B)

Figure 22A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

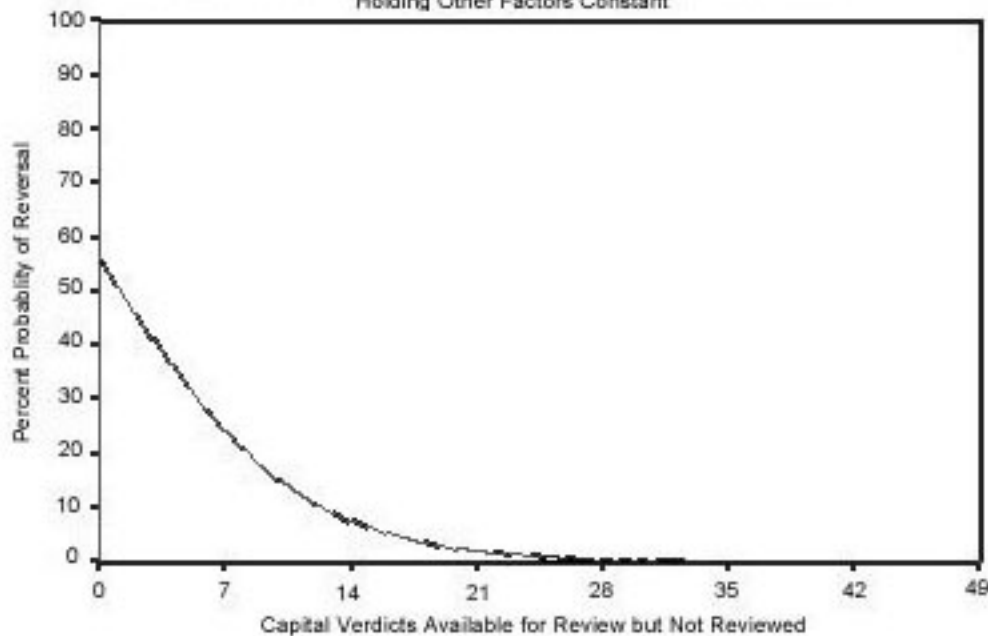


Figure 22B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

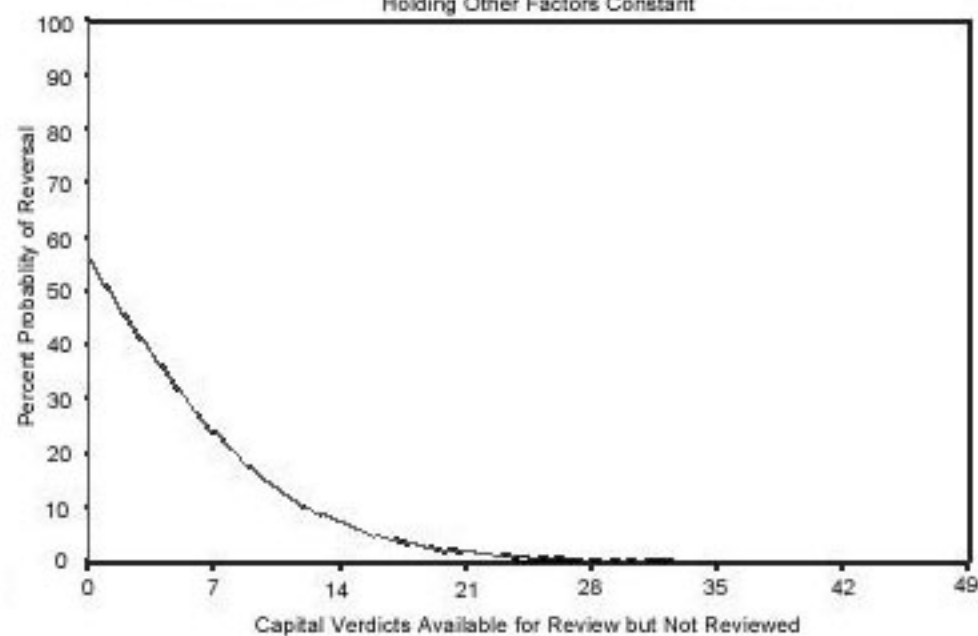


Figure 22C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

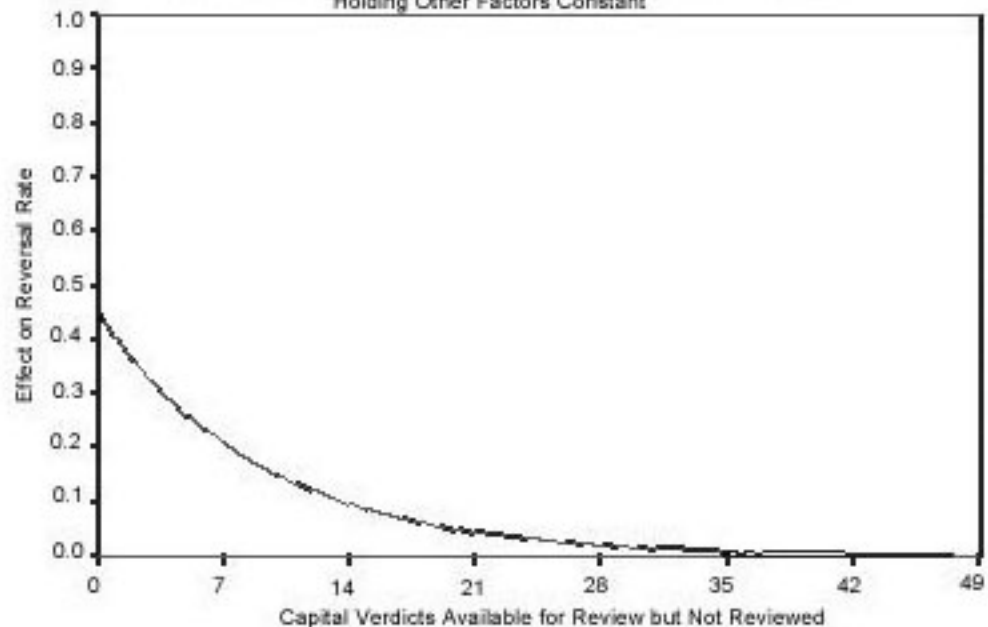
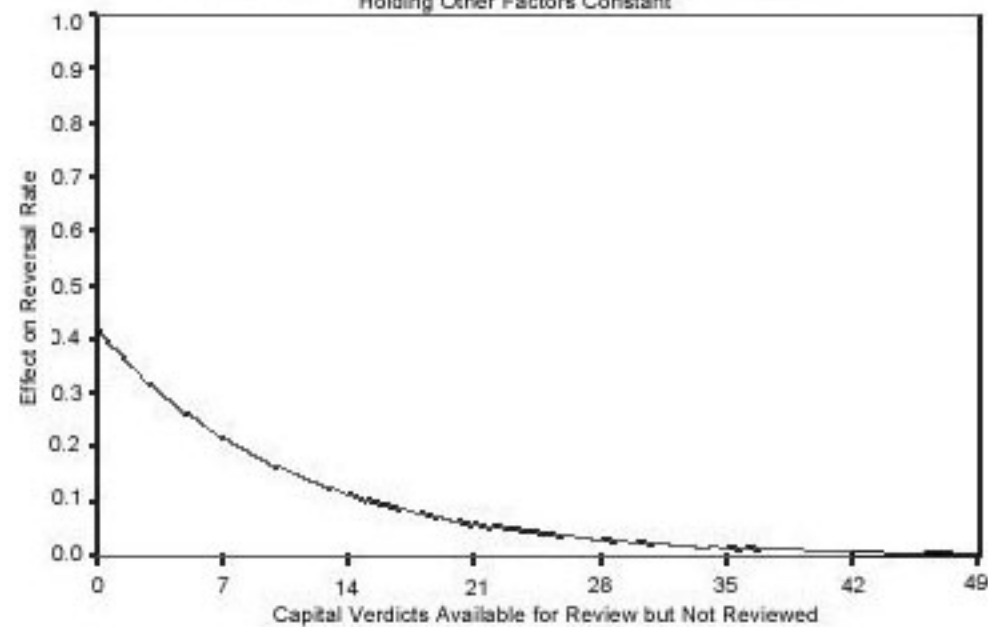


Figure 22D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant



Effect Size: State Court Caseloads (Analyses 1A, 1B, 2A, 2B)

Figure 23A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Four Factor Measure of Net State Court Caseloads, Holding Other Factors Constant

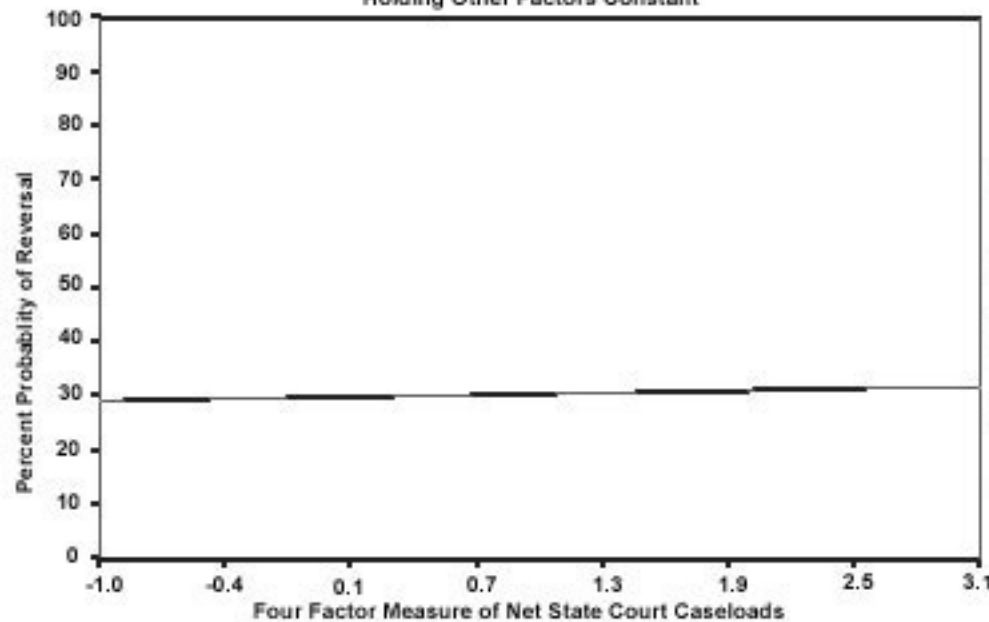


Figure 23B (Analysis 1B) Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Four Factor Measure of Net State Court Caseloads, Holding Other Factors Constant

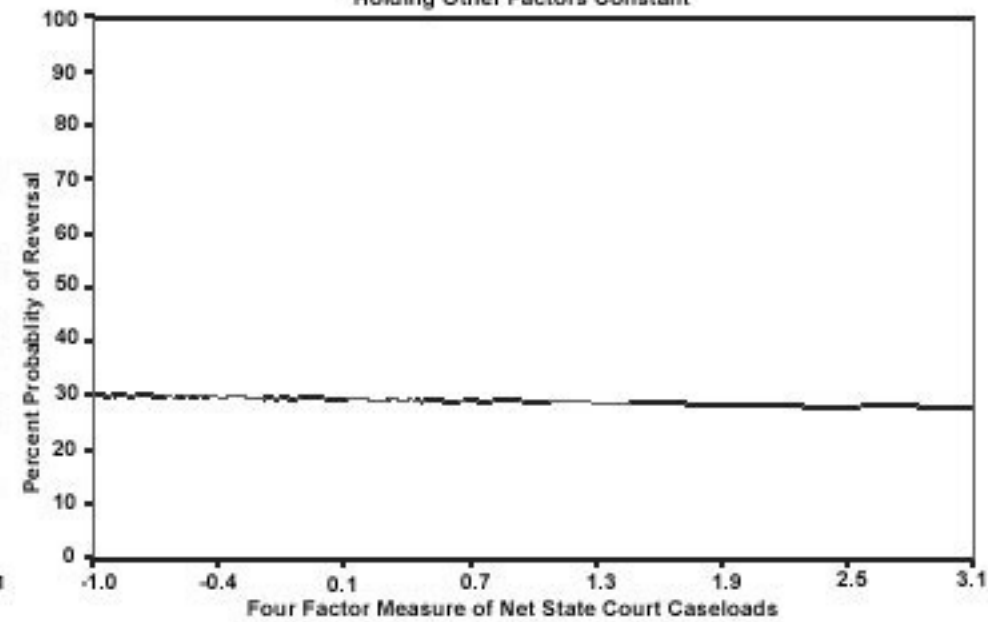


Figure 23C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Four Factor Measure of Net State Court Caseloads, Holding Other Factors Constant

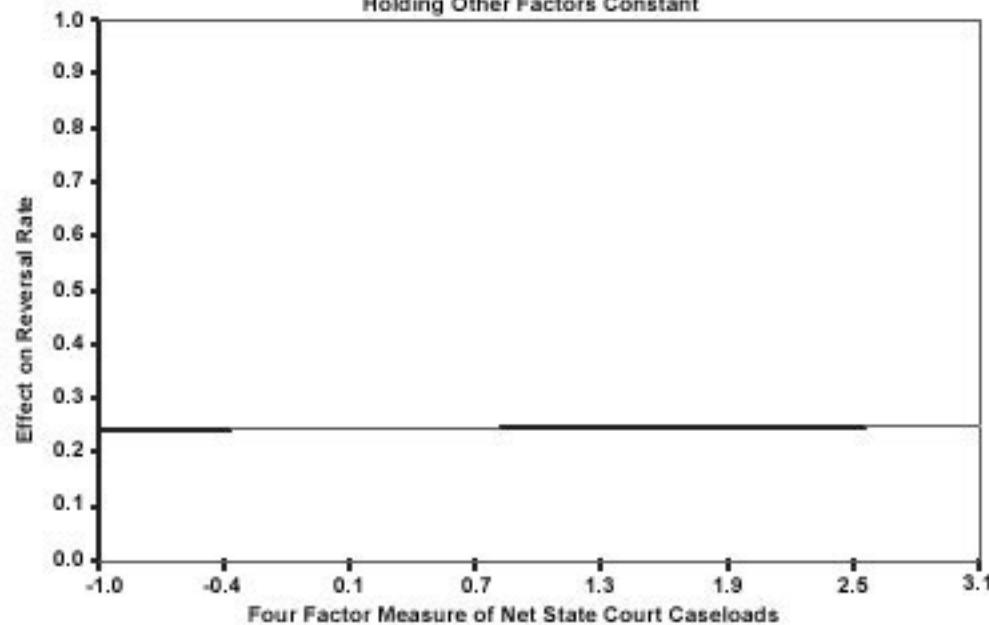
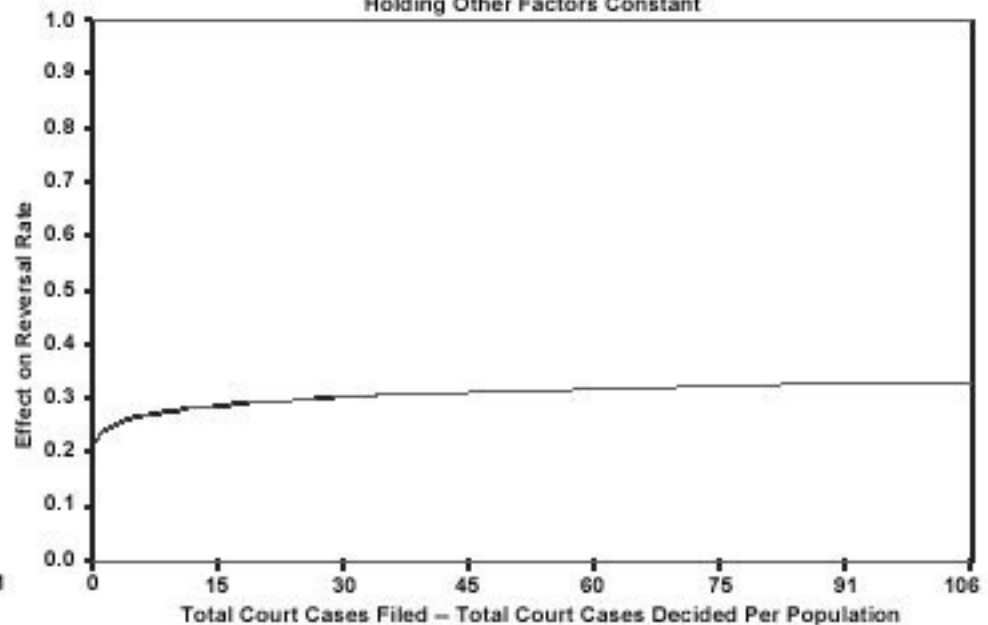


Figure 23D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of All State Court Cases Awaiting Decision, Holding Other Factors Constant



iii. *Year of death verdict.* As is discussed above, court review of death verdicts takes several years at each of three review stages, so that almost no death verdicts imposed in the last years of the study period were reviewed at any, much less at all, review stages by the end of that period.³⁸² More generally, the later a verdict was imposed, the less likely it is that the verdict was reviewed and reversed as of 1995 at any and especially at the later review stages.³⁸³ This holds true for seriously flawed as well as high-quality verdicts. At the federal habeas stage, the effect is magnified for *flawed* verdicts, which take longer to review than valid verdicts.³⁸⁴

The year the death verdict was imposed serves as a control for this non-error-related, downward effect on reversal rates of unfinished appeals.³⁸⁵ If the expected decline over time in reversal rates (as a proportion of imposed verdicts) occurs—as it does in Analyses 1A-2B—that decline indicates that the factor is serving its purpose as a control for the dampening effect on review and thus reversal rates of unfinished appeals. But the decline conveys little information about changes in the probability of *reversible error* over time.³⁸⁶

Figures 24A-D, p. 178 below, display the *size* of this effect as predicted by Analyses 1A-2B. Because that effect is fairly small—a decline in reversal rates of about 1 or 2 percentage points per year in Analyses 1A and 1B—and because that decline is less steep than the drop-off in completed appeals indicated by Table 4, p.142 above, these graphs suggest the possibility that **increases in *flawed* death verdicts over time, after accounting for other factors, may be neutralizing some of the dampening effect on reversal rates of *unfinished* appeals.** If that is so, we should find increases in reversal rates over time (controlling for other factors) in the regressions discussed below³⁸⁷ in which reversal rates are calculated as proportions of actually reviewed, not all, verdicts, because those regressions avoid the non-error-related effect of unfinished appeals.³⁸⁸ Our direct appeal regressions indeed reveal just that increase in reversal rates over time, accounting for other factors.³⁸⁹

Effect Size: Year of Death Verdict (Influence of Unfinished Appeals) (Analyses 1A, 1B, 2A, 2B)

Figure 24A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Year Death Sentence Was Imposed, Holding Other Factors Constant

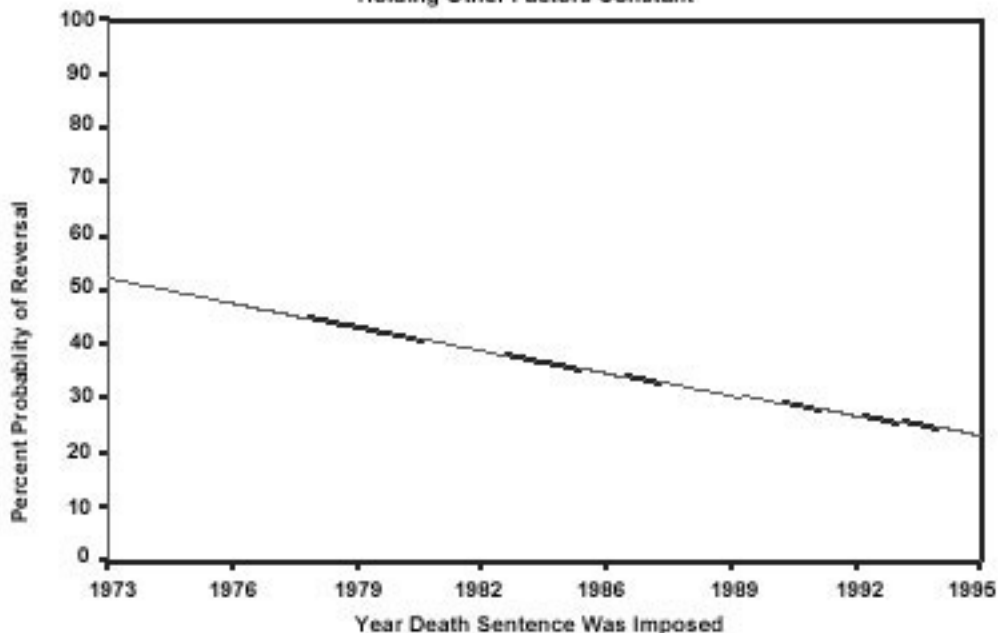


Figure 24B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Year Death Sentence Was Imposed, Holding Other Factors Constant

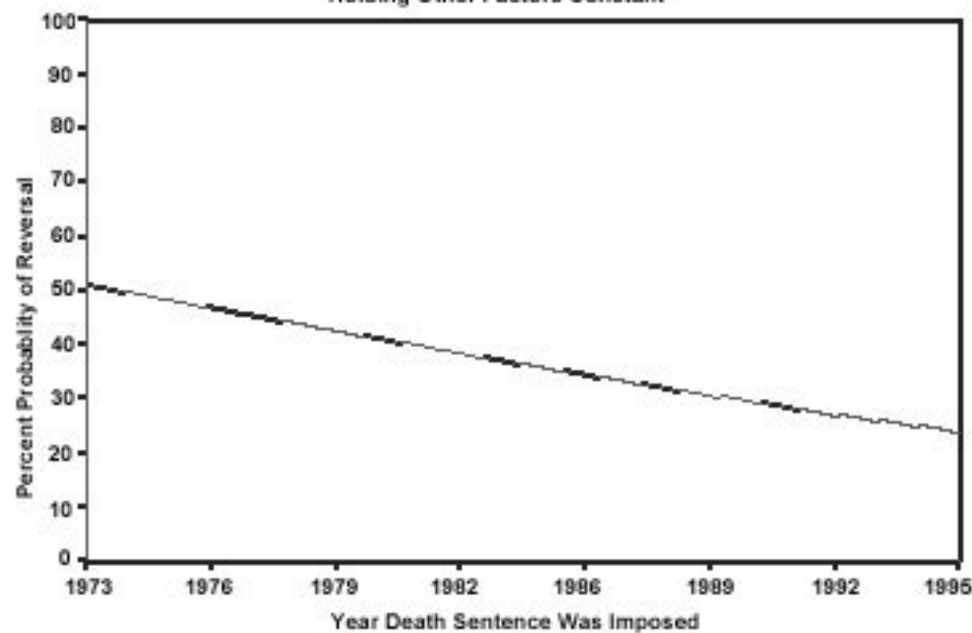


Figure 24C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Year Death Sentence Was Imposed, Holding Other Factors Constant

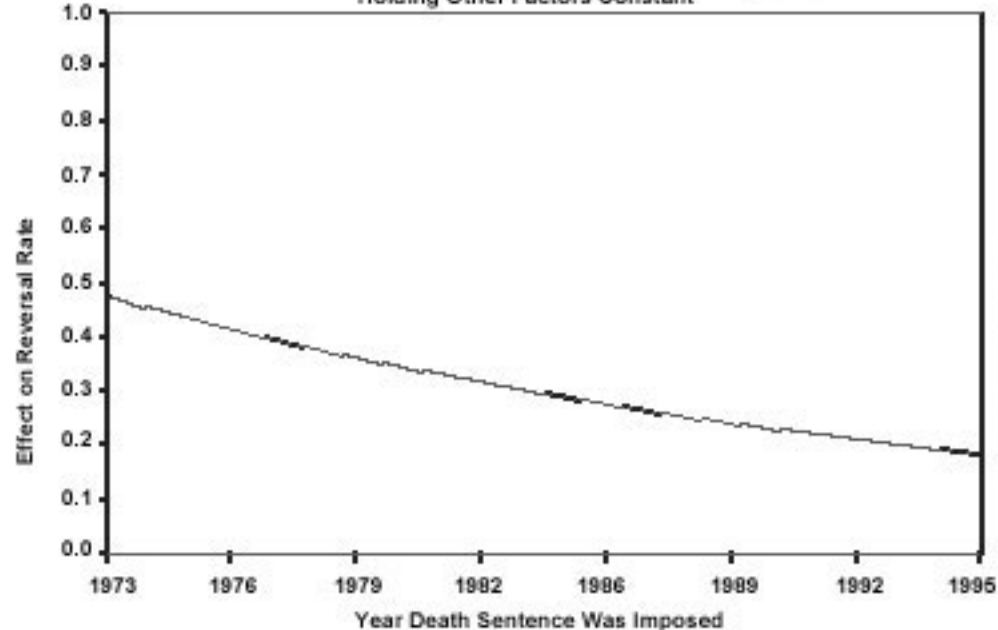
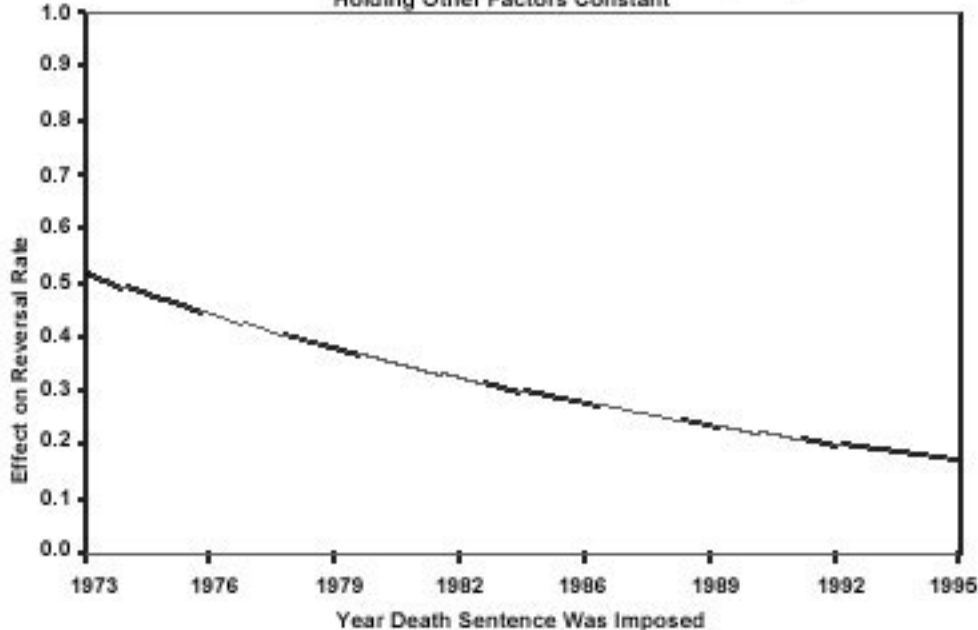


Figure 24D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Year Death Sentence Was Imposed, Holding Other Factors Constant



c. Factors that appear to be related to the quality of death verdicts.

i. Larger African-American population. Figures 25A-D below indicate that **increases in the proportion of African-Americans in a state's population are associated with considerable percentage increases in predicted capital error rates.** During the study period, the African-American proportion of states' population ranged from .25% (Montana, 1978) to 36% (Mississippi, 1975), and averaged about 14% in all 34 capital states and 23 years. Binomial Analyses 1A and 1B (Figures 25A and 25B, p. 180) below, predict that, holding other factors constant at their average, **reversal rates increase more than 4-fold when the black proportion of the population rises from its lowest level among and states and years in the study to the average level for all states and years; doubles when the black proportion of the population rises from 5% to 35%; and increases about 8-fold across the entire spectrum of African-American populations in the study.** Poisson Analyses 2A and 2B (Figures 25C and 25D) predict about 4-fold increases across that spectrum.

Effect Size: Proportion of African-Americans in State Population (Analyses 1A, 1B, 2A, 2B)

Figure 25A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Proportion of African-Americans in State Population, Holding Other Factors Constant

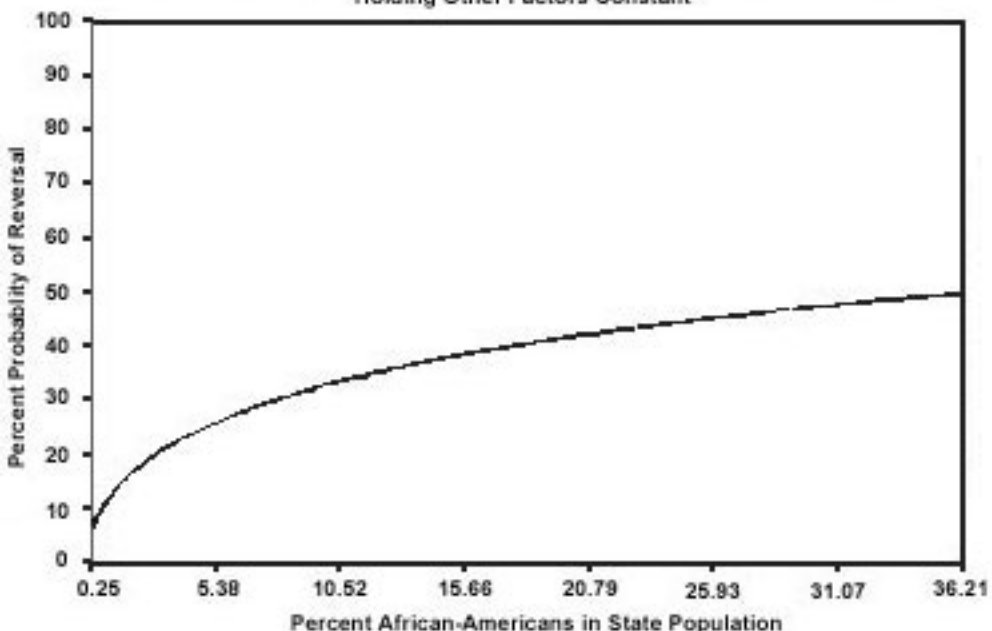


Figure 25B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Proportion of African-Americans in State Population, Holding Other Factors Constant

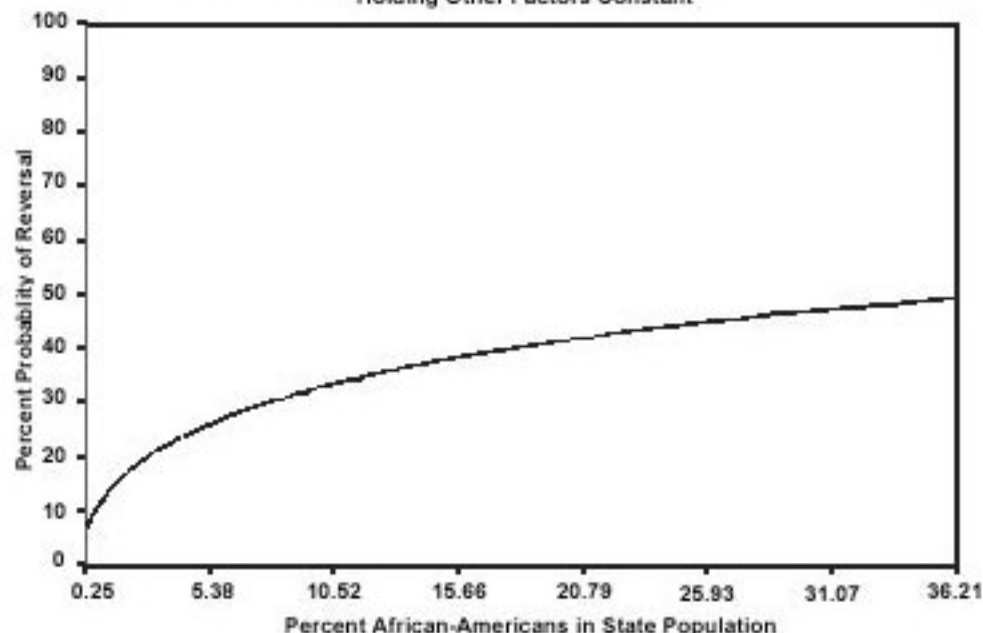


Figure 25C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Proportion of African-Americans in State Population, Holding Other Factors Constant

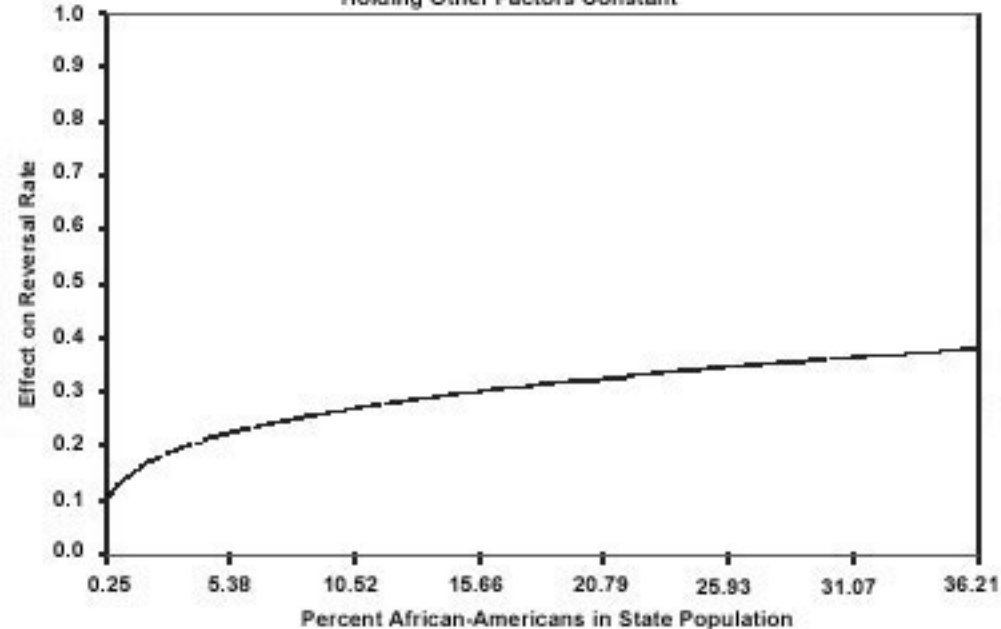
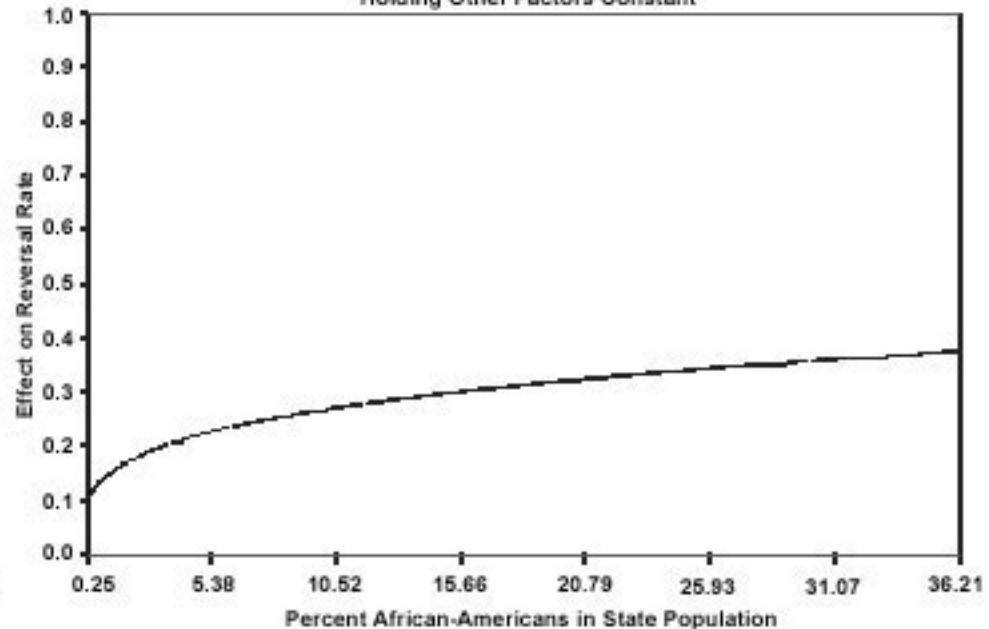


Figure 25D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Proportion of African-Americans in State Population, Holding Other Factors Constant



ii. *Relatively high homicide risk to whites as compared to blacks.* Figures 26A-D, p.182 below, estimate the size of the effect on capital error rates of the rate of white as compared to black homicide victimization.³⁹⁰ In the state and year with the highest white-to-black victimization rate (New Mexico, 1987), the number of white homicide victims per 100,000 whites in the population was just slightly greater than the number of black homicide victims per 100,000 blacks in the population. In the state and year with the lowest rate (Utah, 1974), the number of white homicide victims per 100,000 whites was about 5% that of the black victimization rate. Again, the effect-size graphs enable us to gauge the predicted rise in reversal rates across this spectrum of comparative homicide rates for whites and blacks, holding other factors constant at their averages.³⁹¹ **Other things being equal, where whites and blacks face the *same* risk of being killed by homicide, Analyses 1A and 1B predict that the rate of serious capital error is about twice as high as when the homicide risk to white citizens is only 5% of the homicide risk to black citizens. The predicted increase in reversal rates is about 67% when the homicide risk faced by whites increases from 10% of the homicide risk faced by African-Americans to the same risk. Poisson Analyses 2A and 2B (Figures 26C and 26D) predict somewhat smaller increases.**

Effect Size: Risk of Homicide to Whites Versus Blacks (Analyses 1A, 1B 2A, 2B)

Figure 26A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant

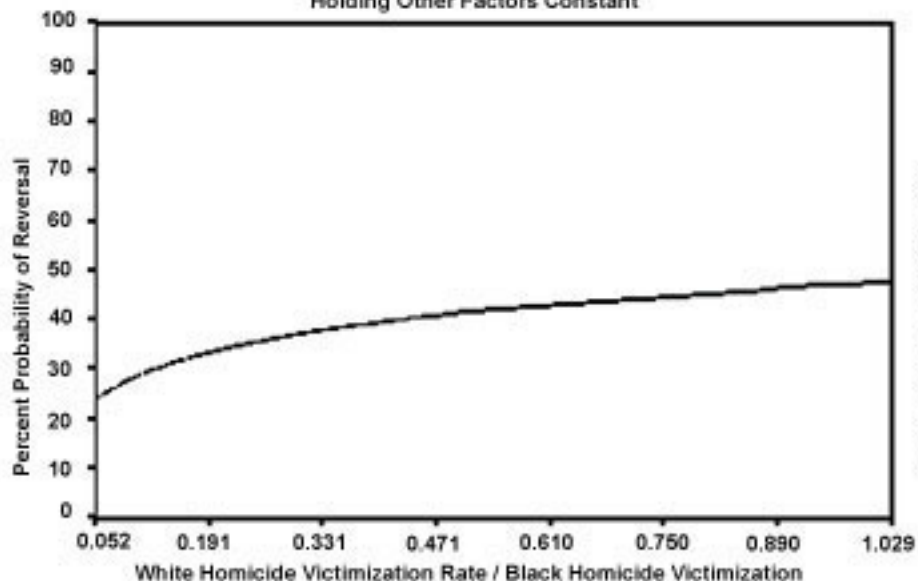


Figure 26B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant

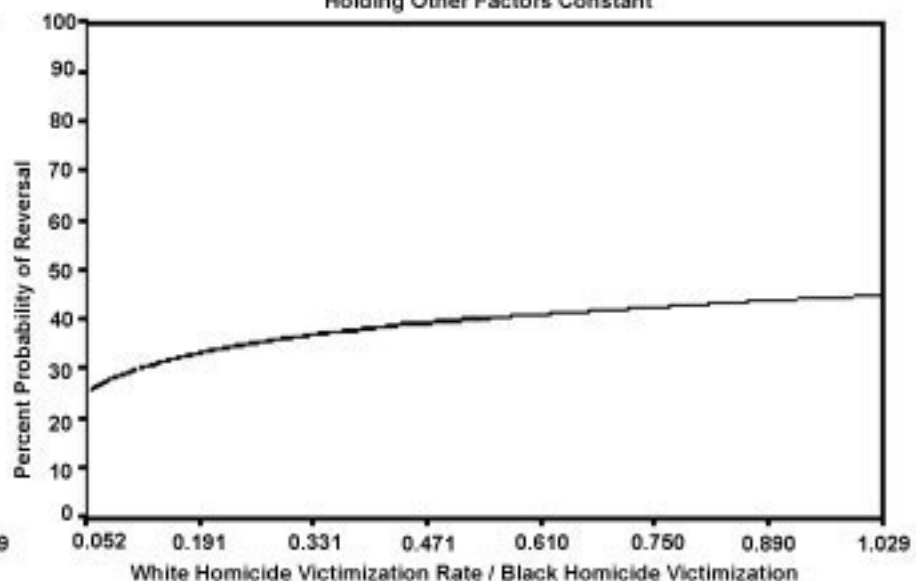


Figure 26C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant

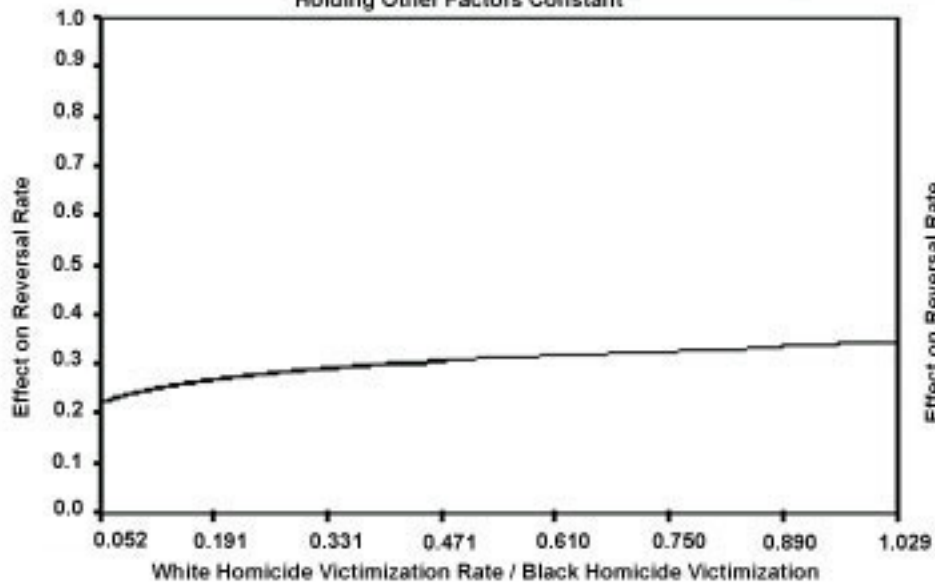
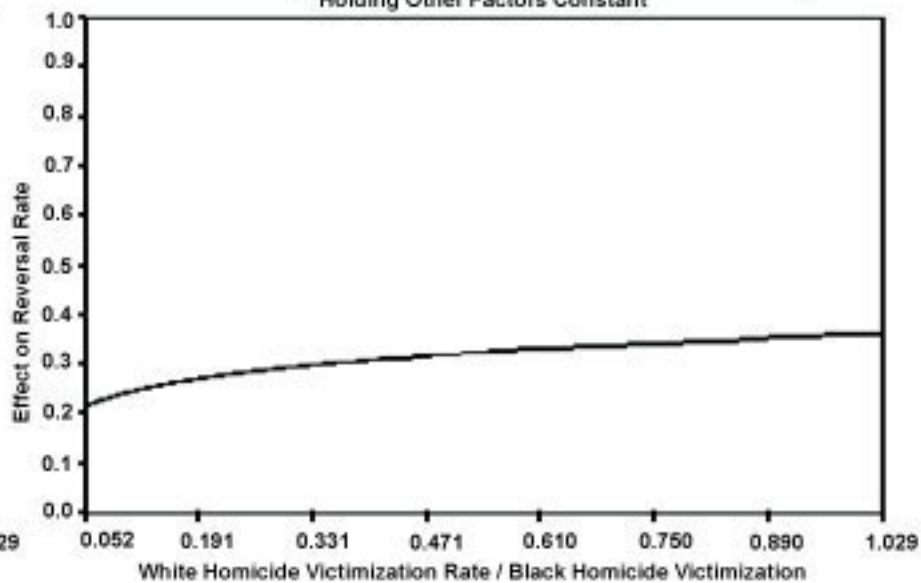


Figure 26D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant



iii. *More aggressive use of the death penalty.* Analyses 1 and 2 find that higher death-sentencing rates are linked to higher capital error rates.³⁹² Across states and years in the study, death verdicts per 1000 homicides varied from about 1 (*e.g.*, Georgia, 1995; Pennsylvania, 1979) to 208 (Idaho, 1982), and averaged about 23.5 for all states and years. As shown in Figures 27A-D, p. 184 below, all four analyses predict large increases in capital reversal rates as death-sentencing rates rise, holding other factors constant. Binomial Analysis 1A (Figure 27A) predicts:

- **almost a doubling in reversal rates (43%, compared to 23%), when death-sentencing rates rise from a quarter of the national average to the national average; and**
- **more than a tripling in reversal rates (over 74% vs. 23%) where death-sentencing rises for a quarter the national average to the maximum level among states in the study.**

Poisson Analysis 2A (Figure 27C) predicts a quadrupling of reversal rates over that same spectrum of death-sentencing rates. As noted, the predicted reversal rates shown here are not true error rates—reversals as a proportion of *reviewed* death verdicts—but, instead, rates of reversals as a proportion of *imposed* verdicts (whether or not the verdicts finished being reviewed for error), which are systematically lower than the true error rate.³⁹³ That, even so, predicted reversal rates rise from 13% to over 75% across the spectrum of death-sentencing rates in the study indicates the sizeable impact of death-sentencing rates on reversal rates. Together with the clogging effect of large numbers of capital verdicts awaiting review,³⁹⁴ the sharply higher reversal rates linked to higher death-sentencing rates begin to suggest that *less is more*. **Singling out fewer—only the most egregious—homicides for capital charges and sentences may mean higher quality death verdicts, fewer reversals, less frequent and shorter delays on appeal and a more efficient system overall.**

Effect Size: State Death Sentencing Rate (Analyses 1A, 1B, 2A, 2B)

Figure 27A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Death Sentencing Rate, Holding Other Factors Constant

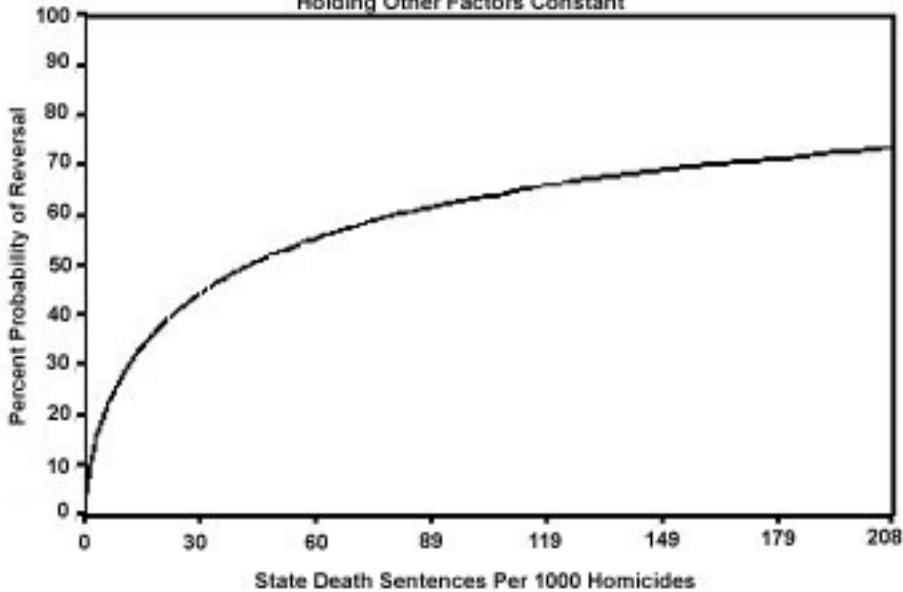


Figure 27B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Death Sentencing Rate, Holding Other Factors Constant

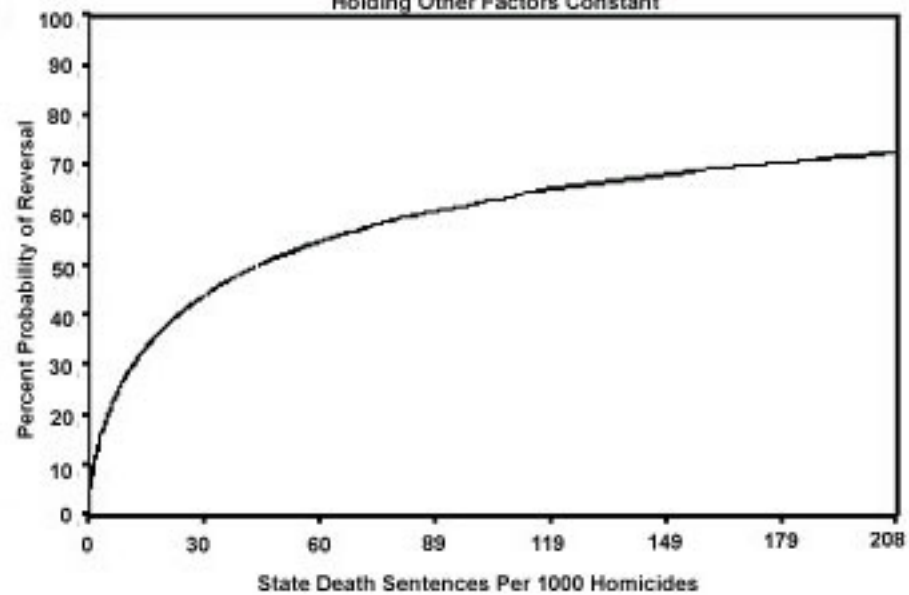


Figure 27C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of State Death Sentencing Rate, Holding Other Factors Constant

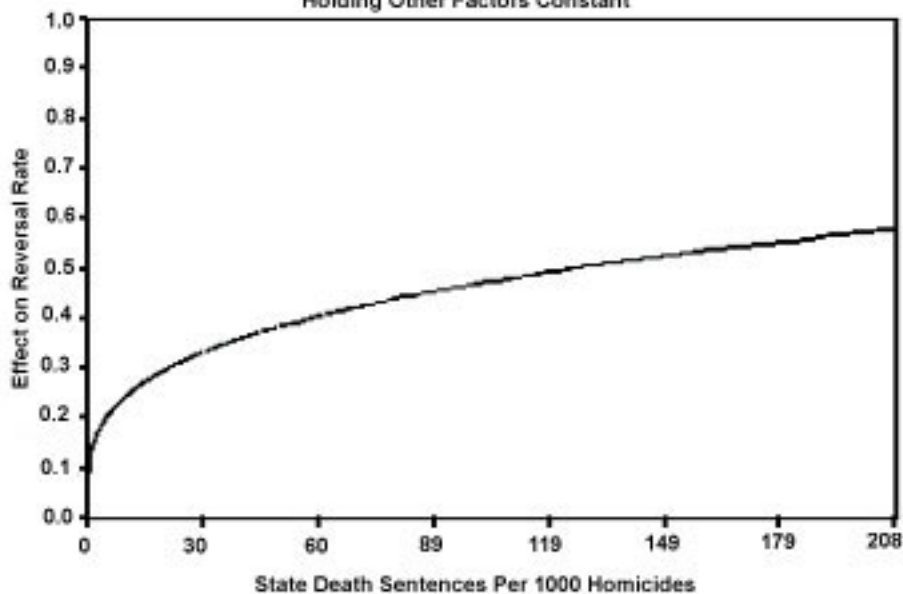
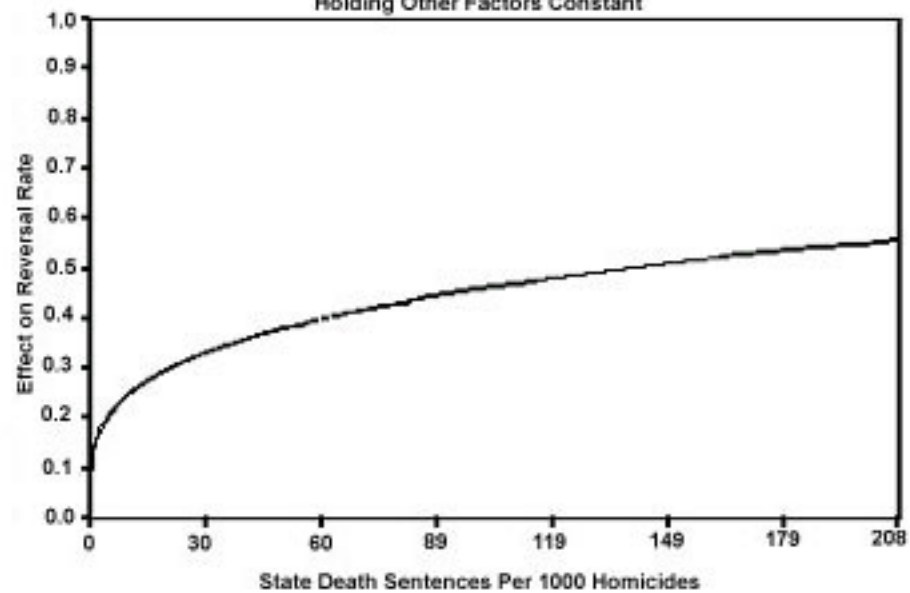


Figure 27D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of State Death Sentencing Rate, Holding Other Factors Constant



iv. Weaker record of apprehending and imprisoning serious criminals. Death verdicts from states with fewer prisoners per 100 serious crimes are significantly more likely to be overturned than verdicts from states with higher rates of prisoners per serious crime.³⁹⁵ States and years range from just about 1, to about 13, prisoners per 100 FBI Index Crimes, averaging about 5.³⁹⁶ Figures 28A-D, p. 186, display the size of this effect, which again is large. Everything else equal, binomial Analysis 1A (Figure 28A) predicts **capital error rates (per imposed verdicts) of about 75% where there is only 1 prisoner per 100 Index Crimes—but only about 36% where there are 4 prisoners per 100 Index Crimes, and only about 13% for the highest number of prisoners per 100 Index Crimes among states and years in the study.** Poisson Analysis 28C predicts a fivefold drop in predicted error rates across the same spectrum. **These analyses predict that states with better records of arrest, conviction and incarceration—i.e., with more effective alternatives to capital punishment that may relieve pressure to sentence capitally in close cases—do a better job of avoiding flawed capital verdicts than states with weaker law enforcement records.**

Effect Size: Rate of Arrest, Conviction and Imprisonment Per Serious Crime (Analyses 1A, 1B, 2A, 2B)

Figure 28A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

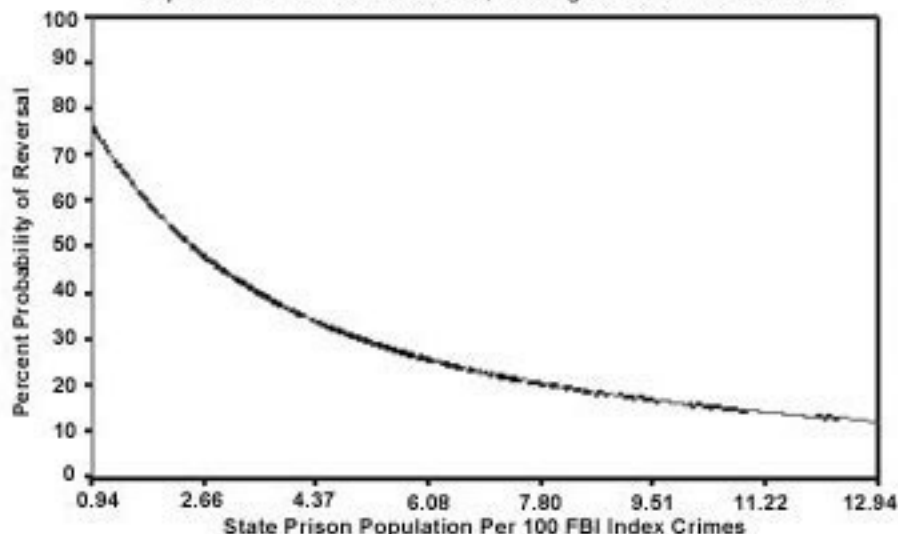


Figure 28B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

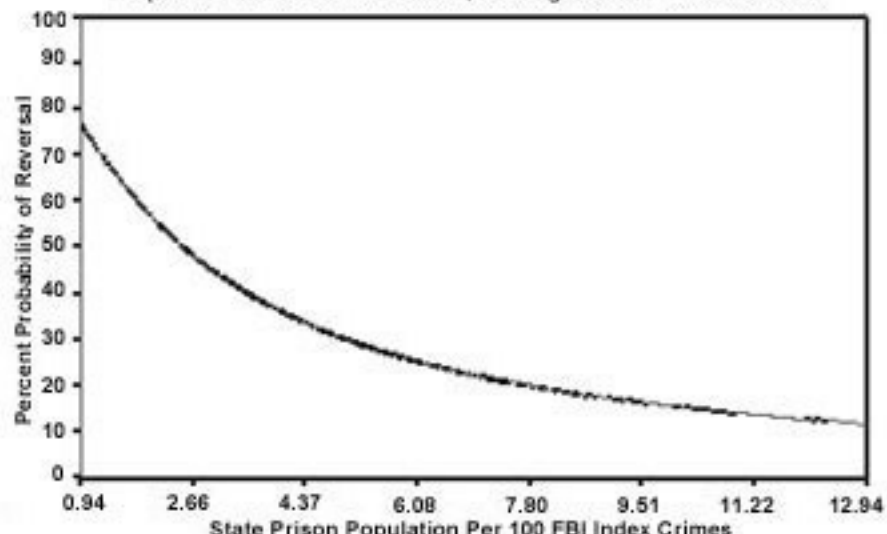


Figure 28C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

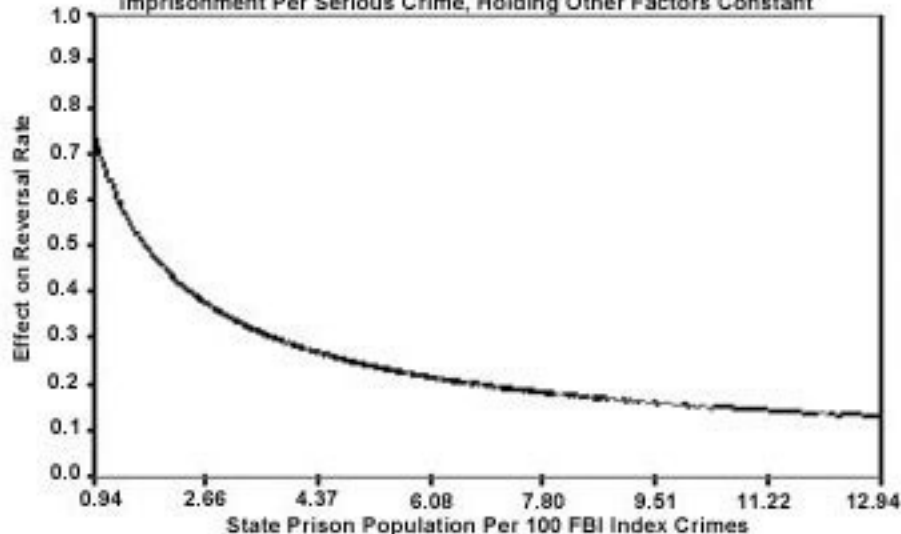
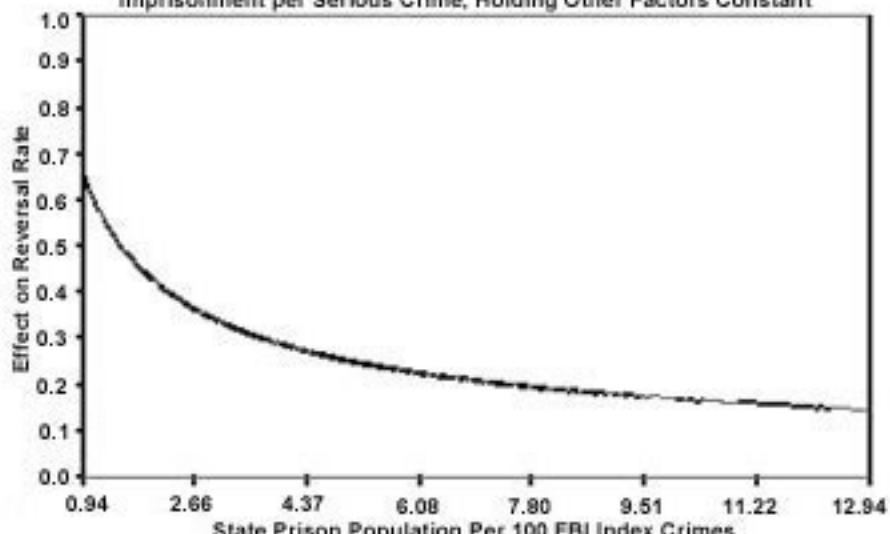


Figure 28D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Rate of Arrest, Conviction and Imprisonment per Serious Crime, Holding Other Factors Constant



v. More political pressure on state judges. Starting in the 1950s with Supreme Court Justice Felix Frankfurter, observers have noted that death penalty cases put officials under intense pressure to use the penalty.³⁹⁷ If that is so, and if the pressures are not always congruent with the quality of the evidence favoring a death verdict—as may occur when the crime is horrible, but the evidence implicating a suspect is weak—capital error may result. As is noted above, we tested this hypothesis by asking whether error rates are related to two alternative gauges of political pressure placed on judges by states’ distinct methods of selecting judges. Each gauge rates state’s judicial selection methods based on a number of traits that may increase the amount of political pressure judges face to conform rulings to the outcome most voters or other politically influential individuals would prefer.³⁹⁸ The first index measures 9 such traits. Study states had anywhere from 2 to all 9 traits, averaging 6.5. The second index measures 8 traits, with states varying between 2 and all 8, and averaging 5.7. Supporting the above hypothesis, one or the other political pressure index was significantly and positively related to error rates in all of our Analysis 1 and 2 analyses.³⁹⁹

Figures 29A-D, p.188 below, show that **increases in the amount of political pressure on state judges are linked to considerable increases in the probability of reversible capital error. In binomial Analyses 1A and 1B (Figures 29A and 29B), states with selection methods putting the least political pressure on judges have expected reversal rates (as a proportion of imposed verdicts) of about 16%. States with selection methods putting the *most* political pressure on judges have 3 times that expected rate of error.** The comparable increase in Poisson Analyses 2A and 2B (Figures 29C and 29D) is between 2 and 3 times. Effect size is similar for the first (9-criteria) and second (8-criteria) political pressure indexes. These results may provide a **reason to use methods to select judges that emphasize professionalism and limit political pressure, or a caution about a penalty that is susceptible to political pressure and resulting mistakes.**

Effect Size: Method of Selecting State Judges (Analyses 1A, 1B, 2A, 2B)

Figure 29A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Method of Selecting State Judges, Holding Other Factors Constant

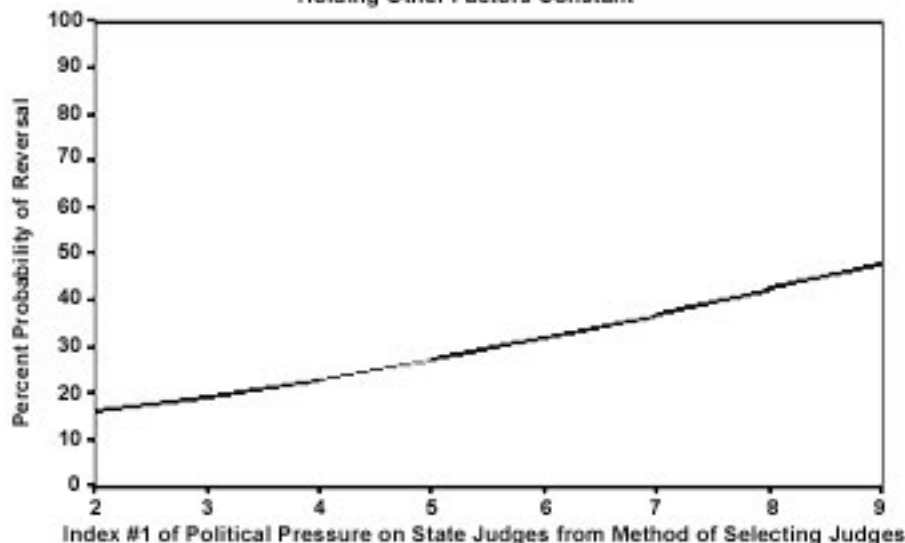


Figure 29B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Method of Selecting State Judges, Holding Other Factors Constant

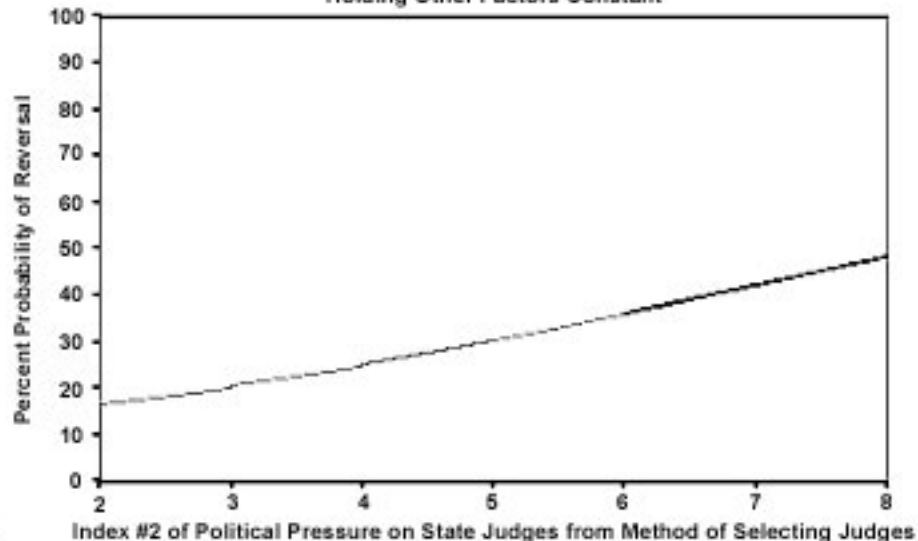


Figure 29C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Method of Selecting State Judges, Holding Other Factors Constant

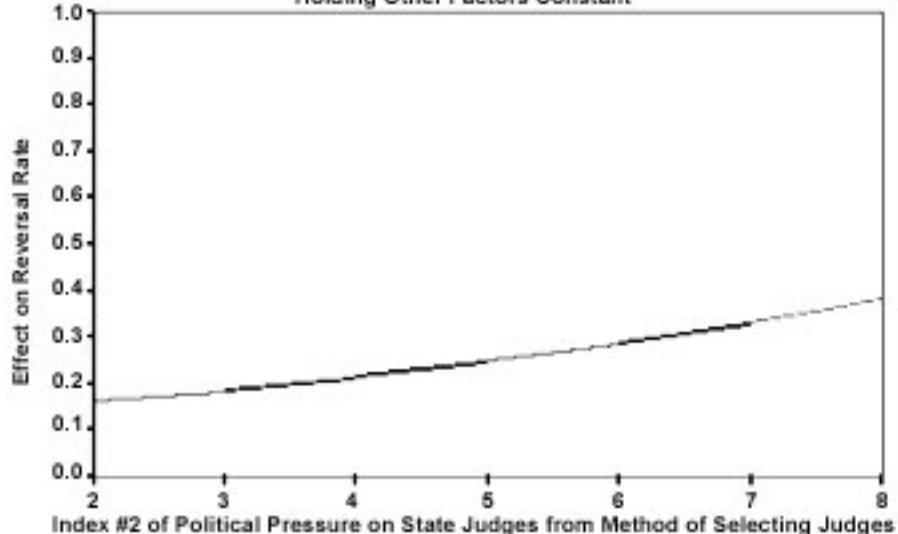
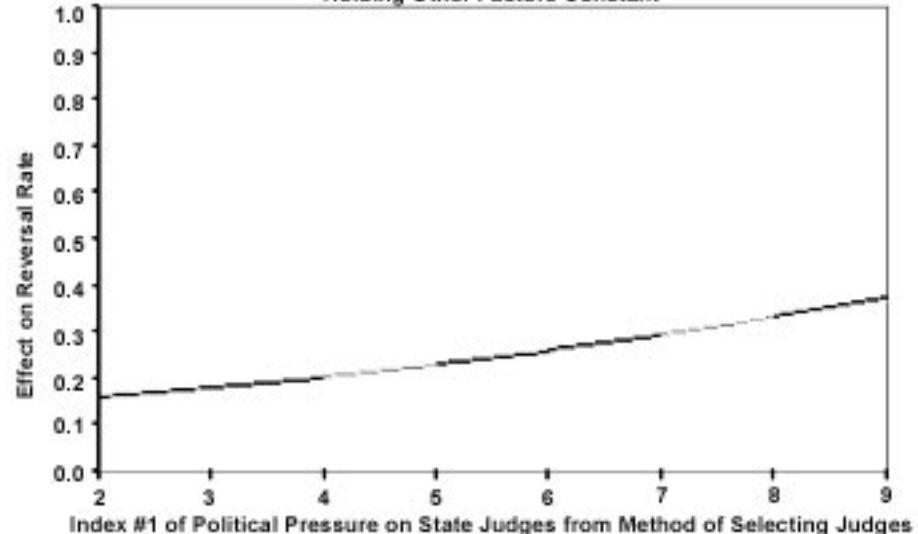


Figure 29D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Method of Selecting State Judges, Holding Other Factors Constant



vi. Higher population size and density. The remaining significant explanatory factor for which effect sizes may be calculated is the size and density of the states' population in the relevant years. The combined scale of population and density is built around a national average score of 0, with highly and densely populated states having scores ranging up to 1.75, with sparsely populated states having scores ranging down to -2.40, and with states averaging slightly above 0 (.175) during the 23-year study period. More heavily and densely populated states have significantly higher reversal rates in these analyses of all three review stages than do more sparsely populated states. As shown by Figures 30A-B, p. 190 below, binomial Analyses 1A and 1B, predict **4- to 5-fold increases in reversal rates when this factor is varied from its lowest to its highest value represented by states in our study, holding other factors constant.** The corresponding increases in Poisson Analyses 2A and 2B (Figures 30C, 30D) are 2- to 3-fold.

Effect Size: Population Structure (Analyses 1A, 1B, 2A, 2B)

Figure 30A (Analysis 1A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Population Structure, Holding Other Factors Constant

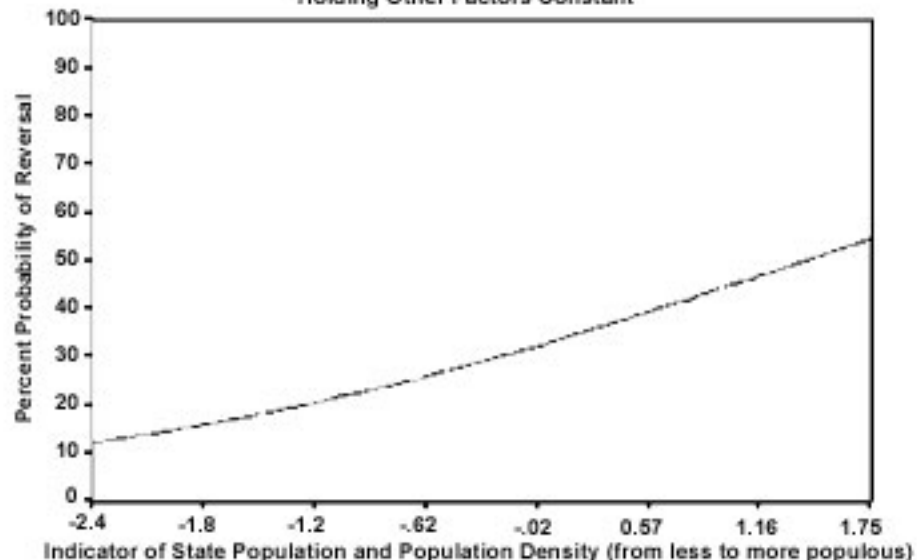


Figure 30B (Analysis 1B): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Population Structure, Holding Other Factors Constant

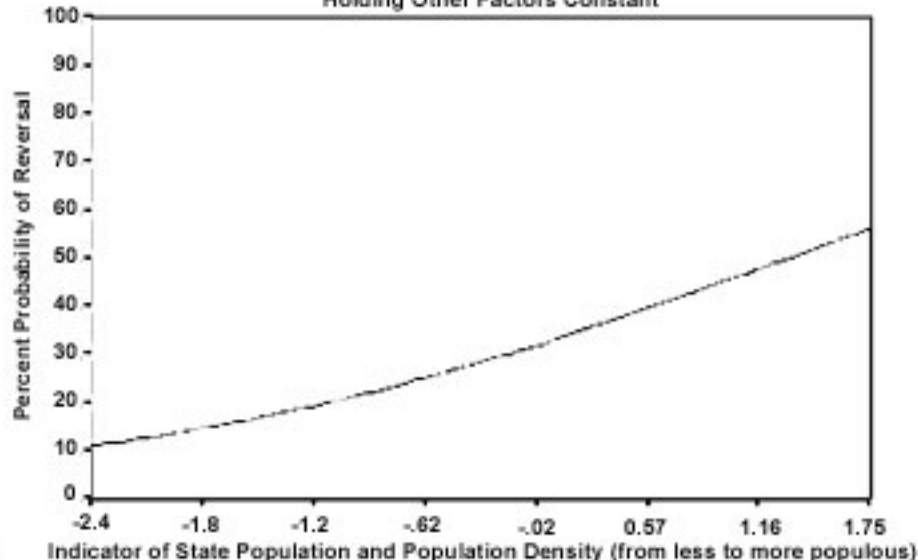


Figure 30C (Analysis 2A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of State Population Structure, Holding Other Factors Constant

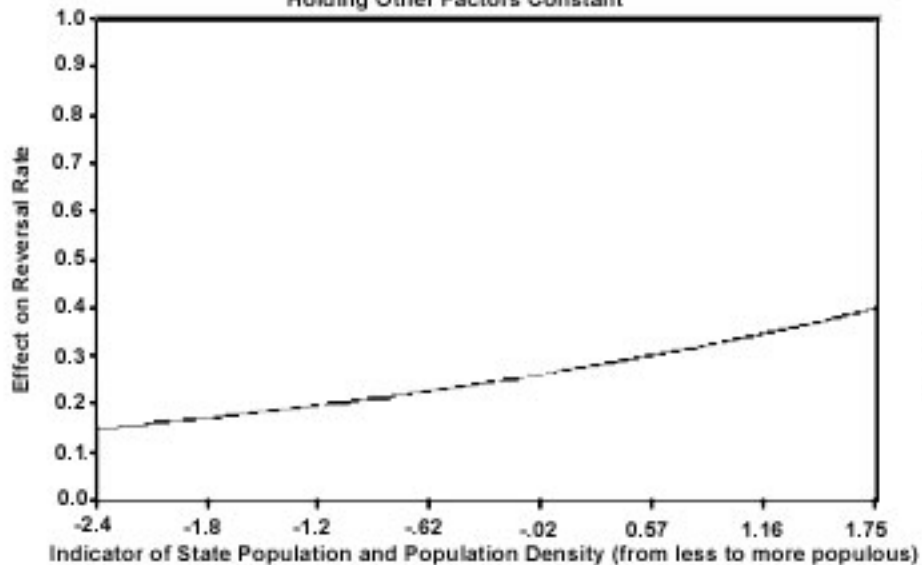
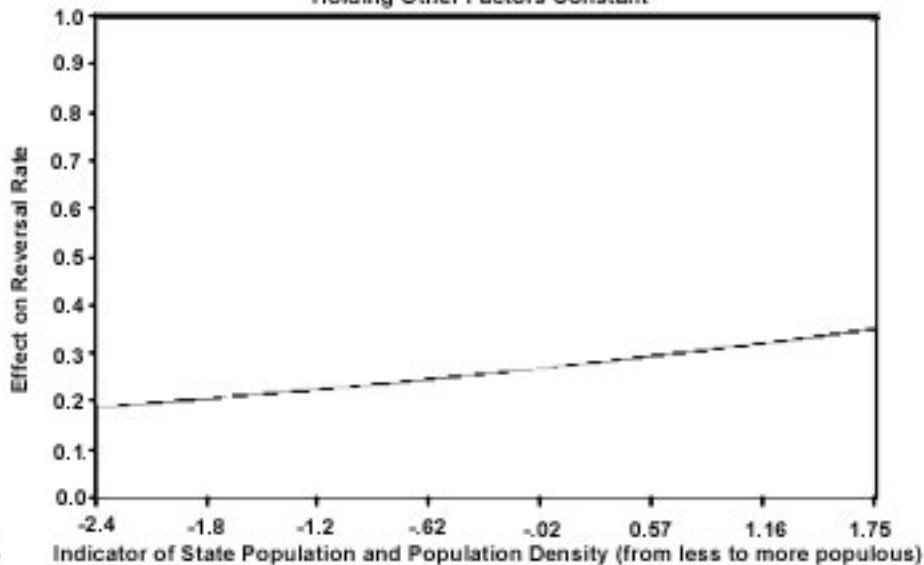


Figure 30D (Analysis 2B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of State Population Structure, Holding Other Factors Constant



d. Summary: evidence of the role of race, politics and zeal for the death penalty.

Except for general court caseloads, all factors that are significantly related to higher rates of serious capital error and for which effect sizes can be calculated have appreciable effects. This is true of:

- the efficiency with which capital appeals are processed, which controls for the non-error-related effect on reversal rates of delay;
- the year in which the death verdict was imposed, which controls for the non-error-related effect of unfinished appeals;
- the relative size of the African-American community;
- how closely the homicide risk to whites approaches that to blacks;
- how frequently states punish homicides with death;
- how often states fail to apprehend, convict and imprison serious criminals;
- the amount of political pressure judicial selection methods place on state judges; and
- the size and density of the state's population.

The listed factors may all warrant attention from policy makers seeking solutions to high capital reversal rates. Before adopting this conclusion, we consider whether our other analyses support them.

C. Results of Analyses 3-6 of Single Stages of Review: More Evidence of the Effects of Race, Politics, Frequent Use of the Death Penalty and Weak Law Enforcement

We next consider analyses that alter those just discussed in two important ways:

- They examine only one review stage at a time, not all three stages at once.
- They examine reversal rates among only actually reviewed verdicts imposed in the relevant state and year (or, in the case of state post-conviction Analysis 5, among verdicts available for review), rather than reversal rates among all verdicts *imposed* in the relevant state and year.

Both changes reduce the number of death verdicts available for study, and thus the number

of states and years with reversal rates to study. This accounts for the drop in the number of observed reversal rates from 519 in Analyses 1 and 2 above, to 453 in Analyses 3 and 4, and to 354 in Analysis 5 and 161 in Analysis 6. Because statistical significance is affected by the number of observations, these declines in the number of observations make it more difficult to obtain useable results.

Two other, more favorable effects of these changes explain why we use them to check the reliability of main Analysis 1 and Analysis 2. First, by altering choices made in designing the main analysis, Analyses 3-6 test whether those choices, not actual relationships in the data, account for the main analysis' results. For example, by studying the results of all three stages at once, Analyses 1 and 2 maximize the amount of data being analyzed but might obscure conditions operating only at a single stage. Studying each stage by itself thus checks the completeness and power of the explanations Analyses 1 and 2 identify. Analyses 1 and 2 also maximize the data being analyzed by using a deflated measure of reversal rates—reversals as a proportion of imposed, rather than only reviewed, death verdicts.⁴⁰⁰ Analyses 3, 4 and 6 alter this feature by analyzing factors related to true error rates, *i.e.*, reversals as a proportion of verdicts actually reviewed.

Second and relatedly, some of our stage-specific analyses more reliably assess the effect of time at the state direct appeal stage. As noted, Analyses 1 and 2 explain differences in reversal rates among all imposed verdicts, whether or not they were finally reviewed. This leads reversal rates in later periods to be lower than would otherwise be the case, not because later verdicts were less seriously flawed, but because there was less time to review them, so that fewer of those with flaws were reversed by the study's end date.⁴⁰¹

Analyses 3 and 4 avoid this bias by studying only verdicts that were actually reviewed on direct appeal. Federal habeas Analysis 6 avoids the same time bias in the same way, but is susceptible

to a separate bias against a full accounting of flawed verdicts imposed later in the study period. As we demonstrate above, federal habeas reversals take longer to occur than affirmances. See Figure 10, p. 93 above. As a result, a disproportionately high number of flawed (as opposed to unflawed) verdicts that were imposed in the study period were not fully reviewed and reversed on federal habeas by the end of that period.⁴⁰² State post-conviction Analysis 5 is subject to the same time bias as our Analyses 1 and 2 of the three review stages combined, because we could only measure reversal rates at that stage against verdicts *available* for review, even if they were not actually reviewed.⁴⁰³

1. Direct appeal Analyses 3 and 4.

a. Tests of the analyses' ability to generate reliable results.

The results of the diagnostic tests in state direct appeal Analysis 3 (a binomial regression analysis) and state direct appeal Analysis 4 (a Poisson regression) show that:

- Even after accounting for the effect of state, year and the passage of time, there was a significant amount of variation in capital reversal rates from state to state and year to year, warranting consideration of more specific factors that might explain the differences.⁴⁰⁴

In addition, all five best analyses of more specific factors in Analyses 3 and 4—3A, 3B and 4A-4C:

- did a significantly better job of fitting the predicted reversal rates to the actual ones than was true of the baseline analyses of only state, year and time trend;⁴⁰⁵
- left substantially less unexplained variance than the baseline analyses;⁴⁰⁶
- identified a series of specific factors that are significantly related to reversal rates; and
- fit the data better and left less unexplained variance than other sets of significant factors.

The significant explanatory factors identified by Analyses 3 and 4 also:

- largely overlap each other, across two different kinds of analyses (binomial logistic vs. Poisson logarithmic regression analyses), and
- generate nearly the same set of significant explanatory factors as Analyses 1 and 2,

notwithstanding the different stages of review being studied (all three stages in Analyses 1 and 2; only state direct appeal in Analyses 3 and 4) and the different definition of the reversal rates being compared (rates of *imposed* verdicts that were reversed, in Analyses 1 and 2; rates of *reviewed* verdicts that were reversed, in Analyses 3 and 4).

These tests suggest the reliability of Analyses 3 and 4, and tend to confirm the results of main Analysis 1 and Analysis 2.

b. Factors significantly related to higher capital reversal rates.

We discuss explanatory factors in the same order here as in discussing Analyses 1 and 2 above.

i. Lower court backlogs. We included capital and general court backlogs in Analyses 1 and 2 to control for the effect of unreviewed death verdicts on reversal rates measured as a ratio of *imposed* (even if not reviewed) death verdicts. Because Analyses 3 and 4 examine only *reviewed* verdicts, we expected court backlogs to be less important in these analyses, and they were. General (*i.e.*, non-capital) backlogs had no significant effect at all. Capital backlogs are still significantly and negatively related to reversal rates,⁴⁰⁷ but effect size is somewhat smaller when only actual direct appeal decisions are considered.⁴⁰⁸ The continuing significance of capital backlogs suggest that **large amounts of death verdicts awaiting review limit either the capacity of overwhelmed appellate courts to find and reverse flawed verdicts, or their willingness to do so in the face of public unhappiness at how slowly capital cases move through the courts.**

ii. Earlier death verdicts. For reasons given above, **Analyses 3 and 4 are our most reliable analyses of the effect of the passage of time on capital-error rates, after accounting for other factors.**⁴⁰⁹ Their results in regard to this factor are important, therefore—and interesting. In the baseline analysis of only state, year and time trend, the passage of time is significantly, and negatively, related to direct appeal reversal rates, suggesting that later death verdicts are less likely to be reversed

than early verdicts. But, when other, more specific factors are considered—appreciably enhancing the analyses’ explanatory power, given (1) significantly improved fit with the actual data, (2) a sizeable drop in the amount of unexplained variance and (3) an expanded range of significant explanations for reversible error—the passage of time turns out to have a *positive* relationship to error rates that is far more powerful than the opposite relationship that initially appeared in the baseline inquiry.⁴¹⁰

Accounting for other significant factors associated with reversal rates, death verdicts imposed later in time were significantly more likely to be reversed on direct appeal due to serious error than verdicts imposed earlier in time. The effect size for this factor is as large as that of any other factor in any of our analyses: If all other explanatory factors had remained constant at their averages, Analysis 3A predicts a reversal rate for death verdicts imposed in the early years of the study of about 9%, rising to 80% for verdicts imposed at the end of the study period. See Figure 32A (binomial Analysis 3A), p. 202 below. See also Figure 32B (Poisson Analysis 4A), p. 202 below, predicting a similar 8- to 9-fold increase. Using the numerical effect-size estimates, this translates into a prediction by Analysis 4A that, apart from the effect of the other factors in the analysis, error rates would have increased about 9% per year over the 23-year period.

Of course, other factors did not stay constant at their averages, and as Figures 2C and 2D, pp. 57-58 above, reveal, overall reversal rates in fact fluctuated considerably during the first part of the study period, then remained fairly constant during the latter half of the study period. Under these circumstances, the result discussed here indicates that:

- changes over time in the other significant factors identified by Analyses 3 and 4 predict a drop in reversal rates over time that did not occur;
- other time-sensitive factors—registered in Analyses 3 and 4 by the general time-trend factor—are associated with large *increases* over time in the amount of reversible error found on direct

appeal; and

- the downward influence of the specific factors identified by Analyses 3 and 4 and the upward influence of the time-sensitive factors registered by the general time-trend combined to produce the flat pattern of rates over the latter half of the study period.

From a policy perspective this suggests that, **even if reforms based on the other factors identified by Analyses 3 and 4 had a downward effect on reversal rates, the factors registered in our analyses by time trend might nonetheless continue to have the opposite effect, with the overall result that reversal rates might abate less than one would hope, or not at all.**

iii. Larger African-American population. In all but one of the groups of factors in Analyses 3 and 4, the proportion of states' population made up of African-Americans was significantly related to capital error rates.⁴¹¹ In one analysis, the factor was just above the .05 level.⁴¹² As in Analyses 1 and 2, **the larger the relative size of the black community, the higher the rate of serious capital error found on state direct appeal.** Analysis 3A predicts close to a doubling of reversal rates across the spectrum of African-American populations as a proportion of the general population in states and years in our study, other factors held constant. See Figures 33A and 33B, p. 203 below.

iv. Relatively high homicide threat to whites relative to blacks. In Analyses 3 and 4, as in Analyses 1 and 2, **the higher the risk of homicide to a state's white community relative to its black community, the higher the probability that the state's death verdicts will be reversed due to serious error.** The relationship is significant in Analyses 3A, 4A and 4B, and barely above significance in 3B and 4C.⁴¹³ Holding other factors constant, Analyses 3A and 4A predict that reversal rates will more than double as the homicide threat to whites relative to the threat to blacks rises from its lowest to highest levels among states in the study. See Figures 34A and 34B, p. 204 below.

v. *The interaction of a large African-American population and a relatively high threat of homicide to whites as compared to blacks.* We tested this interaction effect in Analyses 3A, 4A and 4B. In all three analyses, the direction of the relationship was the same as in Analyses 1 and 2. States with large black communities and high homicide risks to their white communities relative to their black communities tend to have especially high reversal rates, even after accounting for the independent effect of the two component factors. Significance levels are unimpressive, however, though high enough to be of some interest.⁴¹⁴

vi. *Heavy use of the death penalty. In all five sets of factors in Analyses 3 and 4, states with higher death-sentencing rates had higher rates of flawed death verdicts. The relationship was highly significant in all five analyses. Effect size is large.* In Analysis 3A, holding other factors constant, **expected direct appeal reversal rates are about 15% where the death penalty is used most sparingly, but over 65% where it is used most often.** See Figure 35A, p. 205 below. Analysis 4A predicts close to a 4-fold increase in reversal rates across that same spectrum. See Figure 35B, p. 205 below. The largest expected changes in reversal rates are associated with fairly modest changes in death-sentencing rates on either side of the average rate of 25 death verdicts per 1000 homicides. Holding other factors at their average, Analysis 3A (Figure 35A, p.205 below) predicts that:

- States imposing 5 death verdicts per 1000 homicides will have a direct appeal reversal rate of about 27%.
- States imposing the average number of death verdicts per 1000 homicides (25) will have a direct appeal reversal rate of over 40%.
- And states sentencing 50 people to die for every 1000 verdicts—twice the national average, but only a quarter of the top death-sentencing rate—will have a reversal rate of over 50%.

vii. *Fewer serious criminals apprehended and imprisoned.* Throughout Analyses 3 and 4, as

in Analyses 1 and 2, **death verdicts imposed by states that apprehend, convict and imprison fewer criminals per serious crimes are *more* likely to be overturned due to serious, reversible error than death verdicts imposed in states with higher rates of apprehension, conviction and incarceration.** The relationship is highly significant in all five analyses, and effect size is quite large. **Where the rate of apprehension, conviction and incarceration of serious criminals is at the highest level among states and years in our study, Analysis 3A predicts direct appeal reversal rates of only about 12%, other factors constant. But where the apprehension-conviction-incarceration rate is at its lowest level for states and years in the study, the predicted rate of serious capital error found on direct appeal is over 80%. Analysis 4A predicts about a 5-fold increase in reversal rates across that spectrum. See Figures 36A and 36B, p. 206 below.**

viii. More political pressure on state judges. Analyses 3 and 4 are limited to rates of reversal by mainly elected state high court judges reviewing verdicts imposed at trials supervised by other, mainly elected state judges. If, as was found by Analyses 1 and 2—which examine decisions of unelected federal judges as well as elected state judges—increases in the amount of political pressure on state judges are related to increases in the frequency of serious capital error, one would expect this relationship to be somewhat weaker when only the decisions of elected state appellate judges are studied. Because the political pressure we measure—caused by judicial selection techniques—is similar for *all* state judges, appellate as well as trial judges, the same pressures that drive trial-level judges to make errors at particular trials might be expected to discourage appellate judges from reversing the resulting verdicts. Still, the political pressures a local community concentrates on local judges in the immediate wake of homicides probably weigh less heavily on appellate judges who review cases years after homicides occur, live elsewhere than the community harmed by the offense,

and have constituents from many places besides that community. If political pressures are related to high rates of trial error, therefore, the patterns of direct appeal reversals in states where those pressures are stronger and weaker should probably provide some evidence of that relationship.

Subject to these considerations, the results of Analyses 3 and 4 are consistent with those of Analyses 1 and 2: **Higher levels of political pressure on judges are associated with higher levels of serious capital error identified at the direct appeal stage.** But significance levels are less impressive—.06 for Analyses 3A, 4A and 4B, and over .1 for Analyses 3B and 4C.⁴¹⁵ Effect size is moderate, with reversal rates expected to double when the political pressure on judges rises from the lowest to the highest levels among states in our study and other factors are held constant. See Figures 37A (Analysis 3A) and 37B (Analysis 4A), p. 207 below.⁴¹⁶

ix. Greater population size and density. At the direct appeal stage studied in Analyses 3 and 4, as at the three review stages combined in main Analysis 1 and Analysis 2, the size and density of a state's population was significantly associated with the likelihood of reversible error.⁴¹⁷ **Verdicts from highly and more densely populated states were more likely to be reversed than those from more sparsely populated states.** Effect size is about the same as in Analyses 1 and 2. Compare Figures 38A and 38B, p. 208 below, to Figures 30A-30D, p. 190 above. Analysis 3A (Figure 38A) predicts **a rise in direct appeal reversal rates from about 16% to nearly 60% as population structure goes from the least to the most populous among states in our study, other things equal.**

x. A new explanatory factor: low spending on state courts. One explanatory factor with a highly significant relationship to error rates in our direct appeal Analyses 3 and 4 was not significant in Analyses 1 and 2, covering all three review stages: States' per capita direct expenditures on their court systems.⁴¹⁸ As expected, **death verdicts imposed by states that spend less on their courts per**

capita are more likely to be reversed on direct appeal due to serious error than verdicts imposed by states that spend more on their courts.⁴¹⁹ Effect size is large, especially among states spending less than the average of about \$1.75 per resident.⁴²⁰ **Holding other factors constant, Analysis 3A predicts a rise in direct appeal capital reversal rates from 25% for jurisdictions with per capita court expenditures at the average for states in our study, to 74% for jurisdictions with per capita expenditures at the lowest extreme among states in our study.** See Figure 39A (Analysis 3A), p. 209 below. Although all states support a high court, they probably vary substantially in the amounts they spend on local courts. **Evidently, states that spend the least on their courts place an especially heavy quality-control burden on their high courts, which is reflected in higher capital reversal rates on direct appeal.**

Effect Size: Backlog of Capital Verdicts (Analyses 3A, 4A)

Figure 31A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

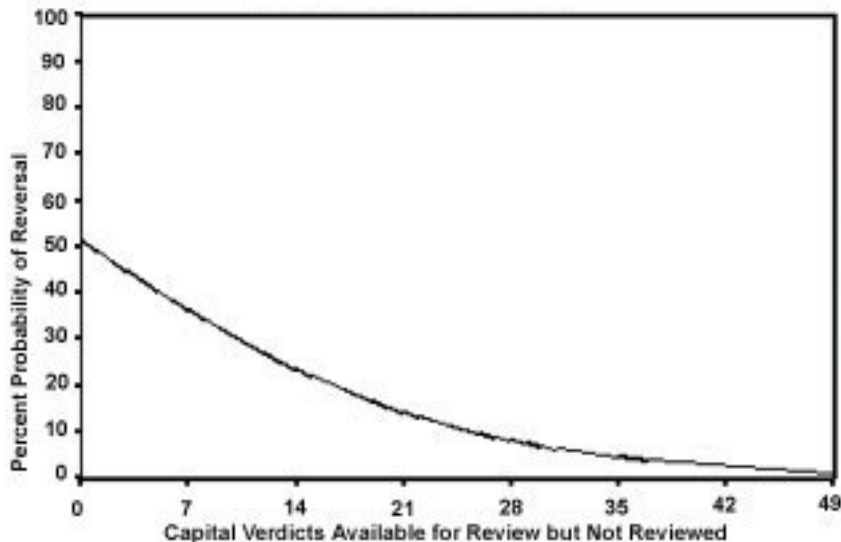
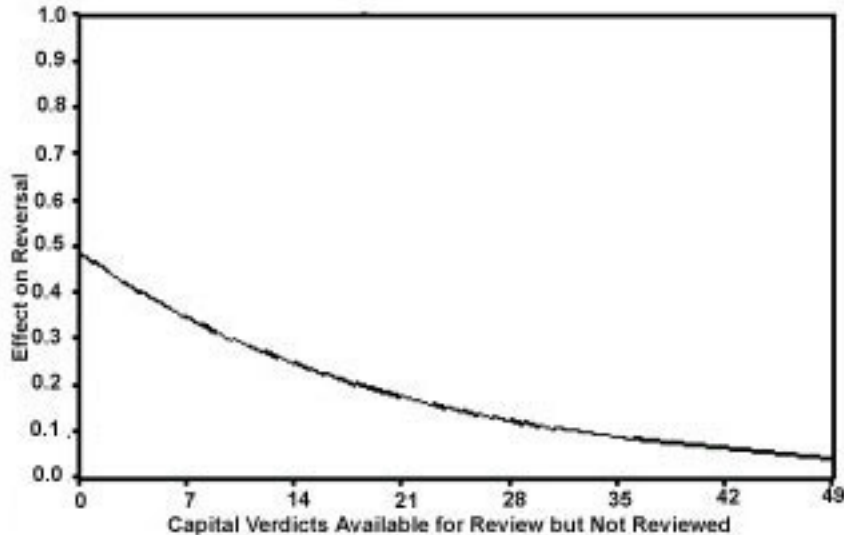


Figure 31B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant



Effect Size: Year of Death Verdict (Analyses 3A, 4A)

Figure 32A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts, direct appeal stage) Based on Year Death Sentence Was Imposed, Holding Other Factors Constant

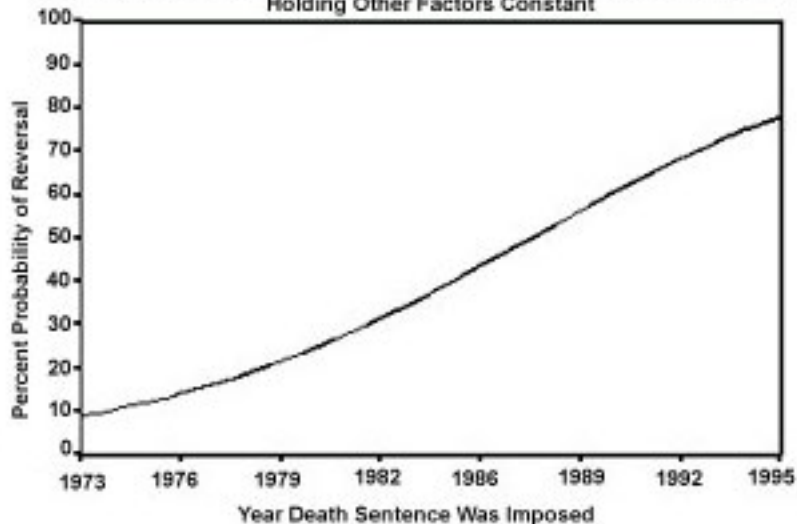
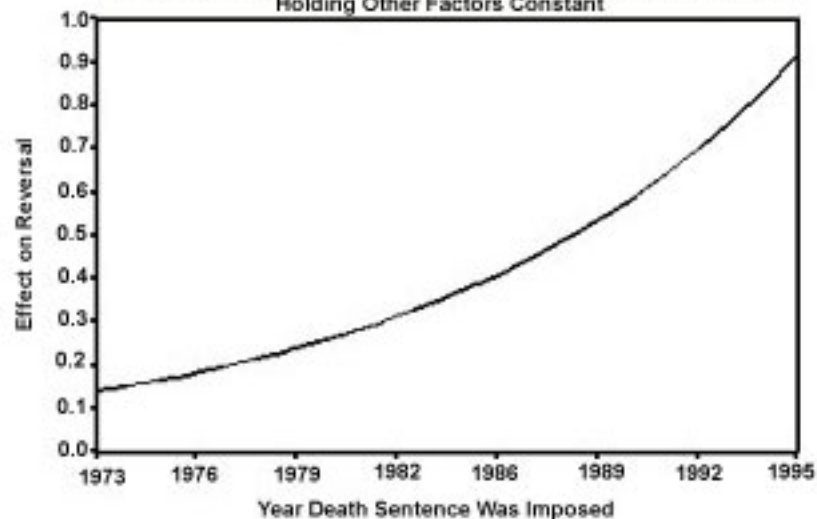


Figure 32B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of Year Death Sentence Was Imposed, Holding Other Factors Constant



Effect Size: Proportion of African-Americans in State Population (Analyses 3A, 4A)

Figure 33A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Proportion of African-Americans in State Population, Holding Other Factors Constant

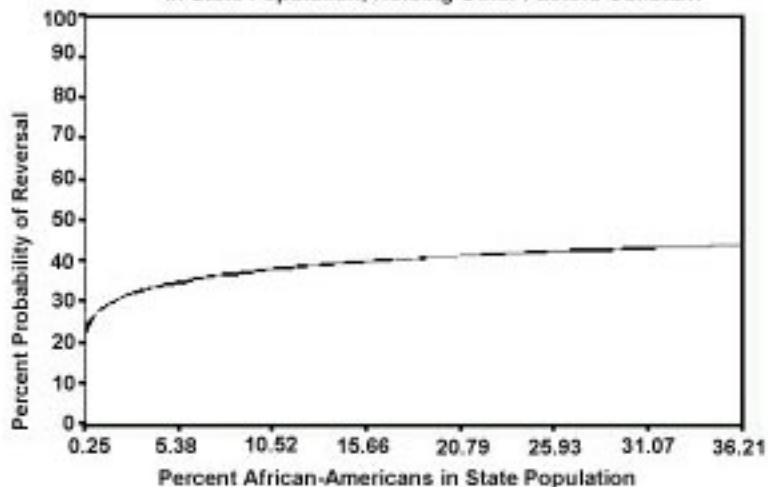
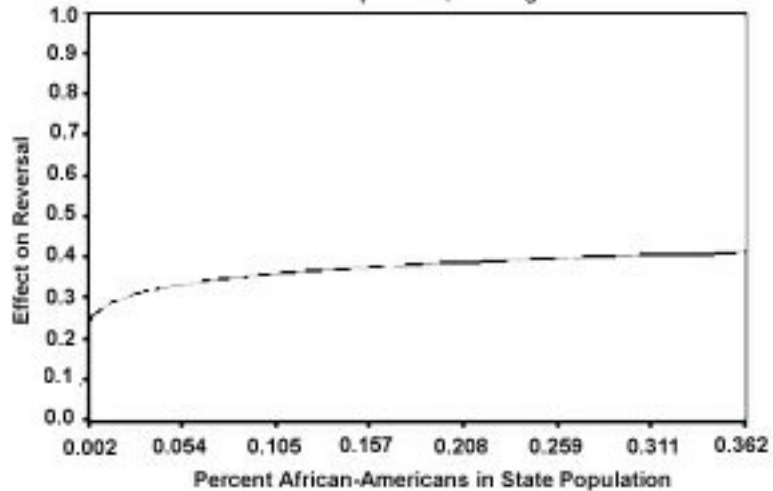


Figure 33B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of Proportion of African-Americans in State Population, Holding Other Factors Constant



Effect Size: Risk of Homicide to Whites Versus Blacks (Analyses 3A, 4A)

Figure 34A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant

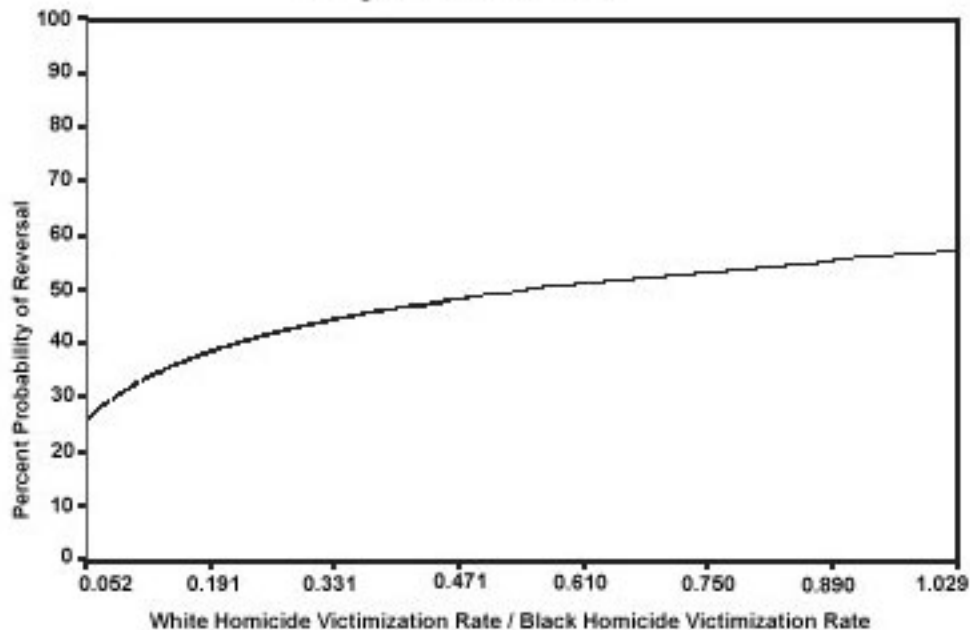
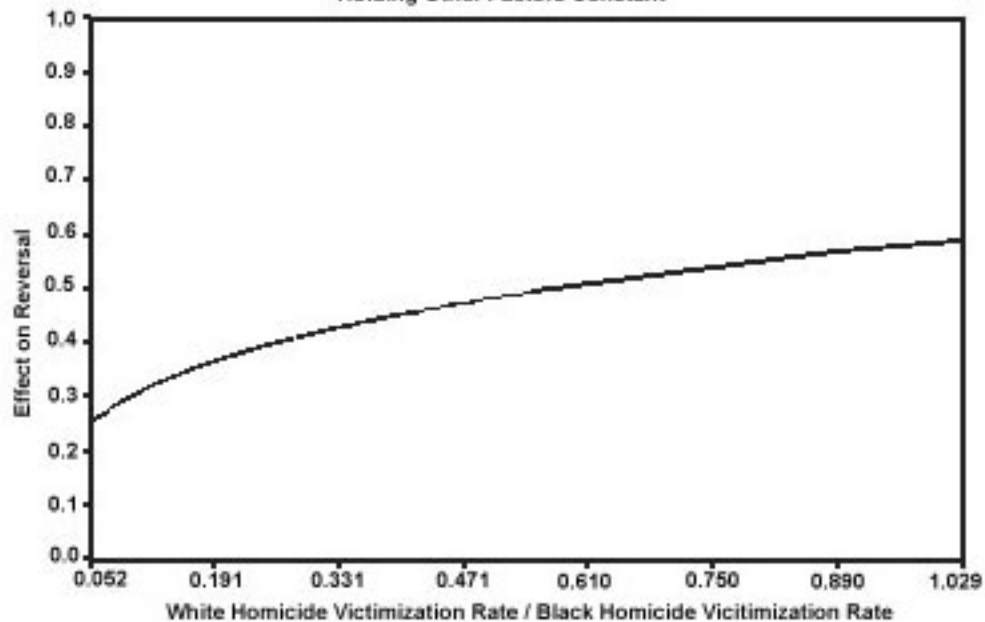


Figure 34B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant



Effect Size: State Death Sentencing Rate (Analyses 3A, 4A)

Figure 35A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on State Death Sentencing Rate, Holding Other Factors Constant

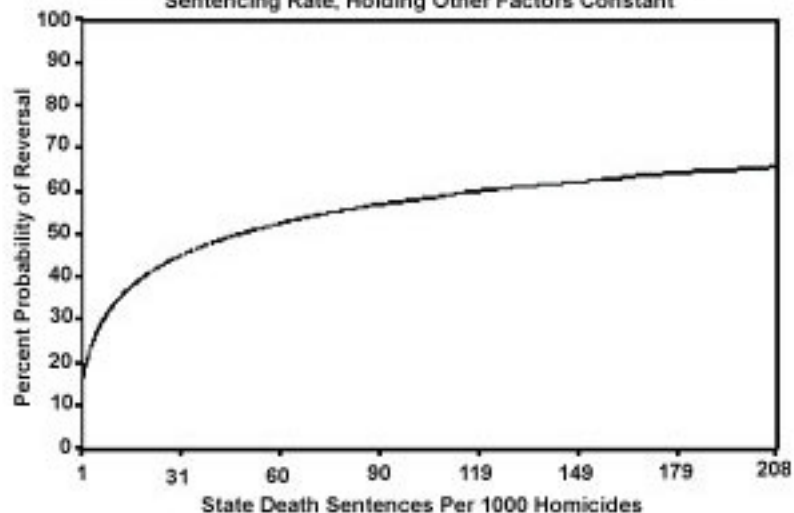
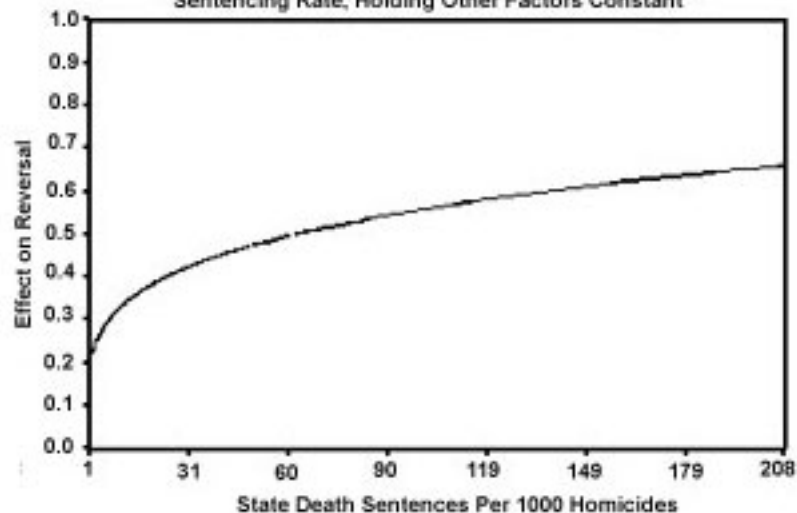


Figure 35B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of State Death Sentencing Rate, Holding Other Factors Constant



Effect Size: Rate of Arrest, Conviction and Imprisonment Per Serious Crime (Analyses 3A, 4A)

Figure 36A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

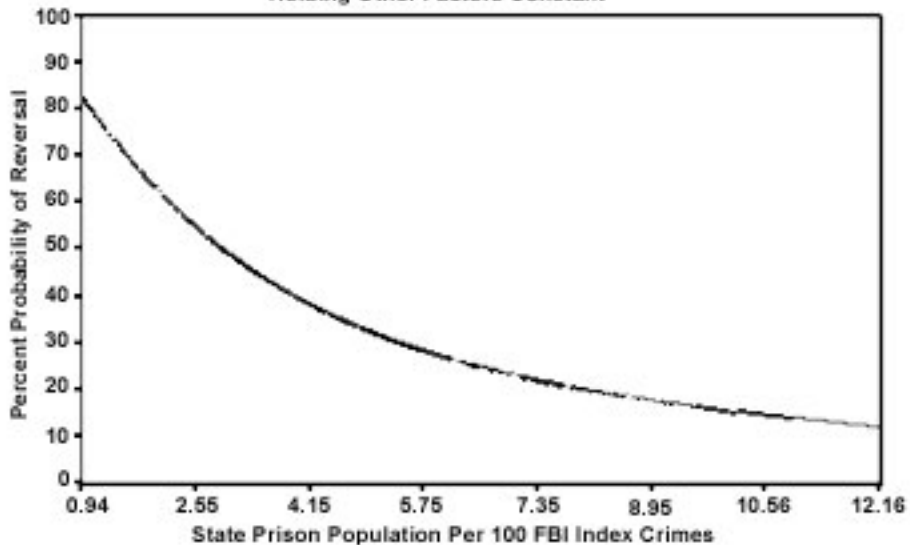
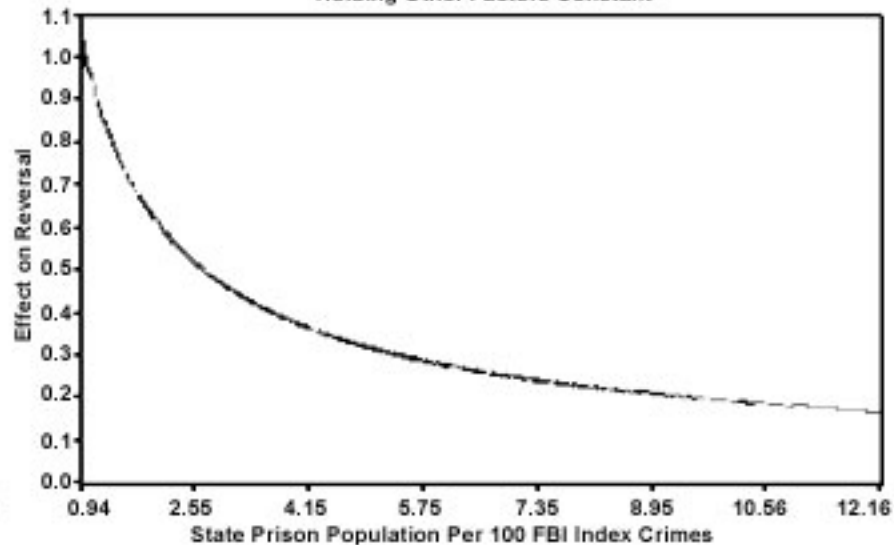


Figure 36B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant



Effect Size: Method of Selecting State Judges (Analyses 3A, 4A)

Figure 37A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Method of Selecting State Judges, Holding Other Factors Constant

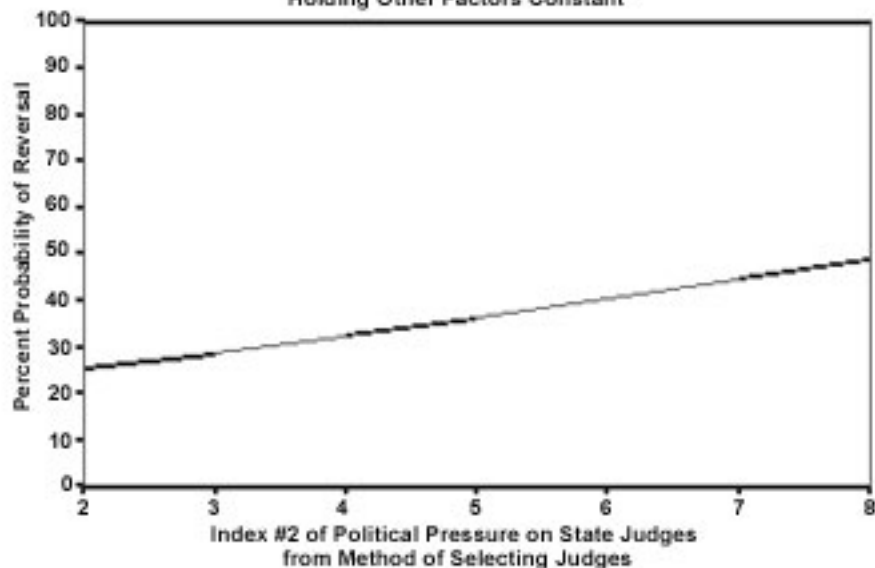
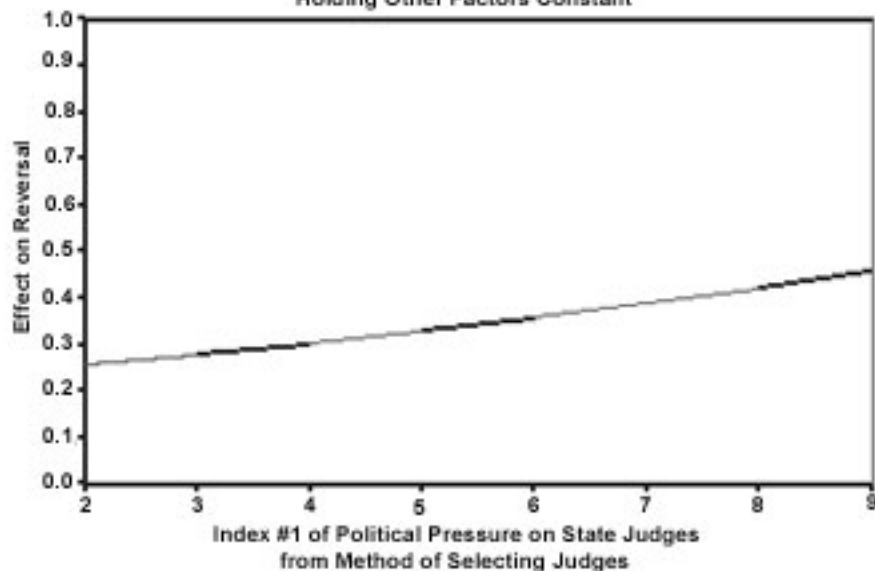


Figure 37B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of Method of Selecting State Judges, Holding Other Factors Constant



Effect Size: Population Structure (Analyses 3A, 4A)

Figure 38A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on State Population Structure, Holding Other Factors Constant

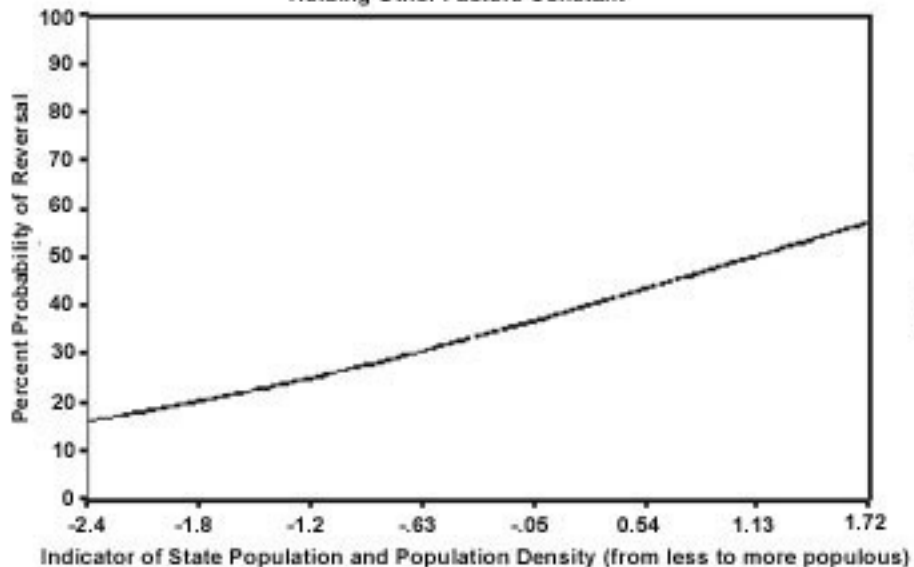
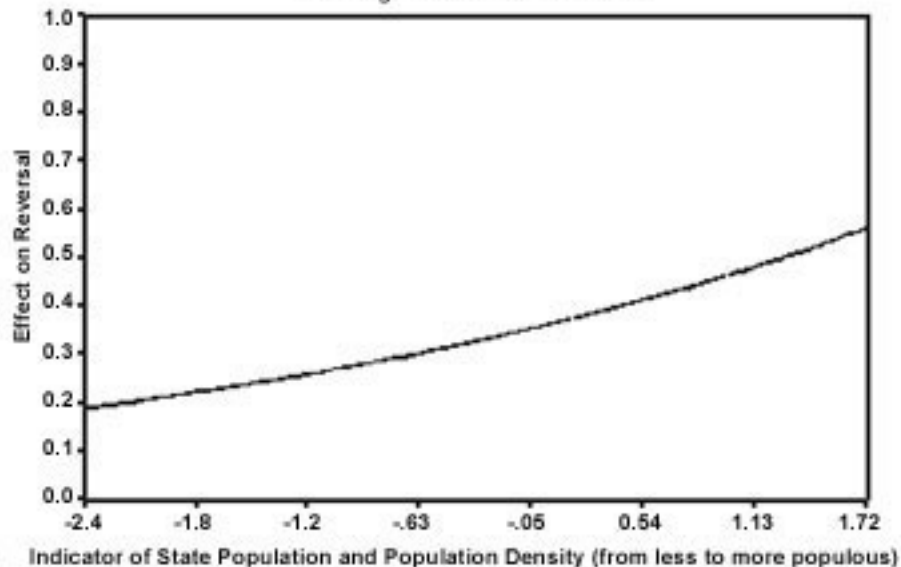


Figure 38B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts, direct appeal stage) of State Population Structure, Holding Other Factors Constant



Effect Size: Per Capita Spending on State Courts (Analyses 3A, 4A)

Figure 39A (Analysis 3A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on State Per Capita Spending on Courts, Holding Other Factors Constant

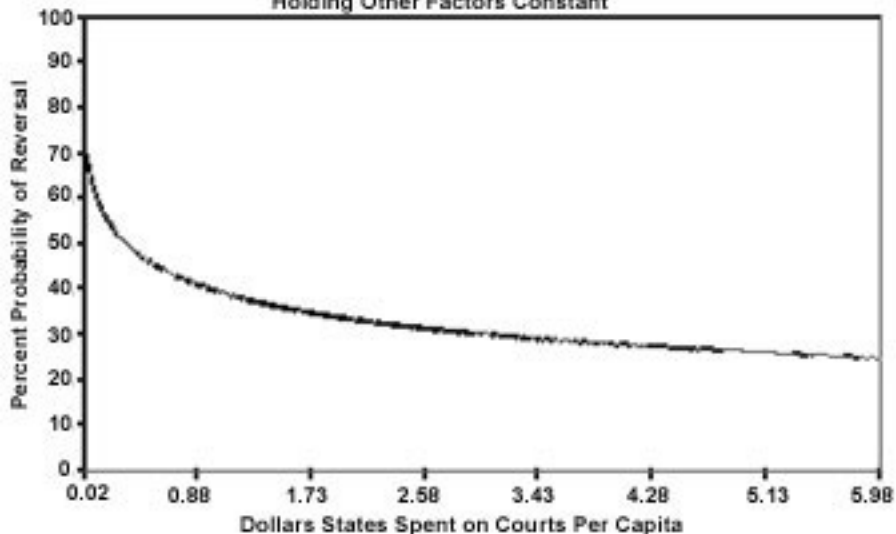
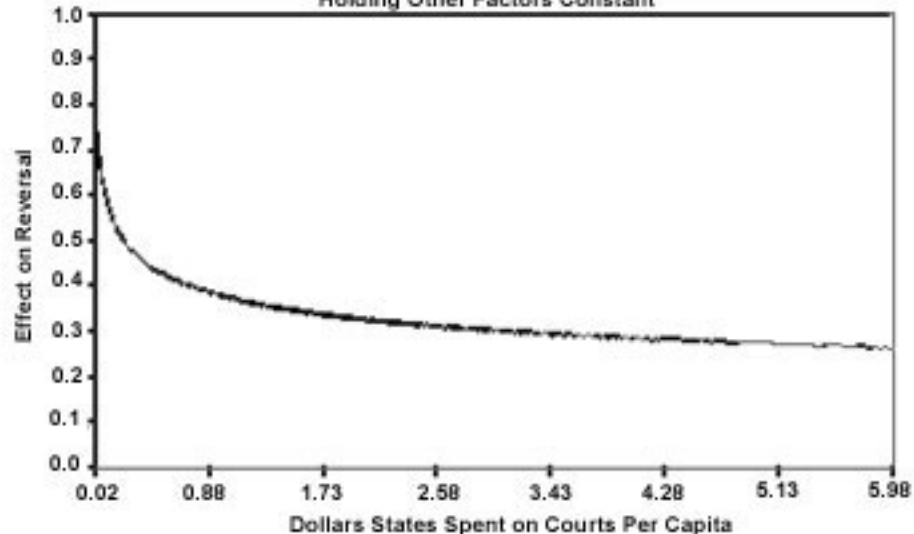


Figure 39B (Analysis 4A): Effect on Reversal Rate (as proportion of reviewed verdicts; direct appeal stage) of State Per Capita Spending on Courts, Holding Other Factors Constant



c. **Summary: consistent results across Analyses 1-4.**

Changing the condition under study from the rate of *imposed* death verdicts that are reversed to the rate of only *reviewed* verdicts that are reversed, and focusing only on a single, state-court stage of review rather than on the combined results at all three review stages, did not change the results. Neither did a 13% decrease in the number of states and years under study from 519 to 453. Instead, the results of Analyses 3 and 4 are strikingly similar, not only to each other, but also to the results of Analyses 1 and 2. All four analyses indicate that:

- **Aggressive use of the death penalty is strongly associated with high capital error rates.**
- **Factors that can heighten the fear of serious crime among people with influence over public officials—encouraging officials to use the death penalty even when the evidence is weak—also are associated with high capital error rates. Those factors include:**
 - **the size of the community’s African-American population;**
 - **the extent to which the homicide risk to members of the white community approaches or surpasses the risk to members of the black community; and**
 - **the weakness of the state’s non-capital response to crime, as measured by the rate at which serious criminals are arrested, convicted and incarcerated.**
- **Error rates are higher in states that select capital trial judges in ways that increase the pressure on them to conform their rulings to popular sentiment.**
- **Large pile-ups of cases awaiting review are associated with lower review and reversal rates.**
- **The more reliable the measure of the effect of the passage of time on the quality of capital verdicts, the stronger the evidence that, after controlling for other factors, later verdicts are more likely to be reversed due to serious capital error than earlier verdicts.**
- **Error rates for death verdicts from populous and urbanized states are higher than those from sparsely populated states.**
- **High error rates are associated with evidence that state courts are overburdened—although at the three review stages combined (Analyses 1 and 2), the indicator that the**

courts are overburdened is the combination of heavy capital and general caseloads, and at the direct appeal stage (Analyses 3 and 4), the indicator is under-funding.

Analyses 3 and 4 thus provide strong confirmation that the relationships identified by main Analysis 1 and Analysis 2 are present in the data collected on reversal rates and potentially explanatory conditions, and are not a function of particular study methods.

2. State post-conviction Analysis 5.

a. Tests of the analysis's ability to generate reliable results.

Analysis 5 is a Poisson regression analysis of state post-conviction reversals as a proportion of all death verdicts available for review at that stage.⁴²¹ Relatively few verdicts became available for state post-conviction review because so many were reversed at the prior, direct appeal stage, and so many others were still awaiting review at that stage.⁴²² The result is fewer states and years in which there was at least one verdict available for state post-conviction review, and thus fewer reversal rates to study. The 354 observed rates in Analysis 5 are a third fewer than the 519 observations in Analyses 1 and 2, and 22% fewer than the 453 observations in Analyses 3 and 4. Fewer observations make it harder to achieve the required degree of confidence about the significance of apparent relationships between reversal rates and explanatory factors.

These limitations led us to doubt that we could obtain useful information by separately studying the state post-conviction phase. The reliability tests we use support these doubts to a degree, but less than we expected. Cutting against confidence in the results, our two best Analysis 5 sets of factors—Analyses 5A and 5B—fit the reversal rate data no better (nor any worse) than the baseline analysis of state, year and time trend.⁴²³ On the other hand, the two best analyses identify significant explanatory factors that modestly reduce the amount of state-to-state variability left unexplained by

the baseline inquiry and that are estimated to have substantial effect size.⁴²⁴ Because the significant Analysis 5 explanatory factors are the same as or similar to significant factors identified by Analyses 1-4, Analysis 5 provides some additional evidence of the importance of those factors, while permitting less confident conclusions about the state post-conviction stage itself.

b. Significant explanatory factors.

Three factors that were significant in Analyses 1-4 were not significant in Analysis 5: the proportion of the population that is African-American; rates of punishing serious criminals; and methods of selecting judges. The non-significance of the last factor is probably explained in part by the fact that state post-conviction judges typically review verdicts imposed at trials they or other trial judges supervised. If political pressures in a given state dispose trial judges to permit flawed capital trials, the same pressures also likely deter the same judges from identifying and curing those flaws.⁴²⁵

The significant Analysis 5 factors are discussed below.

i. Backlogs of capital verdicts awaiting state post-conviction review. In Analysis 5, as in Analyses 1-4, a measure of backlogged death verdicts is negatively associated with reversal rates. The measure here—the number of capital verdicts available for review at the state post-conviction stage minus the number reversed at that stage—is a stage-specific analogue of the three-stage measure used in the other analyses (the number of verdicts awaiting review at all three stages combined). As in Analyses 1 and 2, the reversal rates being studied in Analysis 5 are reversals as a percent of verdicts *available* for review, including many that in fact were not reviewed. As we have noted, the capital-backlog measure serves in analyses such as these to control for the non-error-related, downward effect on reversal rates of delay and resulting pile-ups of unreviewed verdicts. Accounting for the effect of delay helps assure that other significant factors are related to capital error.⁴²⁶

ii. Year of death verdict. As in Analyses 1 and 2, time trend serves mainly in Analysis 5 as a control for the automatically downward effect on reversal rates of unfinished appeals in analyses in which reversal rates are calculated as a proportion of cases available for review, not as a proportion of cases actually reviewed. In such studies, reversal rates are automatically diminished by the number of verdicts that were not reviewed during the study period, and thus could not have been reversed during that period no matter how flawed they were. And this non-error-related effect of unfinished appeals increases as the year in which verdicts were imposed gets later, because the later a verdict was imposed, the less time it had to be reviewed at the second, state post-conviction phase by the study's 1995 end date.⁴²⁷ In Analysis 5, as in Analyses 1 and 2, therefore, the significant negative relationship between the passage of time and reversal rates that was found is at least partly—and may be entirely—a consequence of the non-error-related fact that many later-imposed verdicts simply could not be fully reviewed on state post-conviction during the study period and thus could not be reversed during that period, no matter how flawed they were. Impressionistic evidence of rising state post-conviction reversal rates over time provide another basis for interpreting this result as a measure of the inevitably rising number of unfinished appeals as death verdicts become more recent, and not as a reflection of changes in the quality of death verdicts over time after controlling for other factors.⁴²⁸

iii. Higher threat of homicides to the white community. Analysis 5 uses two different measures of the impact of homicides on the white community. The first compares states and years based on the number of whites killed in homicides per 100,000 whites in the population.⁴²⁹ The second measure—the same factor found significant in Analyses 1-4—compares that same rate to (*i.e.*, divides it by) the number of blacks killed in homicides per 100,000 blacks in the population. The former measure is the homicide victimization rate among whites only; the latter is a measure of the relative risk of homicide

felt by members of the white and black communities. In Analysis 5A, increases in the white homicide victimization rate by itself are associated with increases in the state post-conviction reversal rate, and the relationship is highly significant. In Analysis 5B, a higher homicide risk to the white community relative to the risk to the black community also is associated with higher state post-conviction reversal rates, although the relationship is not quite statistically significant.⁴³⁰ Effect size is large for both factors—indeed, considerably larger than for the analogous factors in Analyses 1-4:

- In the relevant states and years, white homicide victims per 100,000 whites in the population range from about 2 to 17, with an average of about 6. Analysis 5A predicts that states and years with 8 white homicide victims per 100,000 whites in the population will have over twice the state post-conviction reversal rate of states and years with 4 white homicide victims per 100,000 whites. The analysis predicts **reversal rates 17 times greater for white homicide rates at the high extreme among states in the study than for rates at the low extreme, when all other factors are held constant at their averages.**⁴³¹
- Analysis 5B predicts about a 3-fold increase in capital reversal rates when the homicide risk to whites relative to blacks rises from its lowest to highest levels among states in the study.⁴³²

These Analysis 5 findings provide additional support for the Analysis 1-4 findings that **capital error rates increase as the threat of homicide to the white community increases, which may be evidence that politically potent pressures to use the death penalty increase the likelihood that any given death verdict will be flawed.**

iv. More aggressive use of the death penalty. In both Analyses 5A and 5B, **the higher the number of death verdicts a state imposes per 1000 homicides, the higher its rate of serious capital error.** The relationship is highly significant in both analyses. Effect size again is large. The lowest number of death verdicts per 1000 homicides for any state and year in Analysis 5 is just under 2; the average is 27; the highest is 208. In Analysis 5A, **each time the number of death verdicts per**

1000 homicides doubles—e.g., from 5 to 10, from 10 to 20, and from 20 to 40—the expected reversal rate increases by a factor of 2.43 (i.e., it more than doubles). This means, for example, that as the death-sentencing rate rises from one-half the average among states and years in this analysis (about 13.3 death verdicts per 1000 homicides) to twice the average (about 53 verdicts per 1000 homicides), expected reversal rates increase six-fold.⁴³³ This provides additional evidence that **less judicious use of the death penalty is associated with higher rates of capital error.**

v. Greater population size and density. In state post-conviction Analysis 5, as in Analyses 1 and 2 of all three review stages and direct appeal Analyses 4 and 5, **death verdicts imposed in more urbanized (highly and densely populated) states are more likely to be overturned than death verdicts imposed in more sparsely populated states.** Effect size again is considerable—about twice as large as for the same factor in Analyses 2 and 4 (both of which are Poisson analyses, like Analysis 5). Compare Figures 30C, 30D and 38B, pp. 190, 208, above.

c. Summary: more evidence of the effect of heavy use of the death penalty.

Analysis 5 provides more evidence that aggressive use of the death penalty, including in reaction to a high risk of homicide to whites, is related to higher rates of serious capital error.

3. Federal habeas Analysis 6.

a. Tests of the analysis’s ability to generate reliable results.

Analysis 6 uses binomial regression analysis to identify factors related to reversal rates at the federal habeas stage of review. Those rates are the number of verdicts from each relevant state and year that were reversed at the federal habeas stage as a proportion of the number reviewed at the stage.⁴³⁴ Because federal habeas comes at the end of a long process of attrition of capital verdicts

through reversals and delay, only a relatively small number of states and years had at least one verdict that survived review without being reversed or bogged down at the prior two review stages and was finally decided at the federal habeas stage. Analysis 6 thus examines differences in only 161 reversal rates—less than one-third the number of observed reversal rates in Analyses 1 and 2, and less than half as many as in Analysis 5.

Despite the lower number of observations, Analysis 6 achieves results on a par with those of Analysis 5. The best analyses do not fit the reversal rate data any better or worse than the baseline analysis of only state, year and time trend.⁴³⁵ But those analyses: (1) eliminate the significant amount of unexplained and non-random state-to-state variance left by the baseline inquiry; (2) identify significant explanatory factors that are consistent across both best analyses and in important respects overlap the results of other analyses; and (3) identify explanations that are predicted to have large effects on capital reversal rates.⁴³⁶ The analysis thus increases somewhat our confidence in our findings about factors related to reversal rates at all three stages combined and provides some evidence of special factors operating at the federal habeas stage.

b. Significant explanatory factors.

i. Year of death verdict. Analysis 6 avoids a problem Analyses 1, 2 and 5 encounter in trying to measure the effect of time on error rates, because it examines only reversal rates among death verdicts that were actually reviewed at the federal habeas stage, not all verdicts available for review. Analysis 6 thus avoids the non-error-related downward effect on later reversal rates of unfinished appeals.⁴³⁷ But Analysis 6 still is not a good measure of the effect of time on rates of error (as opposed to rates of review), because a different non-error-related effect of the passage of time comes into play at the federal habeas stage. As is illustrated by Figure 10, p. 93 above, the time from imposition to

federal habeas *reversal* of death verdicts was usually from 1.5 to 2 years longer during the study period than the time from imposition to federal habeas *affirmance* of verdicts. As a result, the pool of verdicts imposed during the study period that were not finally reviewed on federal habeas as of the study's end date likely includes a disproportionately high number of flawed verdicts, and that disparity grows as verdicts get more recent.⁴³⁸ The disproportionately high number of flawed verdicts imposed later in the study period that were still awaiting final federal habeas review as of the study's cut-off date artificially depresses reversal rates for death verdicts imposed in later years—not because verdicts imposed later are less flawed, but because flawed verdicts take longer to review and because, in a time-limited study such as this, the main effect of that bias is to keep flawed verdicts imposed *later* in time from being counted. For these reasons, time trend serves in Analysis 6 as at least in part a control for the non-error-related effect of disproportionately delayed habeas reversals. And the negative relationship between federal habeas reversal rates and the passage of time in Analyses 6A and 6B⁴³⁹ is at least in part the product of the time lag for reversing flawed verdicts, and not an indication of declining rates of flawed verdicts, after controlling for other factors.⁴⁴⁰

ii. Higher proportions of people receiving, and expenditures on, welfare. None of the race factors is significant in Analysis 6, but a factor related to economic status is highly significant. **States with high ratings on an index measuring the proportion of a state's population receiving welfare and its per capita expenditures on welfare have higher federal habeas reversal rates than other states.** Effect size is large: **Predicted federal habeas reversal rates rise from 20% to 90% across the range of welfare burdens among states and years in our study.**⁴⁴¹ See Figure 40B (Analysis 6A), p. 220 below.

iii. Heavier political pressure on state judges. Federal judges are appointed for life—unlike

the state judges in our study, nearly all of whom serve for shorter terms and are subject to some kind of direct electoral influence.⁴⁴² Reviewing federal habeas judges thus do not face the same kinds of political pressures as the state judges who supervise the trials at which death verdicts are imposed and also decide state post-conviction petitions, or the state judges who review capital verdicts on direct appeal.⁴⁴³ If political pressures are related to higher capital error rates, as Analyses 1-4 find, one would expect that relationship to appear most clearly when federal judges review state verdicts. And it does.

In both Analysis 6A (using our first index of political pressure on state judges⁴⁴⁴) and Analysis 6B (second index), **the greater the pressure state judges are under to conform rulings to popular sentiment, the more likely it is that their death verdicts will be found to be seriously flawed on federal habeas review. The relationship is highly significant in both analyses. Effect size is substantial. When other factors are held at their averages, Analysis 6A predicts 4- to 6-fold increases in capital reversal rates at the federal habeas stage when political pressure on state judges is varied from the lowest to the highest levels among states in the study—with predicted error rates of about 55% at the high end of political pressure vs. 12% (Analysis 6A) and 9% (Analysis 6B) at the low end. See Figures 40C-1 and 40C-2, p. 221 below. These federal habeas effect sizes are larger than those at the direct appeal stage, and at all stages combined.**⁴⁴⁵ Compare Figures 40C-1 and 40C-2, p. 221 below, to Figures 29A, 29B and 37A, pp. 188, 207 above.

iv. Lower population size and density. As in all previous analyses, population size and density are important in Analysis 6. But the relationship is reversed: **Federal habeas judges are significantly more likely to find serious error in capital verdicts imposed in thinly populated states than in verdicts imposed in more heavily populated states.**⁴⁴⁶ Effect size is large. **Holding other factors constant, predicted reversal rates rise from less than 30% to about 65% across the range of**

population and density ratings among states in our study. See Figure 40D (Analysis 6A), p. 221 below. This result is consistent with lore among federal habeas lawyers that habeas reversals often occur in cases from relatively rural areas, especially in the South.

The difference in the effect of population structure on state versus federal court review is predictable based on the political factors just discussed. On average, less densely populated areas have fewer murders (in number, not per capita) than more highly and densely populated areas. As a result, any murder in a less populous community, and any death verdict imposed for it, is likely to be more publicly visible than murders and death verdicts in more populous areas. This in turn makes the *reversal* of any such verdict more controversial on average in less than in more populous areas. And that probably makes elected state judges subject to political pressures more reluctant to reverse death verdicts from less populous areas where reversals are more controversial on average than verdicts from cities with more murders and death verdicts.⁴⁴⁷ As a result, the pool of verdicts surviving state court review and becoming eligible for federal habeas review probably includes a disproportionately high number of flawed verdicts from less populous areas. And because life-tenured federal judges are less politically vulnerable than state judges, they may be less reluctant than state judges to reverse flawed verdicts from those areas. The fact that politically controversial backlogs of unreviewed capital cases have no significant influence on federal habeas reversal rates, but exert a downward influence on state direct appeal reversal rates, may be further evidence of the lower susceptibility of federal judges than state judges to locally generated political pressures to affirm death verdicts.⁴⁴⁸

c. Summary: more evidence of the influence of politics.

Analysis 6 provides more evidence that local political influences increase the probability of flawed death verdicts, while decreasing the probability that flaws will be corrected by state courts on state direct appeal and post-conviction review.

Effect Size: Year of Death Verdict (Influence of Delayed Reversals); Welfare Recipients and Expenditures (Analysis 6A)

Figure 40A (Analysis 6A): Probability of Reversal (as proportion of verdicts available for review; state post-conviction stage) Based on Year Death Sentence Was Imposed, Holding Other Factors Constant

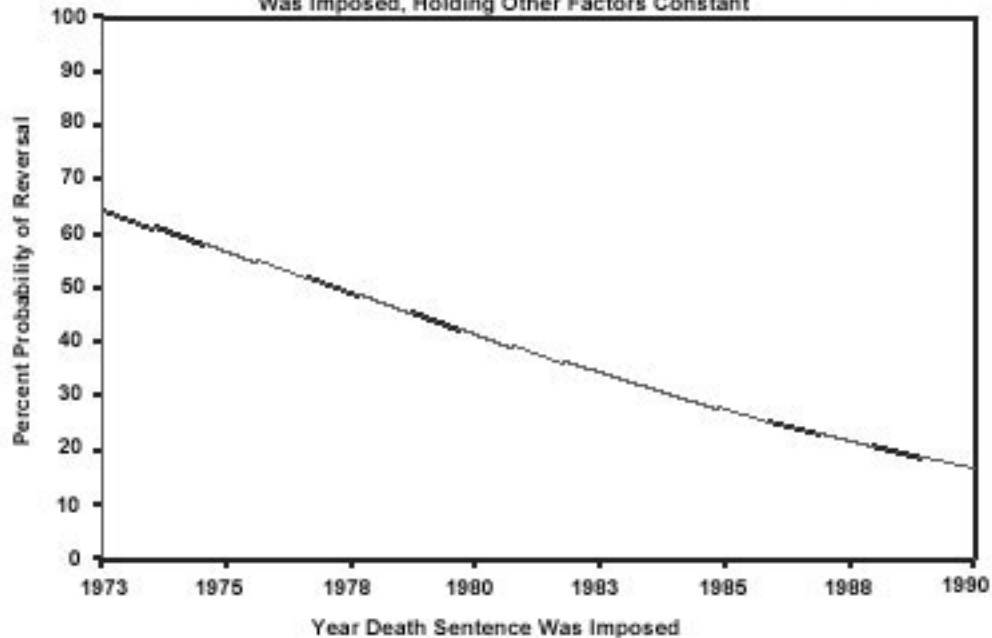
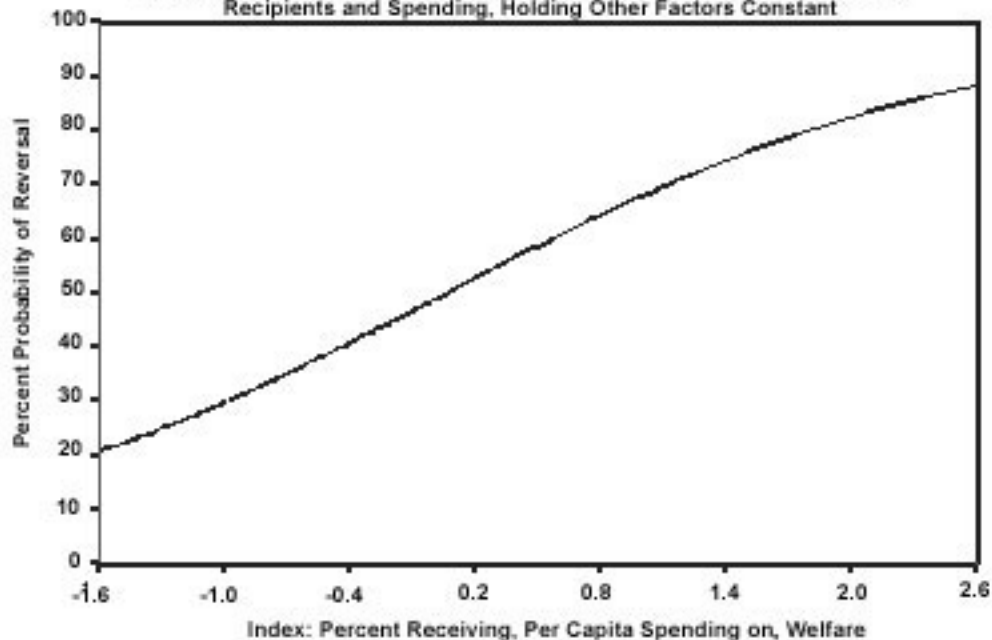


Figure 40B (Analysis 6A): Probability of Reversal (as proportion of verdicts available for review; state post-conviction stage) Based on State Welfare Recipients and Spending, Holding Other Factors Constant



Effect Size: Method of Selecting State Judges; Population Structure (Analyses 6A, 6B)

Figure 40C-1 (Analysis 6A): Probability of Reversal (as proportion of verdicts available for review; state post-conviction stage)

Based on Method of Selecting State Judges, Holding Other Factors Constant

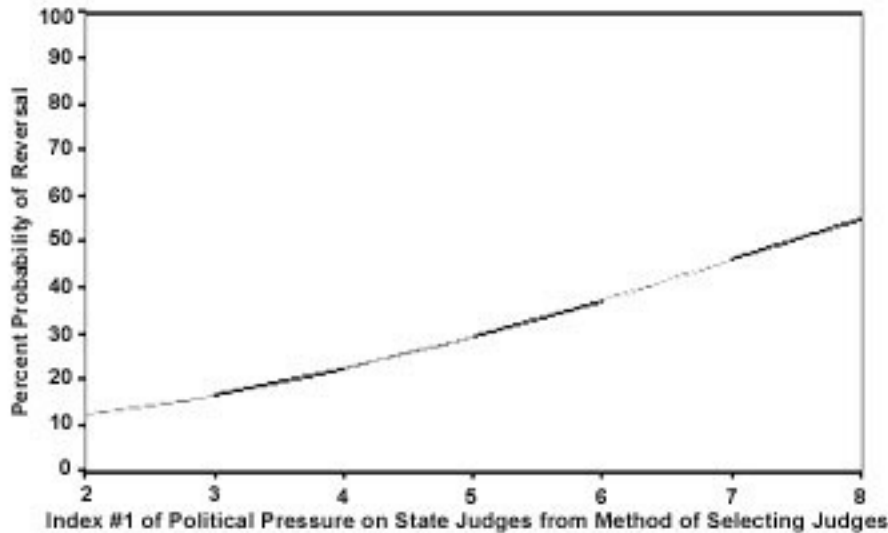


Figure 40C-2 (Analysis 6B): Probability of Reversal (as proportion of verdicts available for review; state post-conviction stage)

Based on Method of Selecting State Judges, Holding Other Factors Constant

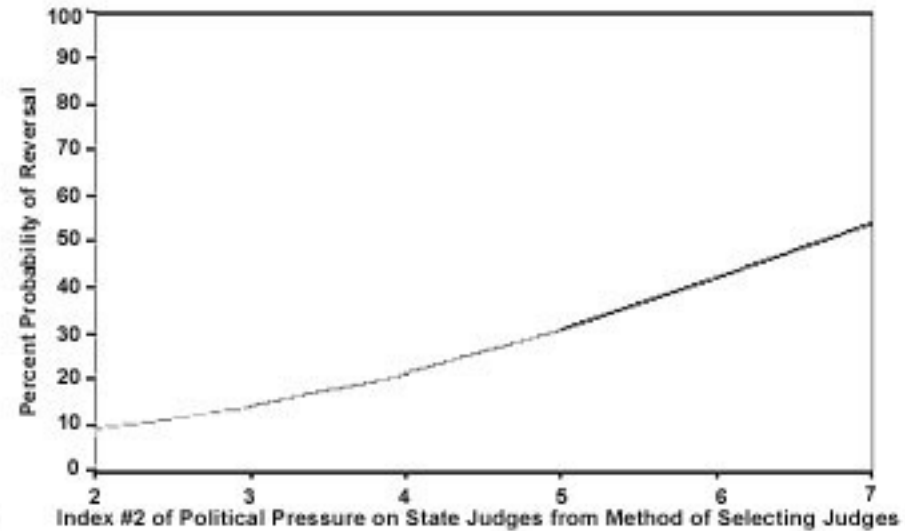
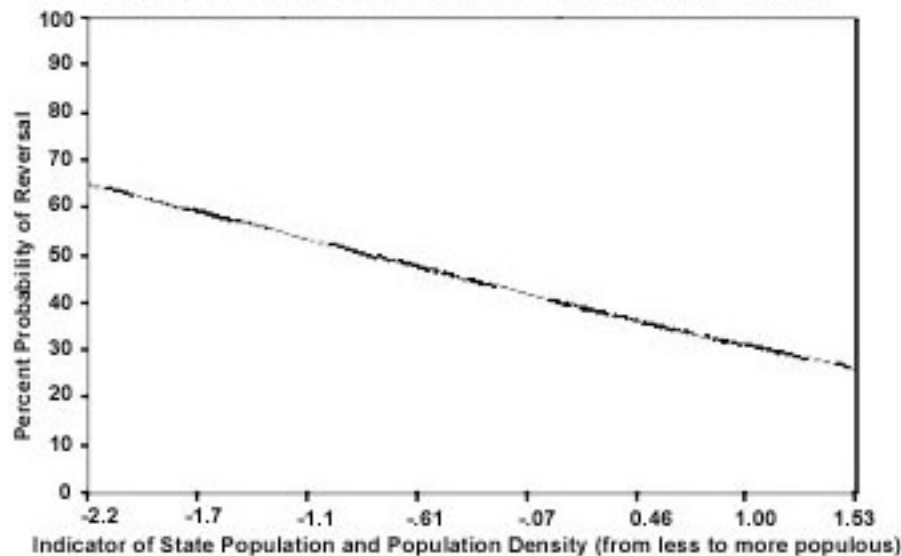


Figure 40D (Analysis 6A): Probability of Reversal (as proportion of verdicts available for review; state post-conviction stage)

Based on State Population Structure, Holding Other Factors Constant



D. Results of Analyses 14 and 15 of the States' 23-Year Capital Experiences: the Role of Race, Politics, Zeal for the Death Penalty and Weak Law Enforcement Confirmed

The statistical analyses above were all programmed to explain variations in capital reversal rates based on two standard statistical assumptions:

- reversal rates in the same state but from different years are likely to behave similarly; and
- reversal rates in the same year but from different states are likely to behave similarly.

This approach provides the most rigorous analysis of factors affecting reversal rates besides state and year. But because of the cross-cutting effect of state and year, this approach does not as directly answer a question policy makers in particular states may have: What factors account for differences between a given state's 23 years of experience with capital reversals and the 23-year experience of all other states? Analyses 14 and 15 provide a more direct comparison of the various states' 23-year reversal rate experiences by programming the analysis to assume that common forces affect all reversal rates for the same state regardless of year, without making the same assumption for all reversal rates for the same year regardless of state. Because the focus is on the various states' overall 23-year experiences with capital reversal rates, the effect of the passage of time is omitted.

Analysis 14 is a binomial regression analysis of all three review stages combined. Analysis 15 is a Poisson regression analysis of the same stages.⁴⁴⁹ In both analyses, the reversal rates being explained are reversals as a proportion of imposed death verdicts.

1. Tests of the analyses' ability to generate reliable results.

As is described above, all our diagnostic tests for Analyses 14 and 15, as for Analyses 1-4, indicate that the results are reliable:

- The baseline inquiries for both analyses leave substantial unexplained variance.

- The four best analyses—14A, 14B, 15A and 15B—all significantly reduce unexplained variance.
- All four analyses do a much better job than the baseline analysis of fitting the predicted results to the actual reversal rates being studied.
- All four analyses identify a set of statistically significant explanatory factors that are fairly consistent across the two analyses and four sets of factors within those analyses.
- With the addition of one possible factor, the significant explanatory factors overlap those identified by our main analysis.
- In all these diagnostic respects, Analysis 15A slightly outperforms the others, and is the main source of our effect-size graphs, Figures 41A-41I, pp. 229-32 below.⁴⁵⁰

2. Significant explanatory factors.

a. Non-error-related factors gauging low rates of review.

i. Capital backlogs. Analyses 14A, 14B, 15A and 15B all found that the states' number of death verdicts backlogged at the three review stages during the study period is significantly and negatively associated with average reversal rates.⁴⁵¹ As in Analyses 1, 2 and 5, this factor serves to control for the non-error-related effect on reversal rates of delayed appeals and resulting low rates of review, as opposed to low rates of error.⁴⁵² States with more verdicts awaiting review on average tend to have fewer death verdicts reversed, at least in part because they have fewer verdicts reviewed. See Figure 41A, p. 229 below, for this factor's effect size.

ii. Court backlogs generally. In all four analyses, a combined four-factor measure of the per capita number of court cases of all types awaiting decision in each state is also significantly and negatively related to reversal rates. As in Analyses 1 and 2, however, effect size is too small to warrant additional consideration of this factor.⁴⁵³ See Figure 41B, p. 229 below.

b. Error-related factors associated with higher capital reversal rates.

i. The death-sentencing rate per 1000 homicides. In all four Analysis 14 and 15 analyses, as in Analyses 1-5, **the more death verdicts a state imposes per 1000 homicides, the more likely it is that any one of those verdicts will be found to be seriously flawed.** The relationship is highly significant in all four analyses, and effect size is very high. **Holding other factors constant, Analysis 15A predicts that reversal rates increase 9-fold as death-sentencing rates rise from the lowest to the highest rates among states in our study.** See Figure 41C, p. 229 below.

ii. Larger black population. As do Analyses 1-4, Analyses 14 and 15 find that **death verdicts imposed in states in which African-Americans make up a comparatively higher proportion of the population were significantly more likely to be seriously flawed than verdicts imposed elsewhere.** This factor is significant to highly significant.⁴⁵⁴ Effect size is considerable. Analysis 15A predicts **close to a quadrupling of reversal rates when black population size rises from its lowest to highest levels among states in our study, holding other factors constant.** Significance⁴⁵⁵ and effect size drop some, however, in analyses that include homicide rates as a potentially explanatory factor. On effect size, compare Figures 41D-1 (Analysis 15A, which does not include homicide rates as a factor) and 41D-2 (Analysis 15B, which includes homicide rates as a factor), p. 230 below. We discuss the connection between the “percent black” and “homicide rate” factors in point *iii* below.

iii. Higher homicide rates. Although we considered homicide rates as a potential explanation for capital error in all our analyses, it usually was not significant. In Analysis 15B, however, but not in any other Analysis 14 and 15 inquires, homicide rates were significantly related to homicide rates.⁴⁵⁶ **The higher a state’s average homicide rate, the higher its average rate of serious capital error.** See Figure 41E-2,⁴⁵⁷ p. 230 below, for the factor’s moderate effect size in Analysis 15B. This

result may provide some additional evidence that **pressures to use the death penalty as a response to serious crime are related to higher error rates.**⁴⁵⁸

Homicide rates and the percent of the state's population that is black are correlated. States with higher black populations tend to have higher homicide rates.⁴⁵⁹ There is some evidence that these two factors compete in Analyses 14 and 15. Percent black is clearly the more important of the two. Unlike homicide rates, percent black is always significant at the .05 level and is sometimes highly significant, both in Analyses 14 and 15 and in most other analyses. But when both factors are included in the same Analysis 14 and 15 analyses, the significance level and effect size for the size of the black population diminishes.⁴⁶⁰ As is noted above, **Analysis 15A predicts nearly a quadrupling of error rates, other factors being equal, when the size of the black population rises from its lowest to highest levels among study states.** The corresponding rise in Analysis 15B, which adds homicide rates as a factor, is less steep. Compare Figure 41D-1 and 41D-2, p. 230 below. **The trade-off in effect between homicide rates and the size of the black populations—with the latter factor being the stronger of the two—supports a point made above: Racial stereotypes may lead influential members of the public to treat the size of a state's black population as a proxy for the threat of homicide, pressuring officials in states with larger black populations to pursue more death verdicts in weaker cases where the risk of error is high.**⁴⁶¹

iv. Relatively higher risk of homicide to whites as compared to blacks. As do Analyses 1-5, **Analyses 14 and 15 indicate that capital error rates are higher for verdicts imposed in states where the risk of homicide to members of the white community is more nearly the same as the risk to members of the black community than in states where the homicide risk to whites is lower than to blacks.** This factor is significant at the .01 level in Analysis 15A and at the .05 level

in Analysis 15B and falls just above the .05 significance level in Analysis 14.⁴⁶² Following the pattern noted above in regard to the size of the black population,⁴⁶³ the effect size of our measure of whether homicides are more or less heavily concentrated on whites relative to blacks is greater in Analysis 15A, which does not include general homicide rates as a factor, than in Analysis 15B, which does include that factor. Compare Figures 41F-1 and 41F-2, p. 231 below.⁴⁶⁴ There is evidence, then, that **(1) a higher risk of homicide—which probably increases pressure to extend the death penalty to weaker cases—contributes to higher rates of capital error, and (2) that the pressure generated by the risk of homicide is greatest when that risk to members of the white community approaches or surpasses the risk to members of the black community.**

v. Interaction of larger black population and higher risk of homicide to whites as compared to blacks. As do Analyses 1-4, Analyses 14 and 15 indicate that, independently of the effect of (1) the proportion of blacks in a state's population and (2) the rate of white, compared to black, homicide victimization, the *interaction of the two factors* is related to error rates: **Death verdicts are especially likely to be seriously flawed in states in which it is true both that African-Americans on average make up a relatively high proportion of the population and that the homicide risk to members of the white community approaches or surpasses the risk to members of the black community.** In those states, that is, reversal rates are even higher than the individual components of the interaction would predict. This interaction is significant in Analysis 15A, but not quite in the other analyses.⁴⁶⁵ **If the relative size of the black community and the relative threat of homicide to the white community each generates potent pressures to use the death penalty and a resulting higher risk of capital error, then the presence of both conditions may multiply the effect.**

vi. Lower rates of arrest, conviction and incarceration per serious crime. In Analyses 14 and

15, as in Analyses 1-4, **the lower a state's ratio of prison inmates to 100 FBI Index Crimes, the higher its rate of serious capital error**. The relationship is highly significant in all four Analysis 14 and 15 analyses, and effect size is large. Analysis 15A predicts that reversal rates increase by more than 8-fold as rates of apprehending and punishing serious criminals drop from their highest to lowest levels among states in these analyses. See Figure 41G, p. 232 below. Our tentative interpretation is that pressures to use the death penalty that are generated by the threat of crime, and the higher capital error rates that result, are greater in states with relatively less effective non-capital responses to crime.⁴⁶⁶

vii. More political pressure on state judges. Analyses 14 and 15 find what Analyses 1-4 and 6 also found: **Death verdicts are more likely to be seriously flawed if they are imposed in states where judicial selection methods increase judges' incentives to conform their rulings to popular sentiment**. The relationship is highly significant in all four analyses. Effect size in this analysis of all three review stages is moderate.⁴⁶⁷ See Figure 41H, p. 232 below. **If political pressures lead to high error rates, the effect should be greater in states where judges have a greater incentive to act consistently with popular opinion. That, indeed, is what Analyses 14 and 15 (and Analyses 1-4 and 6) find.**

c. Other factors.

i. Higher population size and density. As in Analyses 1-6, population structure is significantly related to error rates in all four Analysis 14 and 15 analyses. And as in Analyses 1-5 (but not Analysis 6), the relationship is positive: **The more heavily populated a state on average, the higher its average reversal rates**. See Figure 41I, p. 232 below, for the factor's considerable effect size.

ii. The interaction of high capital and general court backlogs. Analyses 14 and 15 reach the

same result as Analyses 1 and 2 in regard to the interaction of high capital and general court backlogs: Although each type of backlog by itself is *negatively* associated with reversal rates, when the effect of the two factors is multiplied, that interaction is *positively* related to reversal rates. **Even after accounting for their delay-related slowness to review (and thus their small numbers of reversed) death verdicts, jurisdictions whose courts are snowed under by both capital and non-capital backlogs tend to have particularly high rates of flawed death verdicts.**

Effect Size: Backlog of Capital Verdicts; State Court Caseloads; State Death Sentencing Rate (Analysis 15A)

Figure 41A (Analysis 15A): Effect on Reversal Rate
(as proportion of imposed verdicts; all 3 review stages) of Backlog of
Capital Verdicts Awaiting Review, Holding Other Factors Constant

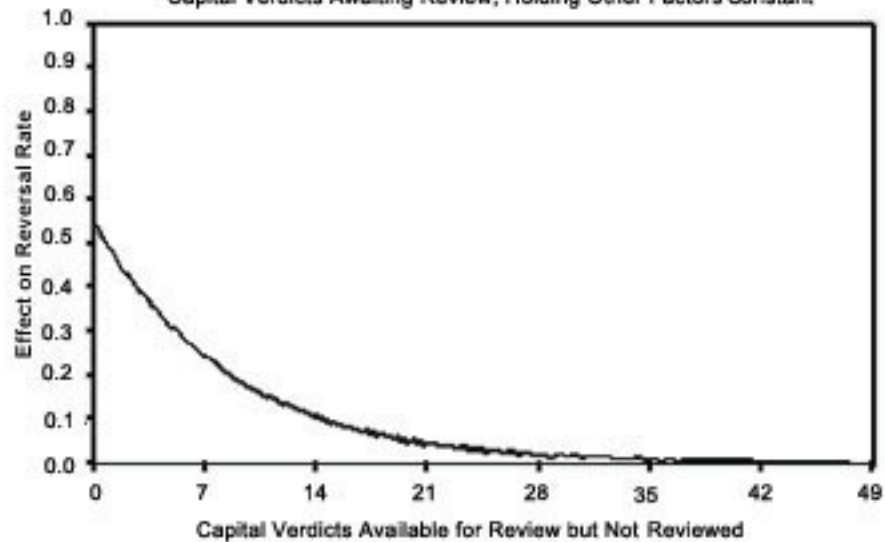


Figure 41B (Analysis 15A): Effect on Reversal Rate
(as proportion of imposed verdicts; all 3 review stages) of Four Factor Measure
of Net State Court Caseloads, Holding Other Factors Constant

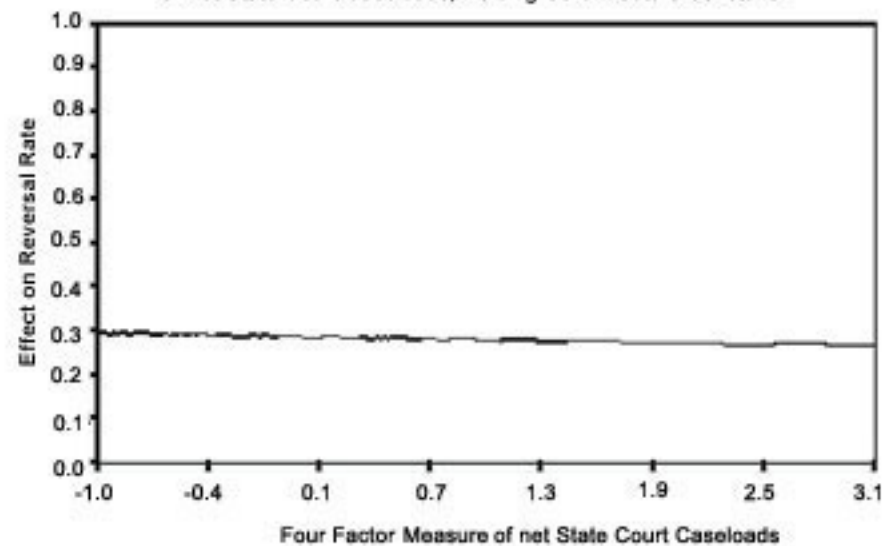
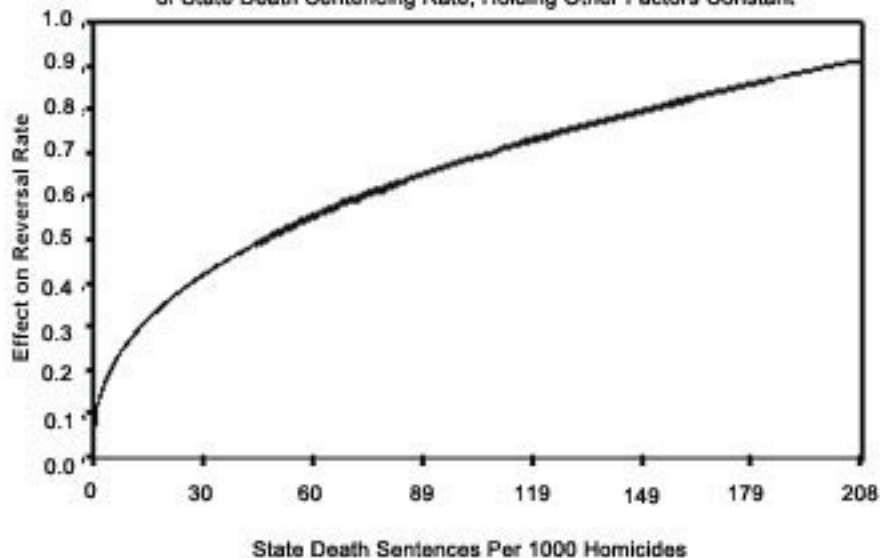


Figure 41C (Analysis 15A): Effect on Reversal Rate
(as proportion of imposed verdicts; all 3 review stages)
of State Death Sentencing Rate, Holding Other Factors Constant



Effect Size: Proportion of African-Americans in State Population; Homicide Rate (Analyses 15A, 15B)

Figure 41D-1 (Analysis 15A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of proportion of African-Americans State Population Holding Other Factors Constant

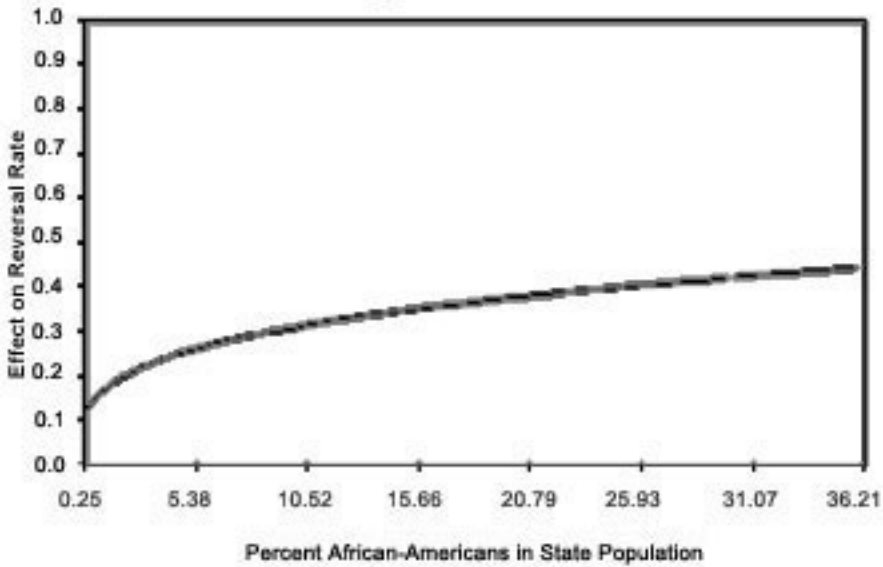


Figure 41D-2 (Analysis 15B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of proportion of African-Americans State Population Holding Other Factors Constant

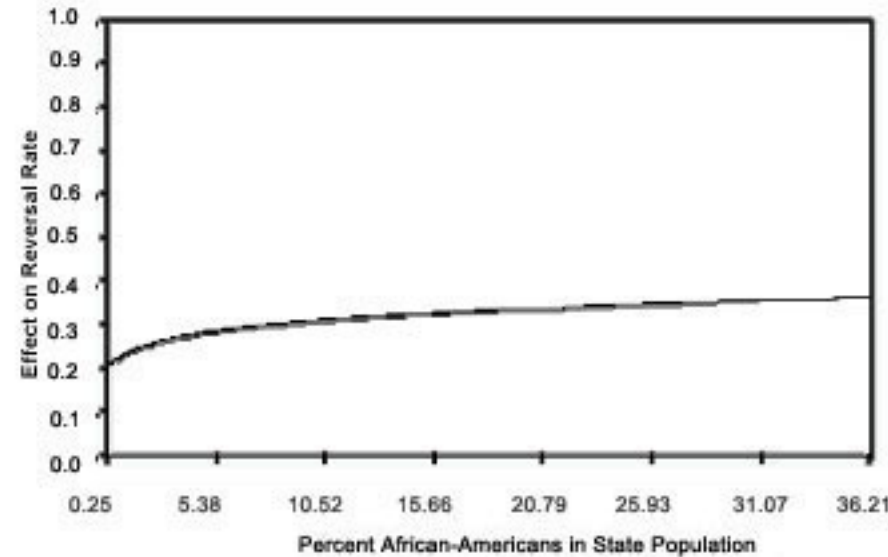
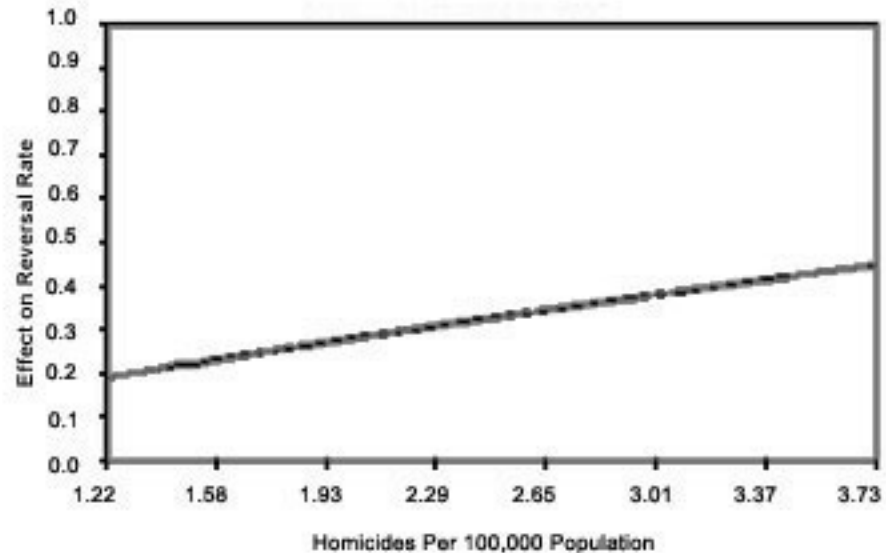


Figure 41E-2 (Analysis 15B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Homicide Rates, Holding Other Factors Constant



Effect Size: Risk of Homicide to Whites Versus Blacks (Analyses 15A, 15B)

Figure 41F-1 (Analysis 15A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review states) of Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant

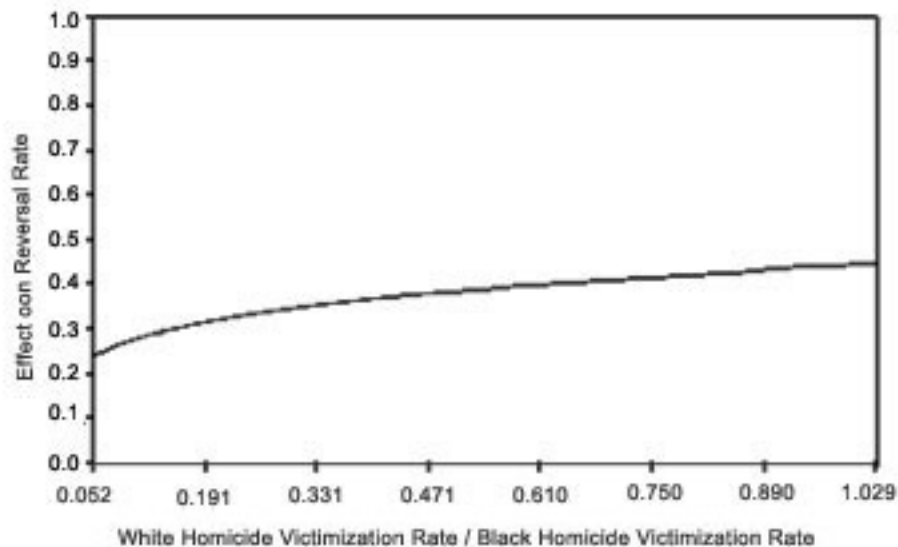
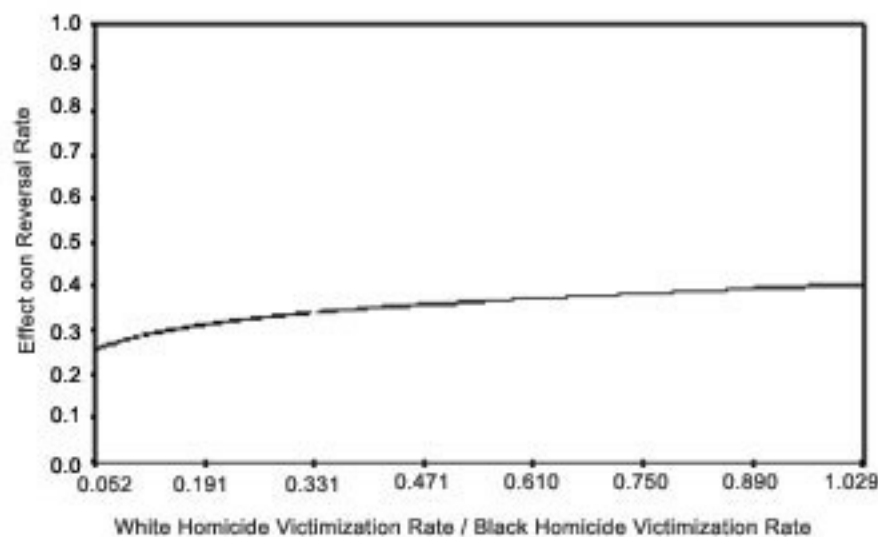


Figure 41F-2 (Analysis 15B): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review states) of Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant



Rate of Arrest, Conviction and Imprisonment Per Serious Crime; Selecting State Judges; Population Structure (Analyses 15A, 15B)

Figure 41G (Analysis 15A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

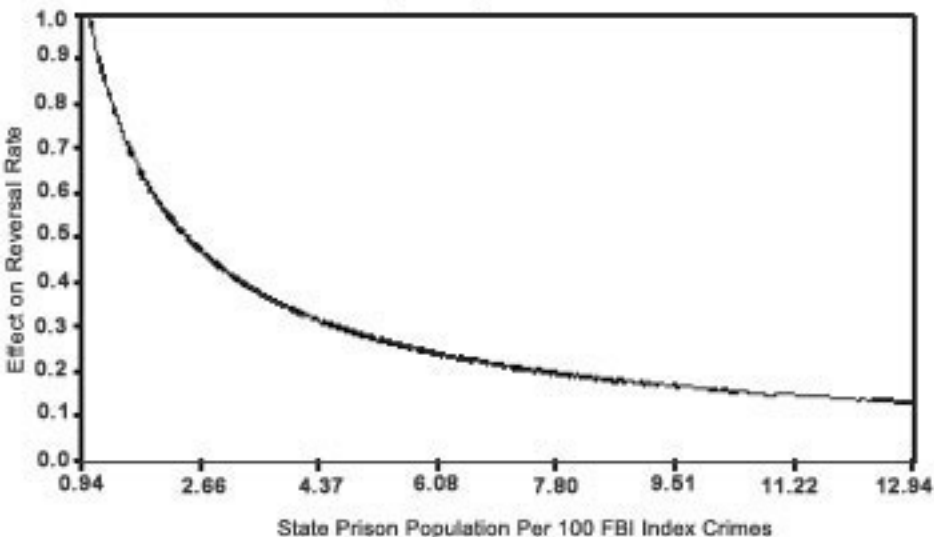


Figure 41H (Analysis 15A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of Method of Selecting State Judges, Holding Other Factors Constant

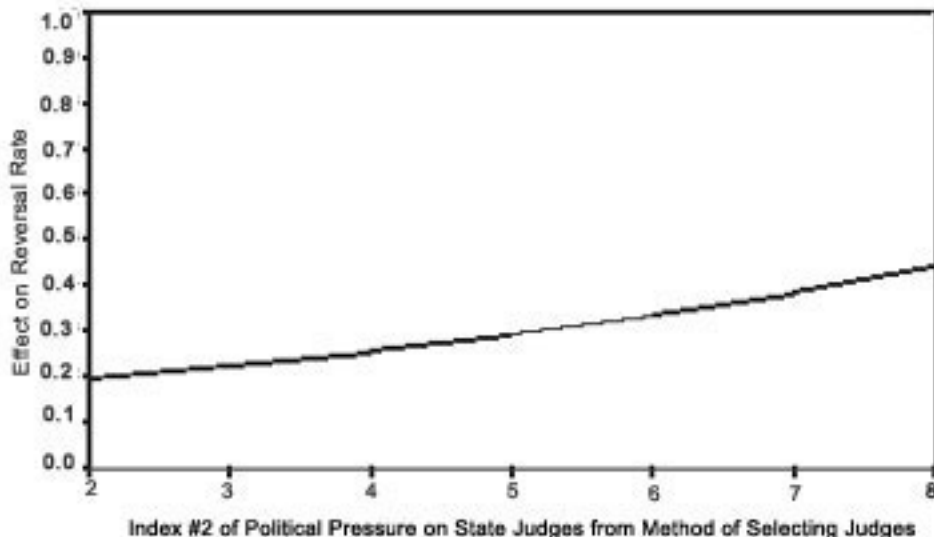
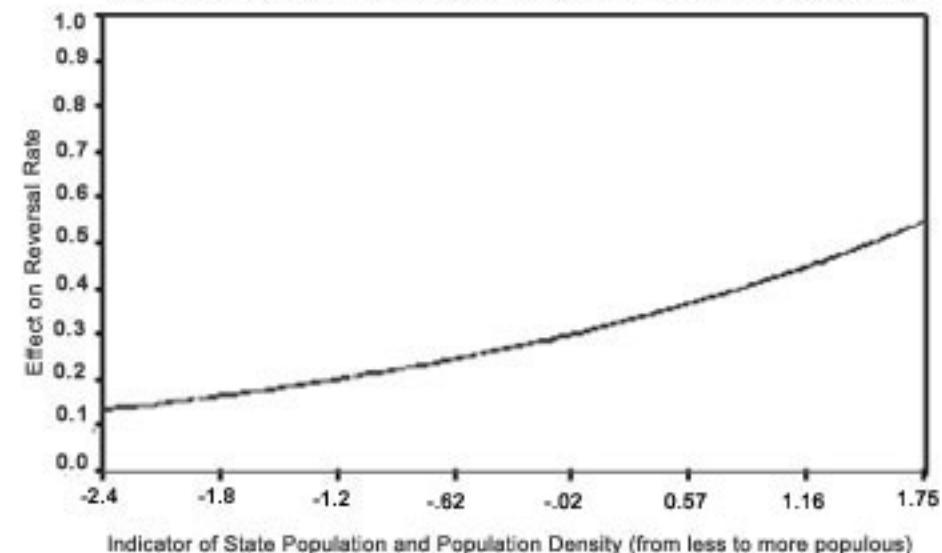


Figure 41I (Analysis 15A): Effect on Reversal Rate (as proportion of imposed verdicts; all 3 review stages) of State Population Structure, Holding Other Factors Constant



3. Summary: the relative unimportance of time.

Analysis 14 and 15's more focused comparison of the 34 capital states' 23-year experiences with capital reversals closely replicates the results of Analysis 1 and 2's combined comparison of each year's 34-state experience and each state's 23-year experience. The only difference is the significance in Analysis 14 and 15 of an explanatory factor not identified by Analyses 1 and 2—homicide rates.⁴⁶⁸ Even here, however, the difference is modest because the new factor is correlated with two factors Analyses 1 and 2 did identify—the proportion of the state's population that is black, and the extent to which the homicide risk to members of the white community approaches or surpasses the risk to blacks. Moreover, those two factors appear to be more targeted and more powerful measures of the same kinds of crime fears that may be generated by homicide rates and may account for their association with error rates.

Analysis 14 and 15's replication of the results of Analyses 1 and 2, despite omitting time as a factor, has important implications:

- Assessing the effect of time—the influence of particular years and of any trend over time—is not crucial to an understanding of the factors that contribute to differences in capital reversal rates.⁴⁶⁹
- The important differences to be explained in capital reversal rates are between states, not between years, or between earlier and later years.
- The individual factors our other six state analyses identify, and the *set* of factors main Analysis 1 and supporting Analysis 2 identify,⁴⁷⁰ have a stable association with capital reversal rates that appears whether or not the generalized effects of year and time trend are considered.

E. The Reliability and Robustness of the Results of the Eight State Analyses

1. The reliability of the eight analyses.

Below we consider the reliability of the eight state analyses based on the results of eight

diagnostic tests:⁴⁷¹

- Test 1: Does the baseline analysis of only state, year and time trend leave significant unexplained differences among states and years?⁴⁷²

Yes: Analyses 1, 2, 3, 4, 5,⁴⁷³ 6,⁴⁷⁴ 14, 15

- Test 2: Does the amount of unexplained variance decrease when other factors are considered?

Yes: Analyses 1, 2, 3, 4, 5, 6, 14, 15

- Test 3: Does the consideration of other factors significantly improve the fit of the predicted and actual results?

Yes: Analyses 1, 2, 3, 4, 14, 15

No: Analyses 5, 6

- Test 4: Are explanations for differences in reversal rates identified that are unlikely to appear by chance (*i.e.*, are statistically significant)?

Yes: Analyses 1, 2, 3, 4, 5, 6, 14, 15

- Test 5: Does the set of significant factors the best analyses identify fit the data better than other sets?

Yes: Analyses 1, 2, 3, 4, 5, 6, 14, 15

- Test 6: Are the significant factors the analysis identifies congruent with those identified by a parallel analysis using a different regression technique (binomial vs. Poisson)?

Yes: Analyses 1, 2, 3, 4, 14, 15

Not tested: Analyses 5, 6

- Test 7: Are the significant factors the analysis identifies congruent with those identified by parallel analyses examining different review stages (all three stages vs. state direct appeal vs. state post-conviction vs. federal habeas)?

Yes: Two-thirds or more of the significant factors in each of five analyses—1, 2, 5, 14 and 15—are also significant in analyses of at least two other review stages.

Yes: Half or more of the factors in each of Analyses 1, 2, 3, 4, 5, 6, 14 and 15 are also significant in studies of at least one other review stage.⁴⁷⁵

- Test 8: Are the significant factors each analysis identifies congruent with those identified by parallel analyses that compare each state's 23-year experiences with reversals?⁴⁷⁶

Yes: Analysis 1 (92%), Analysis 2 (92%), Analysis 3 (89%), Analysis 4 (89%), Analysis 5 (80% [or 100%]). In each of these analyses, the indicated percentage of the significant factors are reached significance in Analyses 14 and 15, which directly compare states' 23-year experiences with capital reversals.⁴⁷⁷

The results of these tests justify confidence in the reliability of the results of main Analysis 1 as well as supporting Analysis 2. These analyses are, respectively, a binomial logistic, and a Poisson regression analysis of factors related to state-to-state and year-to-year differences in reversals of death verdicts at all three review stages as a proportion of all death verdicts imposed during the study period. The diagnostic tests confirm our judgment that **main Analysis 1 and also supporting Analysis 2 are the most reliable methods of extracting the largest amount of information from our detailed data.** The tests reveal that **the results of Analyses 1 and 2 reflect actual relationships in the data, which are not sensitive to different regression techniques, different measures of reversal rates, the review stage being analyzed, or whether the focus is on state-to-state *and* year-to-year or only state-to-state differences in reversal rates.**

The tests also justify confidence in the results of Analyses 3, 4, 14 and 15. Analyses 3 and 4 are a binomial and a Poisson regression analysis of factors related to state-to-state and year-to-year differences in reversals of death verdicts at the state direct appeal stage as a proportion of all verdicts imposed during the study period that were actually reviewed at the direct appeal stage. The state direct appeal review stage analyzed by Analyses 3 and 4 is particularly important because it is the only stage that reviews essentially all capital verdicts and it accounts for nearly 80% of all reversals during the study period.⁴⁷⁸ Analyses 3 and 4 also are important because they provide the most reliable measure of the effect of the passage of time after controlling for other factors.⁴⁷⁹ Analyses 14 and 15 fill out

our knowledge still further by focusing more directly on state-to-state and less on year-to-year differences in reversal rates.

State post-conviction [Analysis 5](#) and federal habeas [Analysis 6](#) confirm results of the other six analyses by identifying explanations for reversal rates at the two later review stages that overlap the other analyses' explanations for reversal rates at the first review stage and the three review stages combined. [Analyses 5](#) and [6](#) justify somewhat less confidence in their stage-specific results. Although both analyses perform well on most diagnostic tests, neither improved on the fit achieved by their baseline analyses, and the explanations [Analysis 6](#) identified overlap less than usual with factors identified by other analyses. We do, however, have some confidence in two stage-specific results of [Analysis 6](#), given their confirmation by results from other analyses:

- A pattern of results from other analyses (1) reveals a positive relationship between capital error rates and judicial selection methods that pressure state judges to conform rulings to popular opinion, and (2) predicts that the relationship will be strongest at the federal habeas stage where judges are life-tenured and more isolated from local political pressures.⁴⁸⁰ The direction, significance and effect size of the relationship [Analysis 6](#) finds between error rates and political pressure is consistent with this prediction, giving us confidence in this [Analysis 6](#) result.
- For similar reasons, the *different* direction of the relationship between capital error rates and population size and density in [Analyses 1-5](#) (where error rates are positively associated with populousness) and [Analysis 6](#) (where error rates are negatively associated with populousness) tends to support the latter result. Because federal habeas is the last review stage and the only one staffed by life-tenured judges, it would be expected to bear more of the burden of reversing flawed verdicts from rural areas where such reversals tend to be especially controversial.⁴⁸¹ Here again, the conformity of an [Analysis 6](#) result to conditions predicted for the federal habeas stage by a pattern of results at the other stages gives us some confidence in this [Analysis 6](#) result.

2. The robustness of the significant explanations for state differences in capital reversal rates.

Table 7 below lists each of the explanations for reversal rates that was significant in one or more of the eight state analyses discussed above. To measure the robustness of each explanatory factor, Table 7 reports the number of analyses out of eight total in which the factor provided a significant explanation for state-to-state differences in capital reversal rates, and the review stage(s) at which the factor offers that explanation.

Table 7: State-Level Factors That Were Sometimes Significant, and How Often They Were Significant⁴⁸²

Which explanatory factor? [†]	Significant in how many of 8 analyses? [#]	Significant at what stages?
Death Verdicts/1000 Homicides	7	all 3, ** da, pc
Population Size and Density	7	all 3, ** da, pc
-Death Verdicts Awaiting Review, 3 Stages	6	all 3, ** da
-Same, State Post-Conviction Stage Only	1	pc
White/Black Homicide Victimization Rate or White Victimization Rate	6 ^{&}	all 3, ** da, pc
% of Population that is African-American	6	all 3, ** da
-Prison Population/100 FBI Index Crimes	6	all 3, ** da
Index 1, 2 of Political Pressure on Judges	5 ^{&}	all 3, ** hc
-4-Factor or 1-Factor Measure of All State Court Cases Per Capita Awaiting Decision [‡]	4	all 3**
Death Verdicts Awaiting Decision x 4-Factor or 1-Factor Measure of All State Court Cases Per Capita Awaiting Decision	4	all 3**
% of Population that is African-American x White/Black Homicide Victimization Rate	3 ^{&}	all 3**
-Per Capita Expenditures on State Courts	2	da
+Passage of Time (reliable) [^]	2 [#]	da
-Passage of Time (unreliable) [^]	4 [#]	all 3, pc, hc
% Pop., Per Capita Spending, on Welfare	1	hc
Homicide Rate	1	all 3**
-Population Size and Density	1	hc
3 Measures of Race of Defendant and Victim for Offenses Punished by Death [#]	1	all 3

- a. **Seven factors robustly related to high capital reversal rates: aggressive death sentencing; large black population, relatively high risk of homicide to whites, heavy political pressure on state judges, weak record of apprehending and imprisoning serious criminals, population size and density, and low capital backlogs.**

Given the range of methods the eight analyses use to explain state-to-state differences in capital reversal rates—different regression techniques, measures of reversal rates, review stages, groupings of reversal rates by state and/or year, and combinations of potential explanations—it is reasonable and conservative to express confidence in explanations for capital reversal rates that are significant in three-fourths or more of the analyses.

Seven explanations—set out below in order of robustness—satisfy this conservative approach. After controlling for other factors in the analyses:

- **States that impose more death verdicts per 1000 homicides have higher rates of serious error than states that use the penalty less often. The more frequently a state uses the penalty per homicide, the more likely it is that any one of its death verdicts is seriously flawed.** [7/8 analyses; 3/4 stages;⁴⁸³ 2/2 ways of grouping reversal rates by state and time.]
- **More densely populated states have higher reversal rates at the state court stages of review than do less populous states.** [7/8 analyses; 3/4 stages; 2/2 ways of grouping rates.] At the third, federal habeas stage of review, the effect is reversed. Federal habeas Analysis 6 finds that sparsely populated states have higher reversal rates than more populous states. [1/8 analyses; 1/4 stages; 1/2 ways of grouping states.] Overall, **capital error rates are sensitive to population size and density.** [8/8 analyses; 4/4 stages; 2/2 ways of grouping rates.]
- **States with more death verdicts awaiting review have lower reversal rates.** This is especially true in analyses where low review rates dictate low reversal rates calculated as proportions of imposed death verdicts. It is also true in analyses where reversal rates are proportions of only reviewed death verdicts, suggesting that backlogs create pressures to approve flawed verdicts. [7/8 analyses,⁴⁸⁴ 3/4 stages, 2/2 ways of grouping rates.]
- **States where the proportion of whites killed by homicide more nearly approaches the proportion of blacks killed by homicide have higher rates of serious capital error than states where the homicide burden is more heavily concentrated on blacks.** In one study it was the white victimization rate by itself that had this effect. [6/8 analyses;⁴⁸⁵ 3/4 stages; 2/2 ways of grouping reversal rates.]

- **States with larger African-Americans populations relative to their white population have higher capital error rates than states where blacks are a smaller part of the community.** [6/8 analyses, 2/4 stages, 2/2 ways of grouping rates.]
- **States that arrest, convict and imprison fewer criminals for every 100 serious crimes have higher rates of serious capital error than states that bring larger proportions of serious criminals to justice.** [6/8 analyses, 2/4 stages, 2/2 ways of grouping rates.]
- **States whose judicial selection methods give judges more of an incentive to conform their rulings to popular sentiment have higher capital error rates.** [5/8 analyses plus 2 other analyses just barely above significance;⁴⁸⁶ (2+1)/4 stages; 2/2 ways of grouping reversal rates.]

There are additional reasons for confidence in these seven key explanations for capital reversal rates. First, four additional factors were significant in some analyses and tend to confirm the seven explanations by identifying similar or related ones:

- In three analyses, the interaction of the two racial factors discussed above is independently related to error rates: **States where a high homicide risk to whites relative to blacks interacts with high numbers of blacks relative to whites have especially high capital error rates.** [3/8 analyses,⁴⁸⁷ 1/4 stages, 2/2 ways of grouping reversal rates.]
- In four analyses, the negative effect of capital verdicts awaiting appeal on review and reversal rates (the fourth key factor) is paralleled by the negative effect of per capita rates of court cases generally that are awaiting decision. This delay-focused effect is felt only in analyses where low review rates dictate low reversal rates calculated as proportions of all imposed death verdicts. This factor has a low effect size, however. [4/8 analyses, 1/4 stages, 2/2 ways of grouping rates.]
- In one analysis, **states with higher general homicide rates have higher capital error rates.** This provides some support for the finding that relatively high homicide rates among whites compared to blacks (the fifth of the seven key factors) are related to high capital error rates. A link between the effect of general homicide rates and the comparative threat of homicides to whites and blacks is indicated by the fact that in the two analyses in which general homicide rates were significant, the effect of the risk of white relative to black homicide victimization diminished somewhat. When general homicide rates were significant in the two analyses, the significant relationship between relatively large African-American populations and high reversal rates (the sixth key factor) also diminished somewhat—suggesting a link between crime fears and resulting capital error rates and the size of the black population.⁴⁸⁸ [1/8 analyses, 1/4 stages, 1/2 ways of grouping reversal rates.]

- In the federal habeas analysis, the size of the black population was not related to high error rates, but a combination of the size of the population receiving welfare and per capita expenditures on welfare *were* related to high error rates. **States with higher percentages of residents receiving welfare and higher per capita expenditures on welfare had higher capital reversal rates at the habeas stage.** This may be due to the correlation between high African-American populations and high rates of welfare assistance, or because large and visible populations of poor people increase crime fears among more well-to-do residents. Either effect is similar to, and tends to support, the influence of high proportions of African-American residents (the sixth of the seven factors). [1/8 analyses, 1/4 stages, 1/2 ways of grouping rates.]

Another reason for confidence in the seven key factors is their effect size. **Moderate (at least a doubling) to very large increases (4-, 6- and even 9-fold increases) in capital reversal rates are predicted when moving from one end of the spectrum among states in our study to the other in regard to each of the seven conditions, holding other conditions constant.**⁴⁸⁹

A third additional reason for confidence in the seven key factors is the existence of a convincing explanation for its failure to appear significant in a minority of the analyses. Consider, for example, the one analysis in which judicial selection methods, and resulting pressures on judges to conform to popular sentiment, are not significant. That is the only analysis that studies decisions by reviewing judges who are subject to essentially the same political pressures as judges making the errors in the first place and who thus are unlikely to spot and cure errors generated by that condition.⁴⁹⁰

As noted, there is also a good reason why high population size and density are associated with high reversal rates in the seven analyses of, or dominated by, *state* court reversals, while the same condition is associated with *low* reversal rates in the one analysis of federal court reversals. Reversals of death verdicts for homicides in rural areas are likely to be more controversial to entire communities on average than reversals of verdicts for homicides in urban areas. And the elected state judges who are mainly responsible for the two state court review phases have greater incentives to avoid such

controversy than the life-tenured judges responsible for final, federal review. The likely result is exactly the division of labor *all eight* analyses find: Elected state judges do a better job of screening urban death verdicts for error, leaving to less politically vulnerable federal judges the more controversial task of reversing flawed verdicts from rural areas and small towns.⁴⁹¹

In the rest of the cases, the minority of analyses in which one of the seven key explanations did not appear significant were ones in which there were fewer observations to be explained, making it more difficult for the analysis to identify important relationships that may nonetheless exist.

Additionally, all seven key factors were significant explanations for capital reversal rates in all six of our best state analyses (Analyses 1-4, 14, 15), judged by the number of reversal rates that were available to be compared and explained. Indeed, the seven factors together make up the broad core of the set of significant explanations for reversal rates in those analyses. And when considered together with the four allied factors listed above, the seven key factors formed virtually the *entire* set of significant factors in the best analyses. There thus is reason for confidence not only in the reliability of each of the seven explanations by itself, but also that the seven factors taken together provide a reasonably comprehensive understanding of the important explanations for state-to-state differences in capital reversal rates during the study period.

b. A single robust explanation with seven interlocking parts: excessive use of the death penalty as opposed to other responses to crime.

As the last point suggests, a final reason for confidence in the seven key explanations for reversal rates is that together they point towards **a single, persuasive explanation for capital reversal rates**. They do so, not only because they consistently appear in the best state analyses but, more crucially, because **they fit together logically, by virtue of what is known about the capital**

system, into an interlocking and potentially convincing rationale for differences in capital error rates across states and years. We sketch out parts of that overarching explanation above, and say more about it below after reporting the results of our county and case-level studies. It is useful here to articulate the explanation and note how it connects the seven factors in which our state analyses give us most confidence, as well as the four allied factors listed above:

When it comes to capital punishment, *less is better; more is worse*. The fewer death verdicts a state imposes per 1000 homicides, the less likely it is that any given verdict will be reversed due to serious error. And the fewer death verdicts a state imposes, the less overburdened its capital review system is, and the more likely it is to carry out the verdicts it does impose. Conversely, states that more often give in to pressures to use the death penalty and extend it to marginal cases have significantly higher rates of serious capital error, more delay in processing appeals, less success carrying out the verdicts they impose, and a greater temptation to approve flawed verdicts on appeal. Among the sources of pressure to overuse the death penalty in these ways are *politics, the ineffectiveness of the state's non-capital response to serious crime, the race and, possibly, the economic status of the state's residents and homicide victims, and on appeal pile-ups of capital cases awaiting review and flawed verdicts' imposition in non-urban communities*.

c. Two other potentially important factors: over-burdened courts and time.

Our analyses suggest two other explanations for high capital error rates. Our demanding tests warrant somewhat less confidence in the two explanations, but each has some support in the analyses, is consistent with the overarching explanation and is plausible:

- **Verdicts imposed by states with over-burdened and under-funded courts are more likely to be flawed than those imposed by states with average or better caseloads and funding:**
 - In analyses of all three review stages, the significant factor is the interaction of heavy capital and non-capital caseloads: **Courts with high capital and non-capital caseloads have high error rates.** [4/8 analyses, 1/4 stages, 2/2 ways of grouping rates.]
 - At the direct appeal stage, the important factor is per capita spending on courts: **Poorly funded state courts generate more capital error.** [2/8 analyses, 1/4 stages, 1/2 ways of grouping rates.]

These two results are different sides of the same coin: Having too many cases to decide likely means having too few resources to decide them reliably. It thus is reasonable to treat the two points as aspects of a single explanation: insufficient resources for capital trials. If the four analyses in which high capital and non-capital caseloads are significant are added to the two analyses in which poor funding is significant, the combined explanation crosses our demanding 75% line for explanations deserving a high degree of confidence. [6/8 analyses, 2/4 stages, 2/2 ways of grouping reversal rates.]

- Whether death verdicts were imposed earlier or later in the study period seems to affect the probability that they will be found to contain reversible error at different stages of review:
 - Controlling for other factors, **death verdicts imposed later in the study period are more likely to be found seriously flawed by judges at the direct appeal stage than verdicts imposed earlier in the study period.** [2/6 analyses,⁴⁹² 1/4 stages.] Direct appeal [Analyses 3](#) and [4](#) are the most reliable measure of changing amounts of reversible *error* found over time, giving us confidence in this result.⁴⁹³ The result is important because **direct appeal reversals are about 80% of all reversals between 1973 and 1995.**⁴⁹⁴ The result has three possible explanations: Controlling for other factors, later verdicts may be more seriously flawed than earlier ones; state direct appeal courts may have *detected* more serious error over time; or both. As we point out above, the fact that this finding occurs only *after* controlling for other factors—reversal rates in fact were quite stable during the latter half of the study period⁴⁹⁵—has an important implication. Evidently the specific factors we have been able to isolate that account for differences in error rates predict that those rates should have declined some over time. The fact that they did not decline indicates the influence of still other time-sensitive factors, captured in our analysis by time trend, that are associated with an *upward* trend in error rates. This means that reforms generated by known specific influences on error rates may not be as effective as one would hope because of the competing influence of conditions that are known to be increasing over time but otherwise are not well understood.⁴⁹⁶
 - **Among death verdicts reviewed on federal habeas** (*i.e.*, among verdicts surviving

inspection at the prior two, state stages of review), and after controlling for other factors, **verdicts imposed earlier in the study period were more likely to be reversed than verdicts imposed later in the period.** [1/6 analyses, 1/4 stages.] **Federal habeas reversals account for 10% of all reversals.**⁴⁹⁷ We are not confident in this result as a reflection of changing amounts of flawed verdicts over time because of a time-sensitive bias: Federal judges take more time on average to reverse flawed death verdicts than to approve unflawed ones. As a result, disproportionately more of the flawed verdicts imposed later in the study period were still under review at the end of the period and were not counted in our study than is true of other verdicts.⁴⁹⁸ The relationship between later verdicts and fewer federal habeas reversals thus could be entirely a function of longer delays in reviewing flawed verdicts, which led us to count a disproportionately small share of the flawed verdicts imposed later in the study period. Or, the result could be a function of that effect (which we know accounts for some of the result) plus the fact that state judges caught more error over time at the direct appeal stage, leaving less error to be caught at the later federal habeas stage. Support for this latter interpretation is found in evidence presented above that state courts increase their scrutiny for serious capital error to compensate for federal courts known to exercise low levels of scrutiny,⁴⁹⁹ and in the fact that a series of judicial and other cut-backs on the ability of all federal habeas courts to scrutinize capital verdicts for error—which began in 1986 and accelerated in 1989-93.⁵⁰⁰ Or, finally, this result could be a function in part of the fact that, controlling for other factors, there is less error in later death verdicts. This last possibility is hard to reconcile with the contrary finding at the direct appeal stage that, controlling for other factors, there is substantially more serious error in later verdicts.

- The remaining analyses demonstrate the obvious in regard to later as opposed to earlier verdicts: Court review takes time, so later verdicts are less likely to have been finally reviewed by the end of the study period than earlier verdicts, and thus were less likely to have been reversed or affirmed. Because the rest of our analyses calculate reversal rates as a proportion of imposed verdicts, not as a proportion of actually reviewed verdicts, the declines in reversal rates those analyses associate with later death verdicts were partly or entirely a result of the decreasing probability of review over time, not the decreasing probability of error. [3/6 analyses, 2/4 stages.]

* * * * *

Before reaching a final interpretation of the conditions that are significantly associated with capital reversals and reversal rates (see Part VII below), we consider how the above results of our eight state-level analyses are affected by results from eight analyses of county (as well as state) conditions and from an additional study of case level conditions (discussed in Parts V, VI below).

V. The Role of County-Level Conditions as Explanations for Serious Capital Error

A. The Potential Importance of County-Level Conditions

Thus far, we have studied the relationship to state capital reversal rates of conditions measured at the state level. There are two reasons to study conditions measured at the local level as well. First, the significant explanations for reversal rates that our state analyses identify may not actually be state-level conditions but instead an aggregate reflection of local conditions. Directly studying local conditions can help resolve this ambiguity. Second, there may be local influences on capital reversal rates—*e.g.*, from the array of capital decisions made by local prosecutors, trial judges and juries⁵⁰¹—that our state analyses miss entirely. For these reasons we conducted eight analyses of local capital reversal rates and of explanations for them that operate or can be measured at the local level.

As is discussed above, we analyze local conditions at the county level because the county is the most frequent site of local capital decision making, and the local jurisdiction about which relevant information is most often available.⁵⁰² Even so, county level analysis encounters three difficulties.

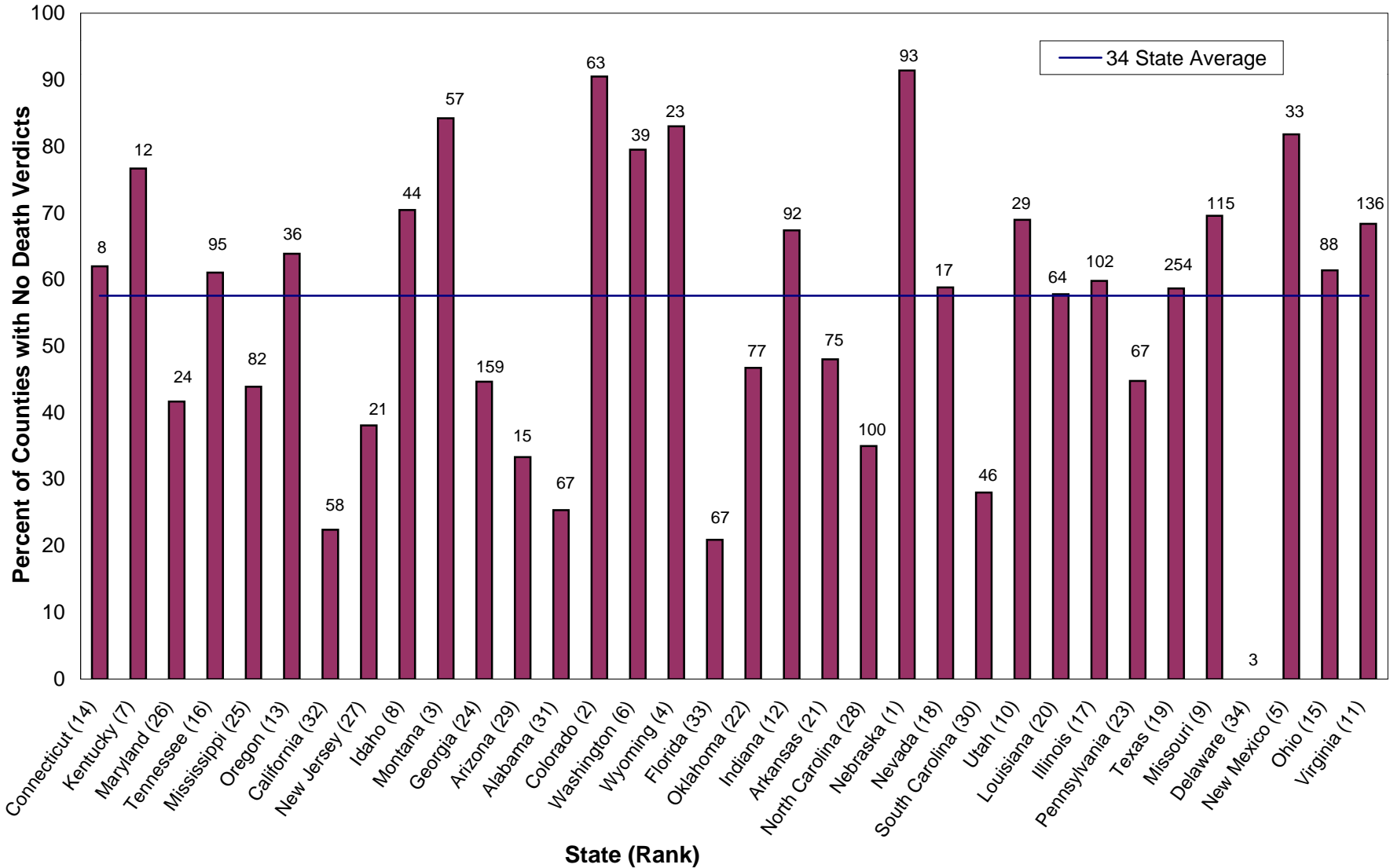
First, as Figures 42A and 42B, pp. 248-49 below, reveal, **a large proportion—58%—of the 2300-plus counties in the 34 states with active death penalties during the 23-year study period imposed *no* death verdicts in the period. Even in well-known capital states such as Texas and Virginia, most counties—respectively, 59% and 68% of the states' 394 counties—imposed no death verdicts in 23 years.** These proportions are influenced in part by how many counties states have. For example, the state with the smallest number of counties—Delaware (3 counties)—is the state with the largest proportion of capital counties (100%).⁵⁰³ But even states with similar numbers of counties have strikingly different proportions of capital counties. Like Delaware, Connecticut has only a small number of counties (8), but unlike Delaware's counties, a majority of Connecticut's

counties (5, or 62%) imposed no death verdicts in the study period. **Among the five states with 63-67 counties, the proportion of capital counties ranged from 79% in Florida and 75% in Alabama to 55% in Pennsylvania, 42% in Louisiana and 10% in Colorado.**

The map in Figure 42B roughly indicates the average size of counties in states, revealing that all states in the mid-Atlantic region (west of the Chesapeake Bay), the Midwest, and the South have large numbers of relatively small counties. But the counties in those regions used the death penalty at quite different rates: **The highest concentrations of death-sentencing counties are in states below the Mason-Dixon Line: the Carolinas, Georgia, Florida, Alabama, Mississippi, Louisiana, Arkansas and eastern Texas and Oklahoma.** Even here, however, there are differences: Counties in the Carolinas, Florida and Alabama use the death penalty much more uniformly than, *e.g.*, Georgia and Louisiana counties. **Next highest in terms of the concentration of capital counties are industrial states of the North and Midwest: Pennsylvania (especially around Philadelphia), Ohio, Indiana and Illinois (especially around Chicago).** Less predictably, perhaps, death-sentencing counties are noticeably *less* concentrated in the border states of Virginia, Kentucky, Tennessee and Missouri. Figure 42B provides less information about the West, but heavy use of the death penalty among counties in Arizona, California and to a lesser extent Nevada and Idaho can be contrasted to light use in, *e.g.*, Nebraska, New Mexico and Colorado.

For these reasons, our regressions have fewer counties to compare than one might expect—1002.⁵⁰⁴ The inability to study counties with no death verdicts also decreases variability because counties that use the death penalty are probably more like each other than counties that do not use it. If so, there is less diversity among the counties studied than one might expect.⁵⁰⁵ By contrast, when states as a whole are studied, the data being examined are from an aggregate of both sets of counties—non-capital as well as capital.

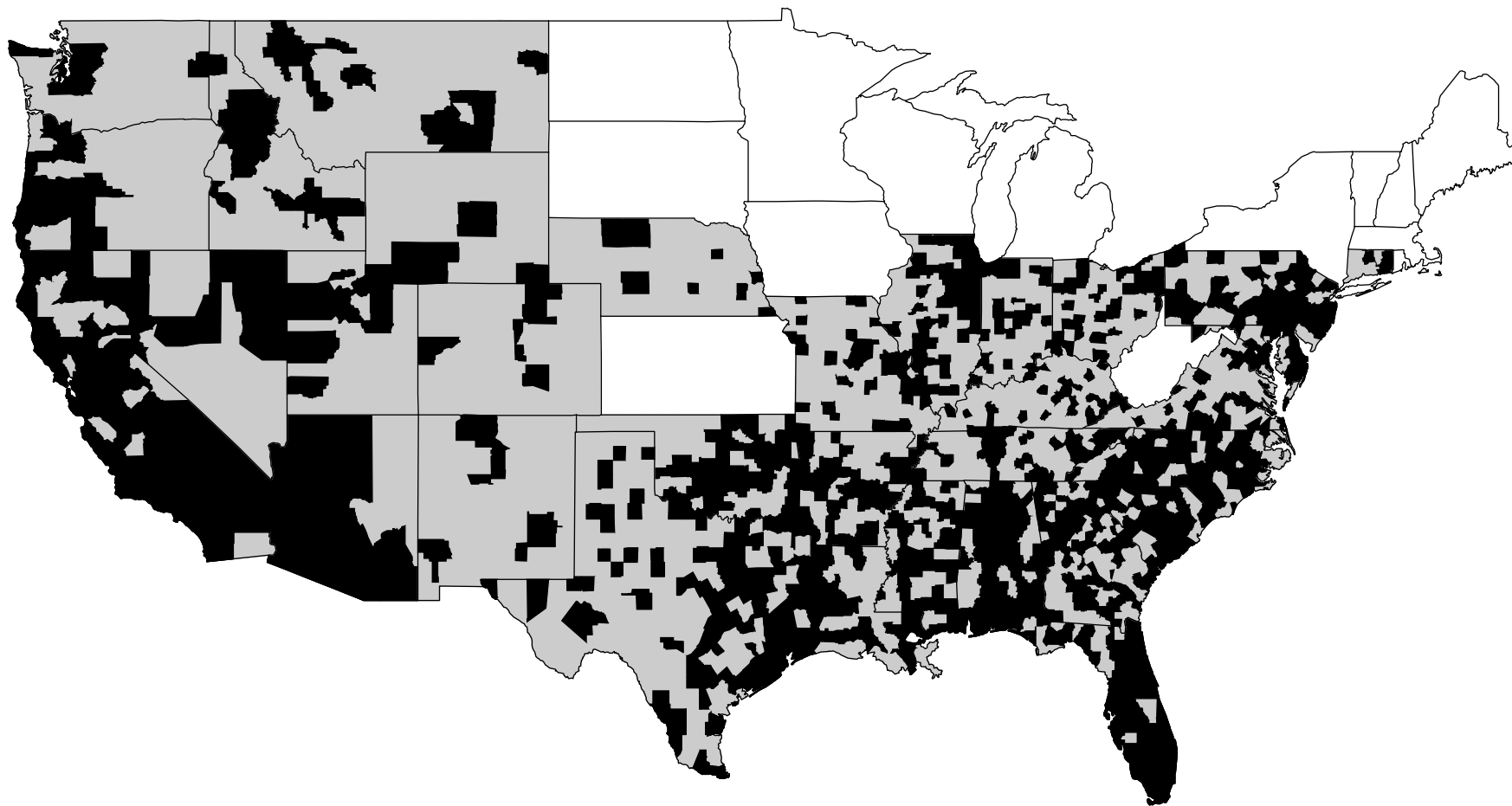
Figure 42A: Percent of Counties in 34 Active Capital States That Imposed No Death Verdicts, 1973-1995⁺



+ States are listed in descending order of overall reversal rates (see Figure 1B).

* Total number of counties in each state shown at top of each state's bar.

Figure 42B. Counties with at Least One Death Sentence in 1973-1995 (dark shading),
Counties in Death-Penalty States with no Death Sentences (light shading)



Second, most of our analyses compare each county's reversal experience in each of the 23 study years in which they imposed death verdicts. In those analyses the number of counties available for study drops to 967, because 35 counties are omitted where the year their death verdicts were imposed is unknown. More importantly, in any given year, only a small minority of the 967 capital counties imposed even one death verdict. As a result, there are not 22,241 (23 x 967) county-years to compare, but only 3054. In other words, each of the 967 death-sentencing counties imposed death sentences in an average of only 3 of the 23 study years.⁵⁰⁶ In 72% of those 3054 county-years, moreover, a total of only 1 death verdict was imposed, with an additional 16% of the county-years imposing only 2 death verdicts during the study period. Thus, 72% of the observed reversal rates being analyzed had only two possible outcomes (0% or 100% reversed), and 16% had only three possible outcomes (0%, 50% or 100% reversed). Given the small number of possible outcomes for a large proportion of the observed reversal rates, the amount of variation to be explained is diminished.

Finally, because of how hard it is for government agencies and scholars to gather data from over 3000 counties nationally, as opposed to only 50 states, not as much data are available on county-level, as opposed to state-level, conditions that might affect capital error.

B. Results of County-Only Analysis 7: Importance of County-Level Factors Similar to Those Identified by the State Analyses

We begin with Analysis 7, which is designed to make it as easy as possible to identify county-level factors that may be related to capital reversal rates. To do this, the analysis omits all states and state-level explanatory factors from the analysis. This makes the analysis an incomplete basis for drawing conclusions about county effects—because it invites county conditions to receive credit for each county's share of what, in fact, are state-level influences. But the approach is a useful starting

point, since it identifies the broadest possible set of potentially important county-level factors for consideration in later analyses.

Analysis 7 uses Poisson regression analysis to explain differences among capital reversal rates in the 967 counties in the 34 study states that imposed at least one death verdict during the 1973-1995 study period and in which the year of the verdict is known.⁵⁰⁷ To maximize the range of reversal rates available for analysis, Analysis 7 (like Analyses 1, 2, 14 and 15 above) calculates reversal rates as the proportion of all death verdicts *imposed* in particular counties and years that were reversed at any of the three review stages during the study period. This produced 3054 observed reversal rates for the 967 counties in each of the 23 or fewer years in which the county imposed death verdicts.

We also enhanced the amount of unexplained variance among counties in this initial analysis—again to help us identify the widest possible universe of potentially important county factors for examination in subsequent analyses—by treating only time trend, but not year or county, as a random effect in the baseline and in the more specific Analysis 7 inquiries. Under these circumstances, highly significant county-to-county and year-to-year variance among the 967 counties' reversal rates remained after Analysis 7's baseline inquiry of the effect of time trend.

The types of county-level factors our county analyses tested are outlined in Table 2, pp. 136-37 above.⁵⁰⁸ Those factors replicate at the county level most, but not all, significant state-level factors that our state analyses identify, as well as analogues of various state-level factors that were tested in our state regressions but were not found significant. Among the factors tested in the county analyses are county-level analogues of the following state-level factors:

- year death verdicts were imposed;
- number of death verdicts imposed per 1000 homicides;⁵⁰⁹

- African-American proportion of the population;
- homicide rate;
- homicide victimization rate among whites compared to among blacks, and white homicide victimization rate itself;
- population structure (population size and density); and
- the proportion of the county's population that is poor.⁵¹⁰

One important state-level factor—the statutory method of selecting judges—has no county-level analogue, because it measures a single state-level condition that is uniform throughout the state. In other cases, a county-level analogue for an important state-level factor could theoretically be calculated, but the factor in fact operates at the state level and none of the data needed to calculate a county analogue are centrally available. This is true for general court caseloads (which are collected only on a statewide basis by centrally organized state court systems with uniform policies across the state), and for imprisonment rates relative to rates of serious crimes (because states, not counties, operate prisons, and do not report the locality where prisoners committed their crimes and were convicted). The number of death verdicts awaiting review is also a state-level factor, because court delays are the responsibility of state courts. We can calculate a county analogue for this factor, however, using the county of origin of backlogged verdicts, and we did so in Analysis 7 only as a control for the effect of delay.⁵¹¹ Unlike Analysis 7, the other county analyses test state as well as county factors, so those analyses instead use the more appropriate, state-level backlog factor.

Analysis 7A is a single best set of factors, which identifies five significant explanations for county reversal rates (including time trend, which was also included in the baseline analysis).⁵¹² Analysis 7A had less unexplained variance among counties and years than did the baseline analysis,

although it did not fit the data better.⁵¹³ Confidence in Analysis 7A is enhanced somewhat because all five (highly) significant factors it identifies are county-level analogues of significant factors identified at the state level:

- **Later death verdicts were less likely than earlier ones to be reversed during the study period.** Given the use of reversal rates as proportions of imposed, not reviewed, death verdicts, this factor indicates that, after controlling for other factors, later death verdicts were less likely to have finished being *reviewed* than earlier ones, but provides no reliable evidence of declining rates of *error* over time.⁵¹⁴
- **Counties with more death verdicts awaiting review had smaller proportions of death verdicts reversed during the study period. At least in part, this result controls for the effect of delay, as opposed to error, on reversal rates calculated as proportions of imposed, not reviewed, verdicts.**⁵¹⁵
- **Counties imposing more death verdicts per 1000 homicides had higher capital error rates than counties imposing fewer death verdicts per 1000 homicides.**
- **Counties with higher homicide rates had higher capital error rates than counties with lower homicide rates.**
- **Counties with high population size and density had higher capital reversal rates than less populous counties.**

Effect size is appreciable for these factors:

- Controlling for all other variables, Analysis 7A predicts that reversal rates drop by a little less than 8% each year—not 8 percentage *points* but .08 times the previous year’s reversal rate.
- Each county death verdict awaiting review is expected to decrease a county’s reversal rate by 34% (in part, at least, because it decreases its review rate). Counties had as many as 14 death verdicts awaiting review.
- Analysis 7 predicts that **for every doubling of the county rate of death verdicts per 1000 homicides, the capital error rate will increase by about 20%**. The minimum rate of death verdicts per 1000 homicides is .08, the maximum is 600, and the average is 27.
- Every doubling of a county’s rate of homicides per 100,000 population is also associated with about a 20% rise in expected capital reversal rates. (Homicide rates per 100,000 population range from 0 to 332 and average about 9.5.)

- Every increase of 1 on our index of population size and density—the index goes from -2.1 to 3.8 among counties studied, averaging about 1.2—is associated with a 78% increase in predicted reversal rates.

Two important state-level factors with testable county-level analogues had no statistically significant effect at the county level: (1) homicide rates among whites compared to blacks, and (2) the African-American proportion of the population. Given the link [Analyses 14](#) and [15](#) revealed between these racial factors and general homicide rates,⁵¹⁶ the significance of homicide rates in [Analysis 7](#) may explain the failure of these racial factors to register at that level. If, as we have suggested, white, relative to black, homicide rates and the black proportion of the population are measures of the perceived threat of serious crime to people with influence over death-sentencing practices, it is not surprising that a grosser measure of those fears—homicide rates generally—registers at the county level, where the small numbers of death verdicts and reversals being compared may make it harder to pick up more subtle effects. As later county analyses become more complete and sophisticated (see below), the explanatory power of the two racial factors (operating at the state level) becomes pronounced, while the explanatory power of simple homicide rates decreases.⁵¹⁷

Overall, the important county-level factors [Analysis 7](#) provisionally identifies are similar to factors the state-level analyses identify. But given that [Analysis 7](#) entirely omits consideration of states and state factors, one would expect county-level analogues of factors important at the state level to register some of the effect of those conditions. The result thus calls for simultaneous consideration of county and state-level explanations in order to identify the level (state or county) or levels at which the important conditions operate or can best be measured.

An equally important result of [Analysis 7](#) is the absence of significant factors identified at the county level that have *no* analogue among the factors found important at the state level. This result

is particularly revealing, given that Analysis 7 was designed to maximize the effect of county-level factors.⁵¹⁸ The result tends to validate the state-level analyses as reliable and fairly complete indicators of important explanations for capital reversal rates.

C. Results of County-State Analyses 8, 9 and 10: Additional Evidence of Role of Factors Identified by State Analyses

One method of gauging the effect of both state and county factors is to include both in the same analysis, treating factors operating at one level of the hierarchy of jurisdictions (here, the lower level: counties) as random effects and factors operating at the higher level (here, states) as fixed effects. Analyses 8, 9 and 10 use this approach. Analysis 8 is a binomial regression analysis of *county* reversal rates calculated as proportions of imposed death verdicts that were reversed at any of the three review stages. Each of the 967 counties with at least one death verdict in a known year is treated as a subject variable and is nested in the state among the 34 studied in which the county is located. Nesting programs the analysis to assume that counties in the same state are more like each other than counties in other states. Reversal rates are calculated for death verdicts imposed in each of the 967 counties in each of the 23 years in which that county imposed at least one death verdict, producing 3054 observations. As in Analysis 7, variance is enhanced, as is the capacity of county-level variables to explain reversal rates, by omitting county and year as random effects. The baseline analysis measures only the effect of time trend, treated as a fixed effect.

Analysis 9 conducts the same analysis as Analysis 8, but using Poisson regression analysis. Analysis 10 conducts the same analysis as Analysis 8 (both are binomial analyses), but analyzes reversals as a proportion of death verdicts *reviewed* at the direct appeal stage. Because Analyses 8 and 9 calculate reversal rates as proportions of *imposed* verdicts, reversal rates are depressed over time

as a result of declining rates of review for verdicts imposed too late in the study period to have been finally reviewed at some or all three review stages as of the study end date. By contrast, Analysis 10 calculates reversal rates as proportions of verdicts *reviewed* at the single, direct appeal stage, assuring that changes in reversal rates over time are not affected by changing rates of *review* and, instead, are sensitive only to changing amounts of reversible *error* found on state direct appeal of verdicts imposed in later, as opposed to earlier, years.⁵¹⁹ Analysis 10 compares county reversal rates for death verdicts in each of 851 counties in each of the 23 years in which at least one fully reviewed death verdict was imposed, producing 2472 observations.

Analyses 8, 9 and 10 have similar results. In all three, a highly significant amount of unexplained variance remained after the baseline analysis of time trend. In each case, the best analyses revealed nearly identical sets of significant state- and county-level explanations for county differences in capital reversal rates. Those explanations all matched ones identified in state-only Analyses 1-6, 14 and 15 and county-only Analysis 7. And the three best analyses left no unexplained variance among county reversal rates beyond the differences expected as a result of random variation. Cautioning against too much reliance on these analyses, however, the reversal rates that they predict do not fit the actual data as well as those predicted by their baseline analyses.⁵²⁰

1. State-level explanations for high county capital error rates: race, politics, heavy use of the death penalty and ineffective law enforcement.

In all three analyses, seven state-level factors were significantly related to county reversal rates. In each case, and as a group, these state-level factors associated with *county* differences in capital error rates parallel the finding of Analyses 1-5, 14 and 15 that the same factors explain *state* differences in those rates. After controlling for the other factors in Analyses 8-10:

- **Counties in states with more death verdicts awaiting review had lower reversal rates.**⁵²¹ For Analyses 8 and 9, part of this effect may be due to the fact that both analyses treat reversal rates as a proportion of imposed, not reviewed, death verdicts, so that high rates of delayed appeals—meaning low rates of review—translate into low rates of reversals (and affirmances). Analysis 10, however, treats reversal rates as a proportion of death verdicts actually reviewed on direct appeal. In that analysis, high rates of delayed appeals are associated with low rates of reversible error found by state high court judges, suggesting pressure on appellate courts to affirm even flawed verdicts in order to speed-up review and decrease backlogs.⁵²² The fact that effect size is only modestly smaller for binomial Analysis 10 than for binomial Analysis 8 suggests that this latter interpretation partly applies to Analysis 8. See Figures 43A (Analysis 8A) and 43B (Analysis 10A), p. 260 below.
- **Counties in states with higher death-sentencing rates per 1000 homicides have higher capital error rates.** This relationship is highly significant in all three analyses, and effect size is large, especially for changes in death-sentencing rates around the average of 25 to 26 death verdicts per 1000 homicides. In that range, small increases in death-sentencing rates are associated with very large expected increases in error rates, holding other factors constant at their averages. See Figures 43C (Analysis 8A) and 43D (Analysis 10A), p. 260 below.
- **Counties in states with larger black communities relative to the total population have higher error rates.** The relationship is highly significant, and effect size is considerable—especially in Analysis 8 of all three review stages combined. See Figures 43E (Analysis 8A) and 43F (Analysis 10A), p. 261 below. Holding other factors constant, Analysis 8A (Figure 43E) predicts that counties in states where 5% of the population is black have reversal rates of just over 20%, rising to almost 55% for counties in states where blacks make up 35% of the population.
- **Counties in states where the homicide risk to whites approaches or surpasses that to blacks have higher capital error rates than counties in states where the homicide risk to whites is much lower than to blacks.** This relationship is highly significant in all three analyses. Effect size again is large, particularly in direct appeal Analysis 10A, with predicted direct appeal reversal rates rising from about 20% where the gap between low homicide victimization rates among whites and high victimization rates among blacks is the greatest, to nearly 70% where whites and blacks face the same risk of homicide, holding other factors constant. See Figure 43H, p. 261 below. See also Figure 43G (Analysis 8A, where the corresponding rise is from just over a 20% to just less than a 60% error rate), p. 262 below.
- **Counties in states with large black populations relative to their white population and with high rates of white, relative to black, homicide victimization have especially high error rates.**⁵²³
- **Counties in states where judicial selection methods give judges more incentive to adapt their rulings to public opinion have higher capital error rates.** Analyses 8A and 9A test

the second political pressure index; Analysis 10A tests the first index.⁵²⁴ The relationship is highly significant in Analyses 8A and 10A and nearly so in Analysis 9A.⁵²⁵ Effect size is moderate. See Figures 43I (Analysis 8A) and 43J (Analysis 10A), p. 262 below.

- **Counties in states with lower rates of arrest, conviction and imprisonment per serious crime have higher capital error rates.** This highly significant factor has steep predicted effects, holding other factors constant. See Figures 43K (Analysis 8A) and 43L (Analysis 10A), p.262 below. Analysis 10A (Figure 43L) predicts direct appeal reversal rates of only 12% where the number of inmates per FBI Index is at its highest level among states in our study, rising to 38% for counties where the apprehension and punishment rate is average for those states and to 83% where the apprehension and punishment rate is at its lowest level among study states.

An eighth factor familiar from the state analyses was significant in Analyses 9 and 10:

- **Counties in more densely populated states had higher capital reversal rates.** Effect size is considerable, with predicted reversal rates almost tripling across the spectrum of population sizes and densities for states in the study. See Figure 43M (Analysis 10A), p.263 below. This factor was not significant in Analysis 8, though the analogous county-level factor *was* significant.⁵²⁶

The best Analysis 8 group of factors was different from the best Analysis 9 and 10 group in two other respects: In Analysis 8, but not the others, counties in states with higher per capita caseloads for all types of court cases, and lower homicide rates, had higher capital reversal rates. Effect size is small for both factors. See Figures 43N and 43(O), p. 263 below.⁵²⁷

2. The role of time.

In Analyses 8, 9 and 10, we included a linear trend for time but excluded the random effects for individual years. For reasons noted above, Analyses 8 and 9 of all three review stages combined had to calculate reversal rates as proportions of imposed, not reviewed, death verdicts and thus cannot reliably assess whether verdicts imposed later in the study period are more or less prone to reversible error than earlier verdicts. Given the time final review takes, later verdicts (no matter how error-prone) are less likely than earlier ones to have been finally reviewed, and thus to have been reversed (or

affirmed), by the study end date. In other words, in studies of reversal rates as proportions of imposed verdicts, time trend controls for the effect of *unfinished appeals*, rather than gauging changes in *error rates* over time.⁵²⁸ In contrast, direct-appeal-only Analysis 10 calculates reversal rates as a proportion of only *reviewed* verdicts. This avoids the confounding effect of delay and enables the analysis to measure the effect of the passage of time on the probability, not of review, but of reversal due to serious error. Under these circumstances, Analyses 8-10 provide substantial evidence that, after controlling for other factors, **county-level rates of reversible error have increased over time:**

- Despite the strong non-error-related propensity of three-stage analyses like Analysis 8 and 9 to depress reversal rates, due to declining review rates, over time, the relationship between the passage of time and lower reversal rates is not statistically significant in Analyses 8A or 9A, and effect size is minimal.⁵²⁹ See Figure 43P, p. 264 below. Apparently, some force is counteracting the strong tendency of analyses like this to depress review, and thus reversal, rates for later verdicts.
- Analysis 10 suggests what that force may be. Because Analysis 10 has no delay-driven tendency to drive reversal rates down in later years, it reliably measures changes over time in the amount of reversible *error*. After controlling for other factors, Analysis 10A shows that **county rates of serious capital error detected on direct appeal are significantly higher for death verdicts imposed later in the study period than for earlier verdicts.** The relationship is highly significant, and effect size is considerable: Counties' expected direct appeal reversal rate for death verdicts imposed at the beginning of the study period is about 20%. **For verdicts imposed at the end of the period, the expected reversal rate is more than triple that (65%).** See Figure 43Q, p. 264 below.

Effect Size: Backlog of Capital Verdicts; State Death Sentencing Rate (County - State Analyses 8A, 10A)

Figure 43A (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

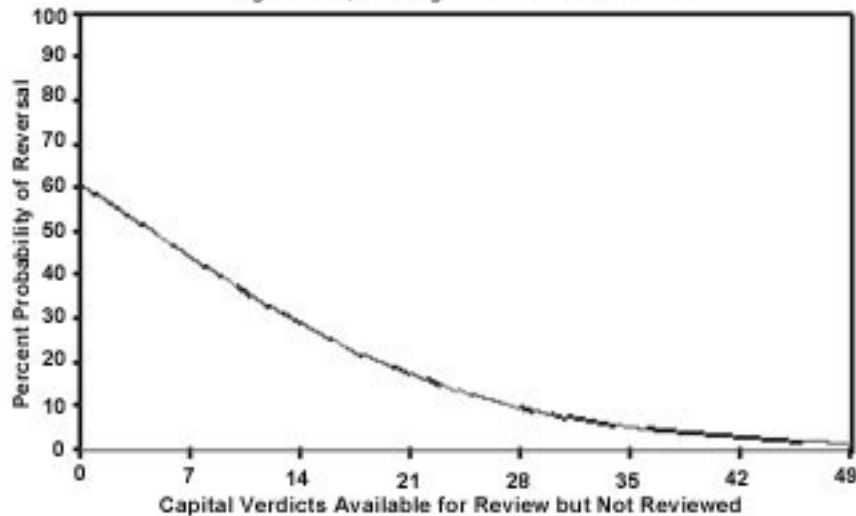


Figure 43B (Analysis 10A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

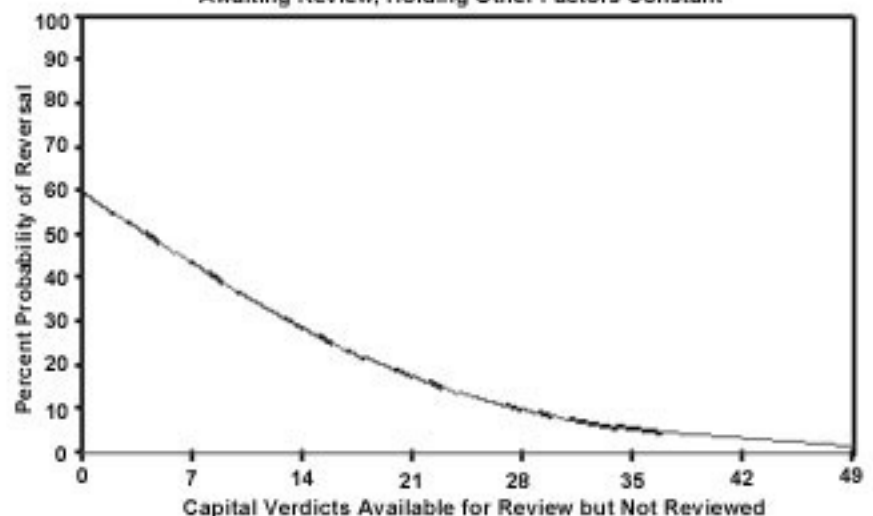


Figure 43C (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Death Sentencing Rate, Holding Other Factors Constant

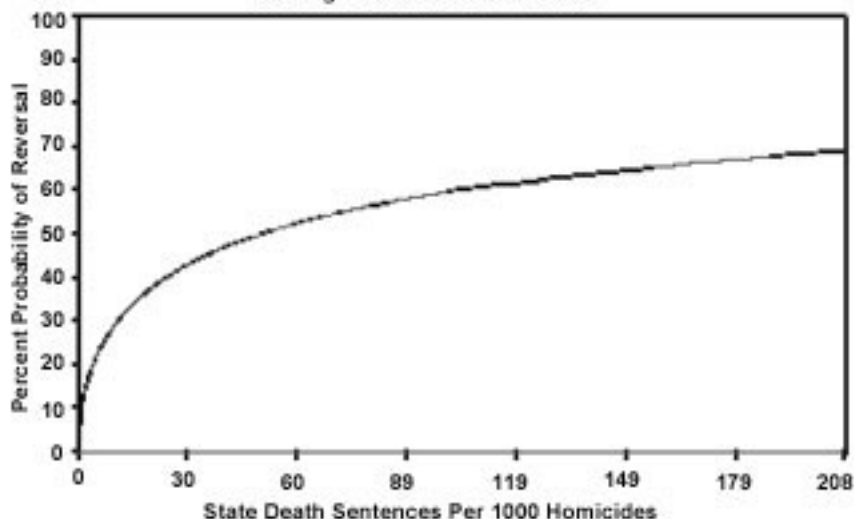
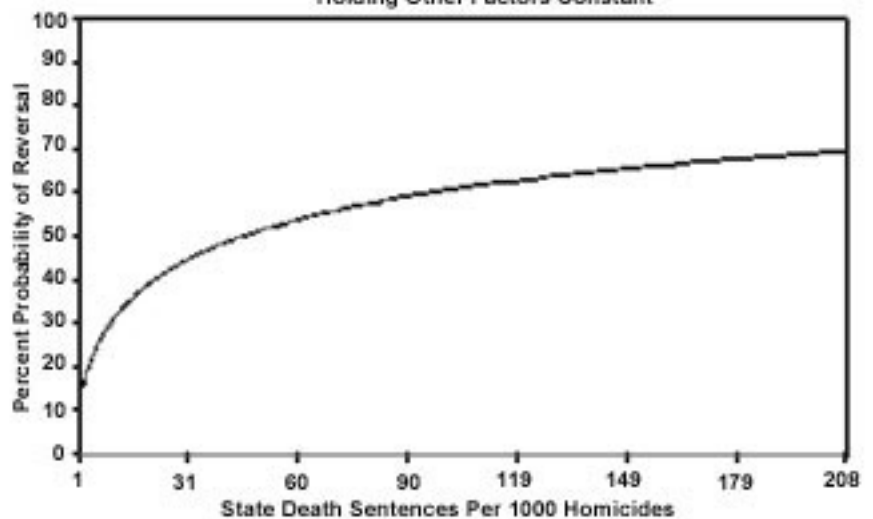


Figure 43D (Analysis 10A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on State Death Sentencing Rate, Holding Other Factors Constant



Effect Size: Proportion of African-Americans in State Population; Risk of Homicide to Whites Versus Blacks (County - State Analyses 8A, 10A)

Figure 43E (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Proportion of African-Americans in State Population, Holding Other Factors Constant

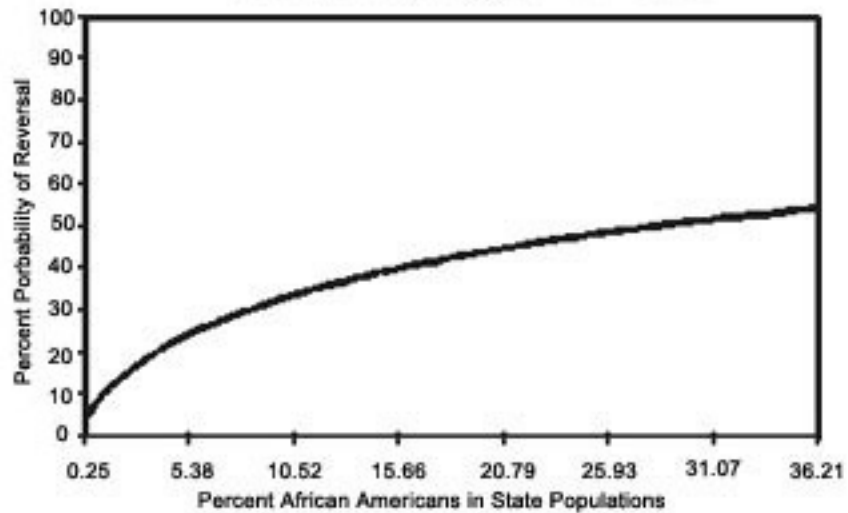


Figure 43F (Analysis 10A): Probability of Reversal (as proportion of imposed verdicts; direct appeal stages) Based on Proportion of African-Americans in State Population, Holding Other Factors Constant

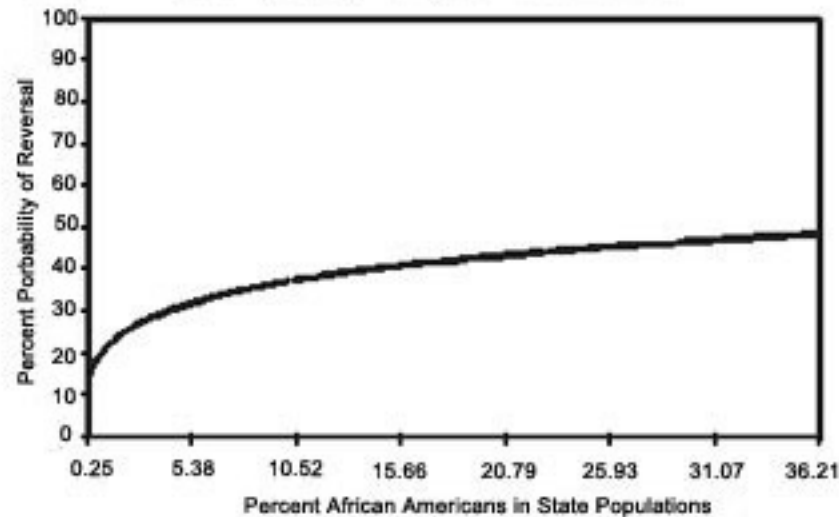


Figure 43G (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant

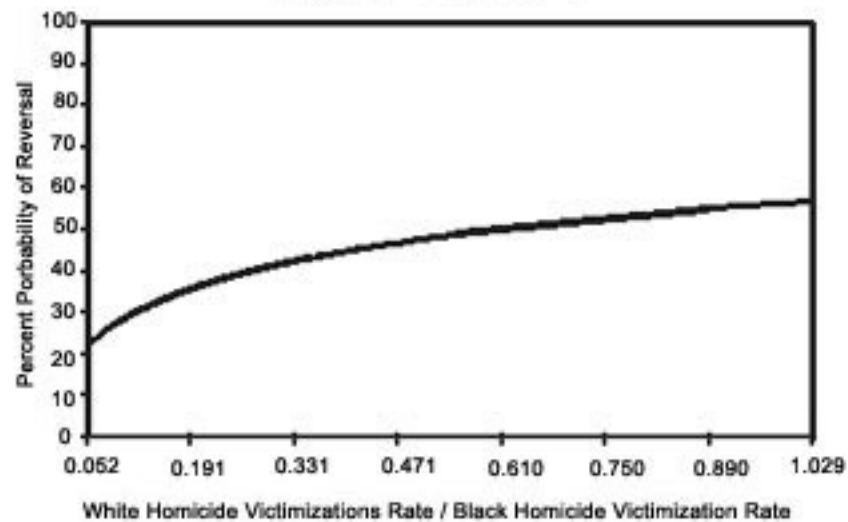
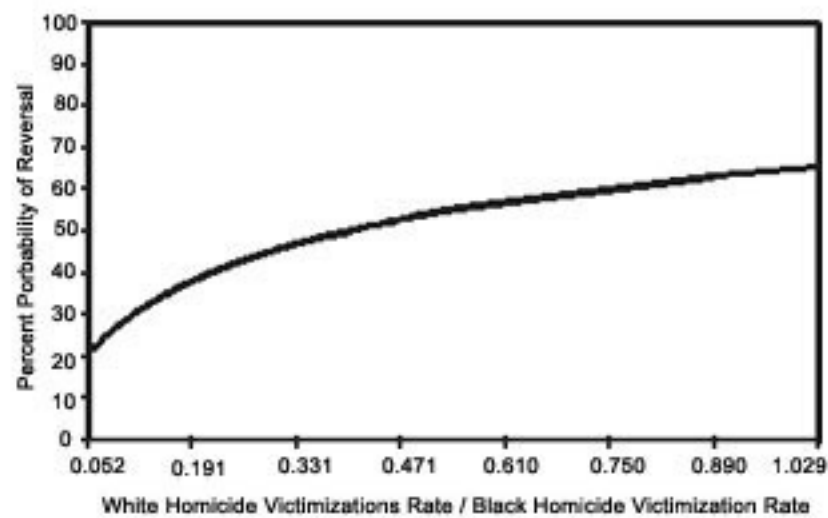


Figure 43H (Analysis 10A): Probability of Reversal (as proportion of imposed verdicts; direct appeal review stages) Based on Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant



Effect Size: Method of Selecting State Judges; Rate of Arrest, Conviction and Imprisonment Per Serious Crime (County -State Analyses 8A, 10A)

Figure 43I (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Method of Selecting State Judges, Holding Other Factors Constant

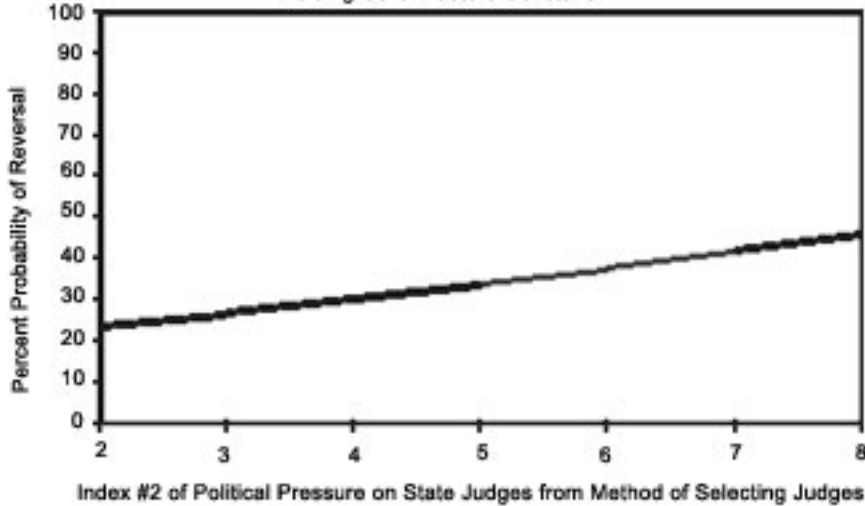


Figure 43J (Analysis 10A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Method of Selecting State Judges, Holding Other Factors Constant

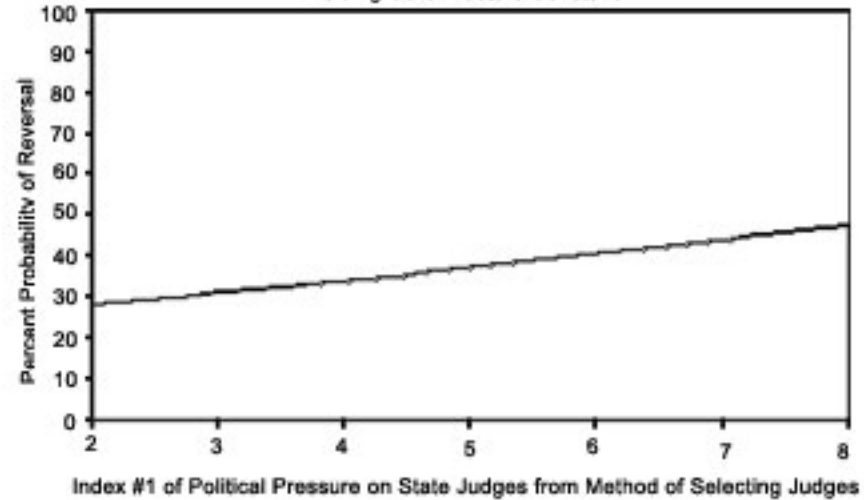


Figure 43K (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

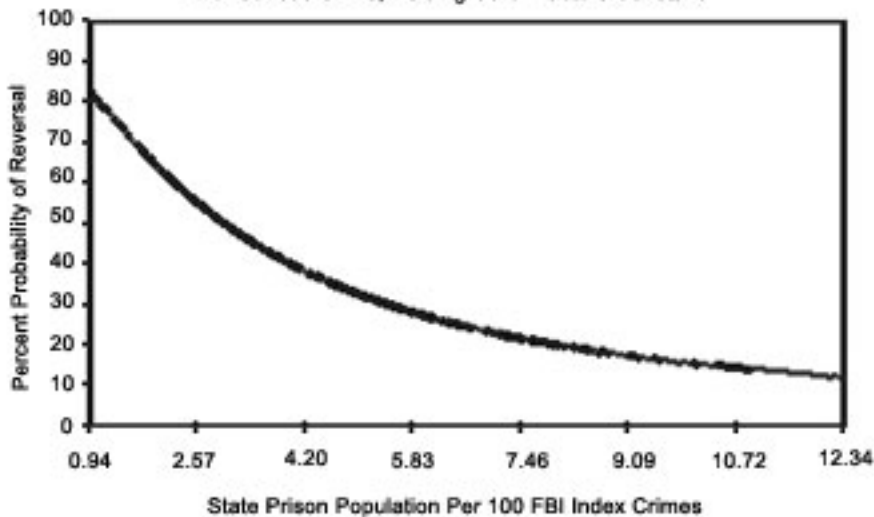
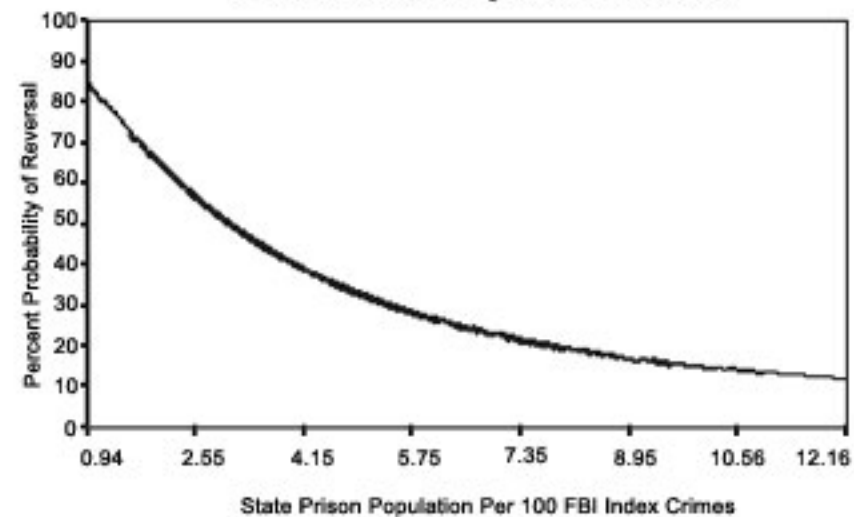


Figure 43L (Analysis 10A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant



Effect Size: Population Structure; State Court Caseloads; Homicide Rate (County - State Analyses 8A, 10A)

Figure 43M (Analysis 10A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on State Population Structure, Holding Other Factors Constant

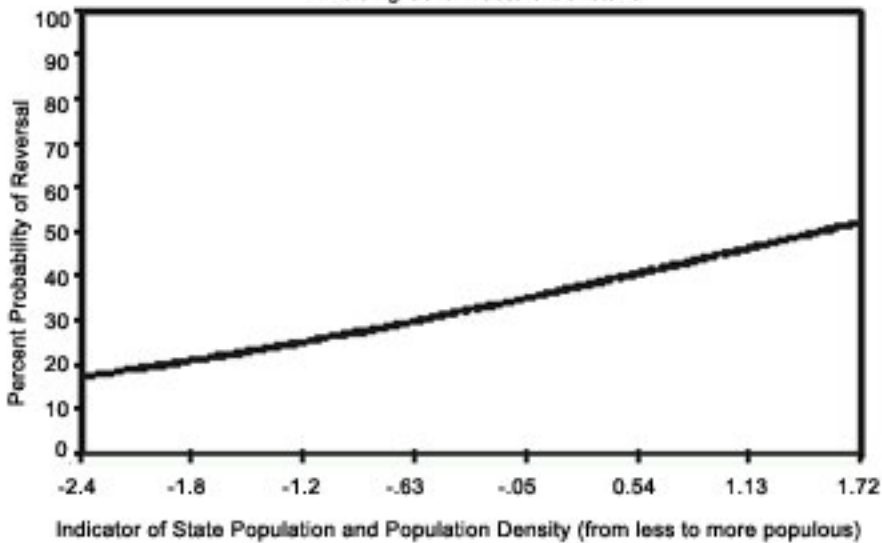


Figure 43N (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Four Factor Measure of Net State Court Caseloads, Holding Other Factors Constant

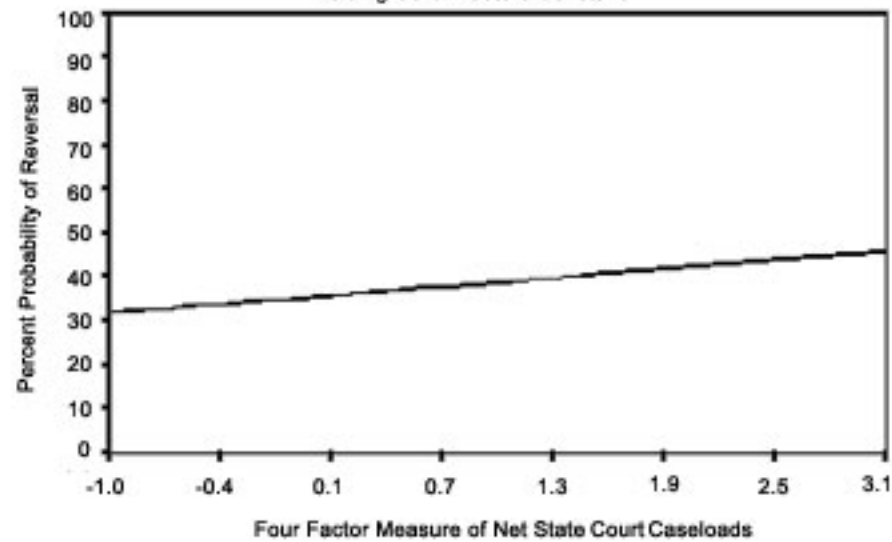
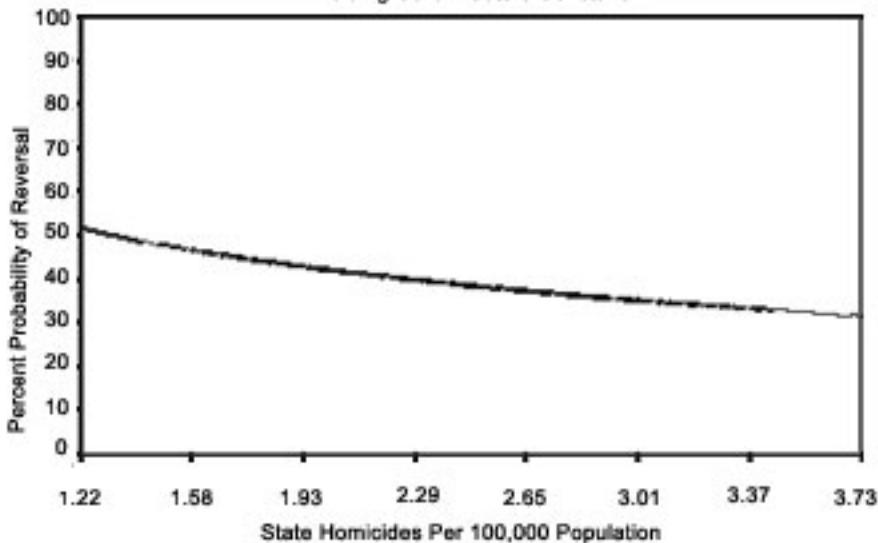


Figure 43O (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Homicide Rates, Holding Other Factors Constant



Effect Size: Year of Death Verdict (Influence of Unfinished Verdicts) (County - State Analyses 8A, 10A)

Figure 43P (Analysis 8A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Year Death Sentence Was Imposed, Holding Other Factors Constant

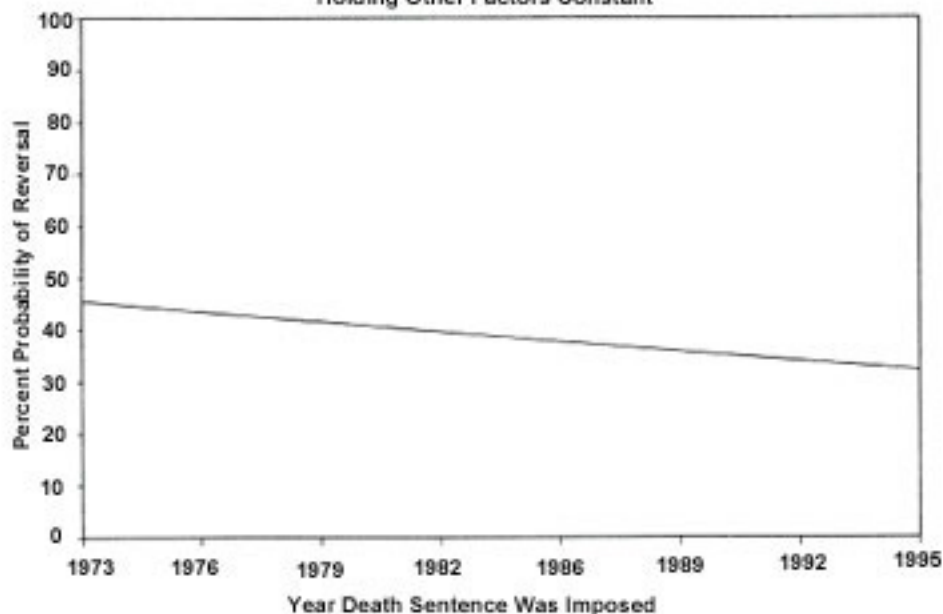
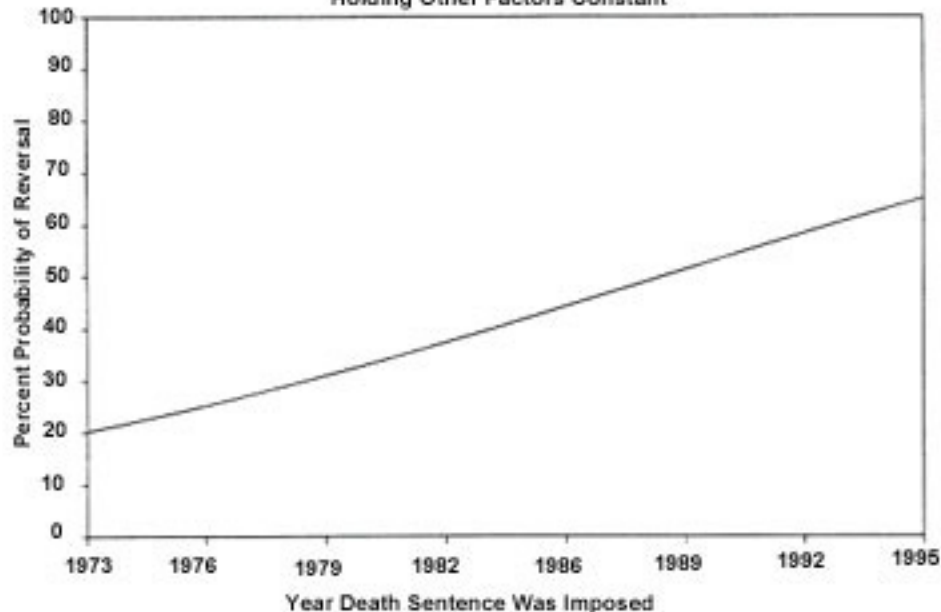


Figure 43Q (Analysis 10A): Probability of Reversal (as proportion of reviewed verdicts; direct appeal stage) Based on Year Death Sentence Was Imposed, Holding Other Factors Constant



3. County-level explanations for county capital error rates: heavy use of the death penalty and population structure.

The results reported above reveal that, **neither (1) the switch from explaining state differences in reversal rates to explaining county differences, nor (2) the inclusion of county-level explanatory factors undermines the importance of the interlocking set of state-level explanations for high capital reversal rates identified by state-only Analyses 1-6, 14 and 15—i.e., aggressive use of the death penalty in conjunction with and possibly in response to specific political, racial and crime-related conditions creating pressure to use the death penalty more frequently.** To this extent, **the three county-state analyses tend to confirm the results and reliability of the state-only analyses.**

Another question is whether including state-level explanatory factors affects the results of Analysis 7, which identified a number of county-level factors that were significantly associated with county reversal rates, when no state factors were considered. The answer is that county-state Analyses 8-10 clarify the role of one of the four county-level factors identified by Analysis 7 (time),⁵³⁰ support two other factors (county death-sentencing rates and population structure) and reveal that state-level factors better capture the effect that a fourth factor appeared to have when only county factors are tested (county homicide rates):

- Analyses 8-10 indicate that, when measured reliably, the amount of reversible error found in death verdicts has not significantly decreased over time, controlling for other factors—and at the direct appeal stage has increased.⁵³¹ This suggests that it is not a decline over time in the amount of reversible capital error, but the lengthy amount of time needed to finally review death verdicts, that explains Analysis 7's association of later verdicts and lower reversal rates.
- In county-state Analyses 8-10, as in county-only Analysis 7, **higher county death-sentencing rates were significantly associated with higher capital error rates, independently of the effect of higher *state* death-sentencing rates.**⁵³²

- In Analyses 8-10, as in Analysis 7, **more highly and densely populated counties had higher capital reversal rates than more sparsely populated counties**. The relationship was significant in Analyses 8 and 9 and just above the .05 significance level in Analysis 10. It coincided with a weakening of the effect of the comparable state-level factor in Analysis 8, where the state-level factor was not significant.⁵³³
- Unlike county-only Analysis 7, county-state Analyses 8-10 found **no significant relationship between county homicide rates and county capital error rates**. Evidently, state-level factors in Analyses 8-10 more reliably captured the effect that this county factor appeared to have in county-only Analysis 7. This may be more evidence of a connection between homicide rates and two state-level factors that are significantly related to reversal rates in Analyses 8-10—(1) the risk of homicide to a state’s white residents relative to the risk to its black residents, and (2) the proportion of a state’s population that is African-American.⁵³⁴ These results also tend to support the interpretation of all these factors as measures of crime fears felt by influential citizens, leading to increased pressure to use the death penalty.

4. Summary: support for results of state analyses.

Although not decisive by themselves, Analyses 8-10 indicate that neither a focus on county as opposed to state capital reversal rates, nor the inclusion of county as well as state explanatory factors, changes the results or challenges the reliability of main Analysis 1 and supporting Analyses 2-4. The same set of forces appears to be at work: Fears about crime among citizens with influence over public officials impel officials to use the death penalty in weak cases where the temptation to cut corners and the risk of mistake is great. For the most part, conditions measured at the state level capture these effects, although two parallel county-level factors also have explanatory power—high death-sentencing rates and population density. These results do not necessarily mean that only or mainly state-level conditions are related to capital error rates. It also could mean that the relevant conditions—despite operating at multiple levels—are more effectively *detected* by the often superior data available at the state level.⁵³⁵

D. Results of County-State Analyses 11-13: Further Confirmation of State-Level Analyses and Results

Five other analyses—11-13, 16 and 17—also assess the power of the state-level explanations identified by main Analysis 1 and supporting state-level Analyses 2-6 to explain differences in county reversal rates and compare state- and county-level explanations for reversal rates.

1. Analyses 11 and 12: the power of state-level Analyses 1 and 2 to predict county reversal rates.

Analyses 11 and 12 use a “predicted value” technique to compare state and county effects. Each analysis uses a set of explanatory factors identified by Analyses 1 and 2 to generate predicted values—*i.e.*, capital error rates the state analyses predict—for each county in the 34 capital states. The predicted values then are treated as a county-level explanatory factor in an analysis designed to determine (1) if the amounts of capital error the state analyses predict for each county are, indeed, significantly related to actual county reversal rates, and (2) whether, after accounting for the values predicted by the state analyses, other local factors are significantly related to county reversal rates.

Analysis 11, a binomial regression analysis, uses the set of significant factors identified by Analysis 1B (also a binomial analysis) to generate predicted county reversal rates. Analysis 12, a Poisson regression analysis, uses the set of factors identified by Analysis 2A (a Poisson analysis) for the same purpose. Both analyses test the power of county reversal rates predicted by 519 observed state reversal rates (34 states x each of the 23 study years in which the state imposed at least one death verdict) and various other county factors to explain 3054 observed *county* reversal rates (967 death-sentencing counties x each of the 23 years in which the county imposed at least one death verdict). In both analyses, reversal rates are the proportion of all death verdicts imposed in the relevant county that were reversed at one of the three review stages.

In two ways, Analyses 11 and 12 make it more difficult than Analyses 7-10 for specific local factors—including the county reversal rates the state analyses predict—to achieve significance. First, Analyses 11 and 12 use a baseline analysis to test the power of county reversal rates predicted by three general factors—state, year and time trend—to explain actual county reversal rates. Second, they treat each of the 967 capital counties as a random effect. These methods keep specific county factors, including the county reversal rates predicted by specific state-level factors, from achieving significance unless they surpass four general factors as explanations for county reversal rates: state, county, year and time trend. And they keep county factors other than the predicted reversal rates from achieving significance unless they surpass the predicted rates as explanations for actual county reversal rates.

The Analysis 11 and 12 baseline inquiries show that at the *state* level, consideration of the effect of state, year and time trend leaves ample variance in state reversal rates to be explained by specific state-level factors,⁵³⁶ but the same is not as true at the *county* level. Thus, the county reversal rates predicted by a baseline analysis of only the state, the year, and the relevant points in the 1973-1995 time trend when the county imposed its capital verdicts do such a good job of explaining non-random variance among county reversal rates that there is little or no variance left to be explained by specific county-level factors.⁵³⁷ This suggests that, although capital reversal rates vary greatly from state to state, variance among *county* reversal rates is harder to detect or to model. This could mean either that reversal rates do not differ much from county to county within states,⁵³⁸ or that the number of homicides and resulting death verdicts in many counties and years is too small to reliably reveal variance that exists.⁵³⁹

In Analyses 11 and 12, the state reversal rates predicted by specific state-level explanatory

factors fit the actual state reversal rates significantly better than the baseline analyses of only state, year and time trend, and greatly diminish the unexplained variance remaining after the baseline analyses. The same is not true for the county level, however. There, the baseline analyses fit the actual data slightly better than analyses of specific county-level factors. The analyses of specific factors do, however, modestly decrease the small amount of unexplained variance left by the baseline analysis.⁵⁴⁰ This provides more evidence that factors associated with the state in which a county sits, or that influence along with the years in which the county imposed its death verdicts, are about as good at predicting county capital reversal rates as any other factors that can be identified—at least given how difficult it is to compare the minute bits of information contained in each county-and-year’s worth of reversal rates.⁵⁴¹ This reveals the value of our state-level analyses: **Assuming policy responses to chronically high capital error rates cannot wait for decades until a larger body of county-level outcomes builds up—especially given that additional data might simply confirm that counties within the same state do not vary enough to make further inquiry possible⁵⁴²—the best way to understand county differences in capital reversal rates is to understand state differences.**

Because Analyses 11 and 12’s study of specific county level factors decrease unexplained variance somewhat, they provide clues about the relationship between specific county-level factors and county reversal rates. Those clues lead to the same conclusion, however: Conditions operating or best measured at the state level provide the best explanations for county-level capital reversal rates:

- **After accounting for the effect of state, county, year and time trend, the county reversal rates predicted by the important state-level factors in Analyses 1 and 2 are highly significant explanations for actual county reversal rates. Effect size is large.⁵⁴³**
- **No other county-level factor is significantly related to county reversal rates.**

2. **Analysis 13: the power of state-level factors to explain county reversal rates.**

Analysis 13 confirms the power of state-level factors and the weakness of most measurable county-level factors as explanations for county reversal rates. This is a Poisson regression analysis of 3054 observed county reversal rates for each of 967 capital counties in the 34 study states in each of the 23 study years in which the county is known to have imposed a death verdict. Unlike Analyses 8-10, this analysis does not include any random effects,⁵⁴⁴ but like those three analyses, it treats each county with at least one death verdict in a known year as a subject variable and nests the county in the state where it is located—programming the analysis to assume that counties in the same state are more like each other than counties in other states. By removing general factors such as county and year, and omitting continuous county-level factors as random effects, this technique considers whether any aspect of our statistical methodology is keeping county-level factors from out-competing state-level factors as explanations of county reversal rates. Reversal rates are calculated as proportions of imposed capital verdicts that were reversed at one of the three review stages during the 23-year study period. Its baseline analysis includes only time trend as a fixed effect.

The reversal rates predicted by two “best” analyses of specific factors do not fit actual reversal rates as well as the baseline analysis, but leave substantially less unexplained variance.⁵⁴⁵ Analyses 13A and 13B provide additional support for three overlapping propositions:

- **Main Analysis 1 identifies the most important set of measurable conditions contributing to capital error rates.**
- **Most significant explanations for capital error rates operate or are best detected at the state level.**
- **The county factor with the most consistent relationship to high county rates of capital**

error—high county death-sentencing rates—operates simultaneously at the state level.

There are nine results common to Analyses 13A and 13B. The first shows the non-significance of time trend. The rest identify significant factors that track results of main Analysis 1. Taking account of all other factors:

- There is **no significant relationship between the passage of time and county reversal rates.**⁵⁴⁶ This is important because the kind of analysis used in Analysis 13 (calculating reversal rates as a proportion of imposed, not reviewed, verdicts) automatically depresses reversal rates for later-imposed verdicts, which are less likely to have been finally reviewed by the study end date.⁵⁴⁷ Despite this non-error-related factor's downward tug on reversal rates over time, there is no significant relationship between later verdicts and lower reversal rates—indicating a **competing upward push on reversal rates from increasing amounts of error over time.**⁵⁴⁸
- **Counties in states with high numbers of backlogged capital verdicts awaiting appeal have low capital reversal rates.**⁵⁴⁹
- **Counties in states with high death-sentencing rates have high capital error rates.**⁵⁵⁰
- **Counties in states with large proportions of black residents have high capital error rates.**⁵⁵¹
- **Counties in states where the homicide risk to whites approaches or surpasses the risk to blacks have higher capital error rates than counties and states where the homicide risk to whites is much lower than to blacks.**⁵⁵²
- **Counties in states with both high proportions of black residents and a high homicide risk to whites as compared to blacks have especially high capital reversal rates.**⁵⁵³
- **Counties in states with judicial selection techniques that put judges under greater pressure to conform their rulings to popular sentiment have higher capital error rates.**⁵⁵⁴
- **Counties in states with low rates of arrest, conviction and incarceration per serious crime have high capital error rates.**⁵⁵⁵
- **Counties in states that are more heavily and densely populated have higher rates of capital error than counties in less populous states.**⁵⁵⁶

Other Analysis 13 findings of interest are:

- Analysis 13B, which reduced unexplained variance more successfully than any other Analysis 13 combination of factors, replicates the entire set of state-level factors identified by main Analysis 1 (Analysis 1B). To the significant factors listed above, Analysis 13B (tracking main Analysis 1B) adds two other factors:
 - **Heavy state court caseloads, like heavy capital appellate caseloads, are related to lower county capital reversal rates.**⁵⁵⁷
 - The interaction of both types of heavy caseloads has the opposite effect: **Counties in states with high capital *and* noncapital caseloads have higher reversal rates.**⁵⁵⁸
- Only one county factor was significantly related to higher county capital error rates in at least one of Analysis 13's groupings of factors—or, with even modest consistency, in other combinations of factors tested in that analysis: In Analysis 13A, **higher county capital-sentencing rates are significantly related to higher county capital error rates— independent of the effect of higher state death-sentencing rates, which also are related to higher county capital error rates.** Effect size is small, however, and the factor is not significant in Analysis 13B.⁵⁵⁹
- Neither state nor county homicide rates were significantly related to county capital reversal rates.⁵⁶⁰ We extensively tested both rates in our Analysis 13 inquiries, but neither made a significant contribution to any useful set of factors.

3. Analyses 16 and 17: the weakness of county-level factors as explanations of counties' and states' 23-year experience with capital reversals.

One hypothesis about the weakness of county-level explanations for reversal rates is that dividing counties into each of 23 years of reversal experience leaves too few death verdicts in most counties and years to study reliably.⁵⁶¹ Analysis 16 (a binomial analysis) and Analysis 17 (a Poisson analysis) test this hypothesis by treating each county, not as up to 23 separate observations—one for each study year in which the county imposed at least one death verdict—but instead as a single observation made up of the county's aggregate reversal rate during the entire 23-year period. Aggregating many years of experience into a single reversal rate increases the number of death verdicts and reversals in each observation, and thus the observation's reliability as a reflection of the county's relative proclivities during the period. Whereas county-years with only one or two death

verdicts account for 88% of the reversal rates analyzed by Analyses 7-13, they account for only 59% of the 23-year reversal rates analyzed by Analyses 16 and 17.

In addition, Analyses 16 and 17—like Analyses 14 and 15, in this respect—directly compare each of the 34 *states*' overall experiences with capital reversals during the 23-year study period. Analyses 16 and 17 do this by treating state as a subject variable, which programs the regression analysis to assume that the capital reversal experience of counties within states is likely to be more similar than the experience of counties from different states. These analyses thus treat each state as a composite of the 23-year capital experiences of all of the state's capital-sentencing counties.

Aggregating each county's various years of capital experience into a single reversal rate, and treating each state's 23-year experience as the composite of its capital counties' aggregate reversal rates, removes time as a consideration. This, in turn, enables us to include in the analysis 35 counties in which death verdicts were imposed during the study period but in unknown years—increasing from 967 to 1002 the number of counties whose reversal rates are being compared. As does Analysis 13, Analyses 16 and 17 treat state and county explanatory factors as fixed effects, testing their comparative strength by forcing state and county factors to compete directly with each other.

In Analyses 16 and 17, highly significant amounts of variance remain to be explained after conducting the baseline inquiries. The best set of specific factors—Analyses 16A and 17A—substantially reduce the unexplained variance left by the baseline analyses, although the improvement in fit is not quite significant.⁵⁶² Controlling for other factors:

- **Counties in states (and states treated as composites of their capital counties), that have high numbers of back-logged capital verdicts awaiting appeal have low capital reversal rates.**⁵⁶³ For effect size, see Figure 44A, p. 276, below.
- On the other hand, **counties in states (and state composites of capital counties) with high**

levels of both capital and non-capital cases awaiting decision tend to have high capital error rates.⁵⁶⁴

- **Counties in states (and state composites of capital counties) with high death-sentencing rates have high capital error rates**—though the relationship is just above the .05 significance level.⁵⁶⁵ Predicted reversal rates nearly double across the range of death-sentencing rates represented by jurisdictions in our study, holding other factors constant. See Figure 44B ([Analysis 16A](#)), p. 276 below.
- **Counties in states (and state composites of capital counties) with high proportions of black residents have high capital error rates.**⁵⁶⁶ Predicted capital error rates again double across the spectrum of jurisdictions in our study with the smallest to the largest African-American populations, when other factors are held constant. See Figure 44C ([Analysis 16A](#)), p. 276 below.
- **Counties in states (and state composites of capital counties) where the homicide risk to whites relative to blacks is high have high capital error rates.**⁵⁶⁷ Predicted reversal rates almost triple as the white homicide rate rises from its lowest level relative to the black homicide rate in jurisdictions in our study to about the same level as the black homicide rate, other factors held constant. See Figure 44D ([Analysis 16A](#)), p. 276 below.
- **Counties in states (and state composites of capital counties) where judicial selection techniques put judges under greater pressure to conform their rulings to public opinion have higher capital error rates.**⁵⁶⁸ Holding other factors constant at their averages, jurisdictions with the least political pressure on judges have expected error rates of about 20%; those placing the most pressure on their judges have expected error rates of about 55%. See Figure 44E ([Analysis 16A](#)), p. 277 below.
- **Counties in states (and state composites of capital counties) with low rates of arrest, conviction and incarceration relative to the number of serious crimes have high capital error rates.**⁵⁶⁹ Predicted error rates range from over 70% for jurisdictions with the lowest rates of apprehension and punishment to about 25% for those with the highest rates, when other factors are held constant. See Figure 44F ([Analysis 16A](#)), p. 277 below.
- **Counties in more heavily and densely populated states (and state composites of such counties) have higher rates of capital error than less populous jurisdictions.**⁵⁷⁰ Capital error rates are expected to rise from less than 15% for the least populous jurisdictions to almost 60% for the most populous jurisdictions, when other factors are held constant. See Figure 44G ([Analysis 16A](#)), p. 277 below.

All of the significant factors listed above are state-level factors. Although comparable county-level factors were tested, none was significantly related to counties' 23-year aggregate reversal rates

or to state composites of those rates. Thus, in an analysis designed to test the hypotheses that larger aggregates of county data would enhance the explanatory power of county-level explanations for capital reversal rates, the opposite occurred: The modest county-level effects identified in the other county-state analyses disappeared. This indicates that **the weak power of county-level explanations for capital reversal rates is not a function of too little information about particular counties in particular years, and instead is a function of the strong explanatory power of the state-level predictors of reversal rates identified by our main Analysis 1, and supporting Analyses 2-6, 14 and 15.**

Effect Size: Backlog of Capital Verdicts; State Death Sentencing Rate; State Court Caseloads; Risk of Homicide to Whites Versus Blacks (Analysis 16A)

Figure 44A (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Backlog of Capital Verdicts Awaiting Review, Holding Other Factors Constant

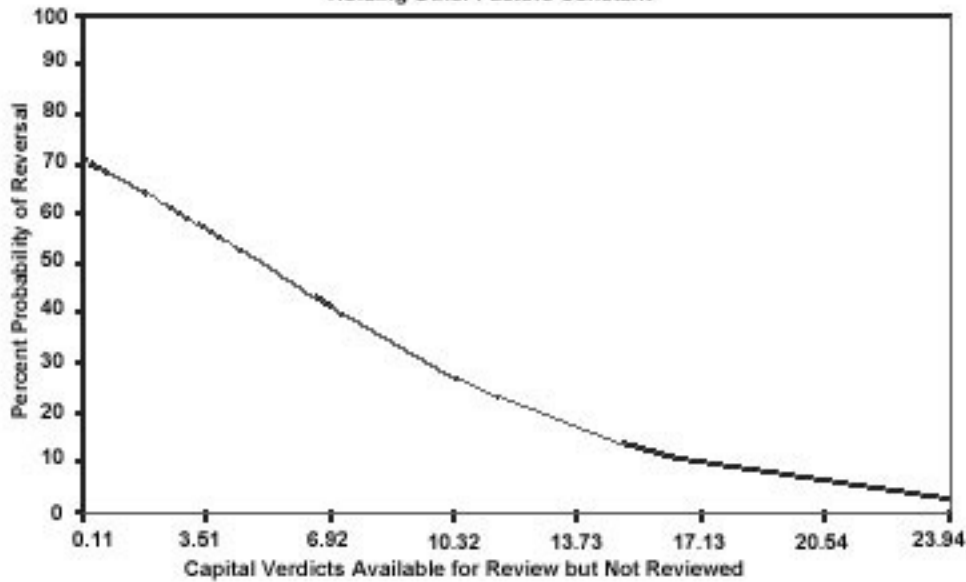


Figure 44B (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Death Sentencing Rate, Holding Other Factors Constant

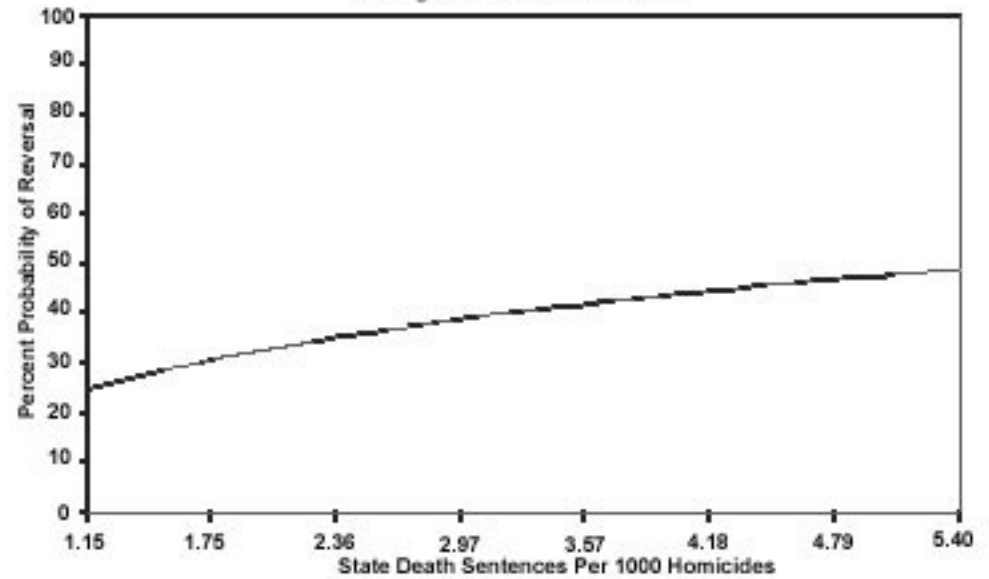


Figure 44C (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Proportion of African - Americans in State Population, Holding Other Factors Constant

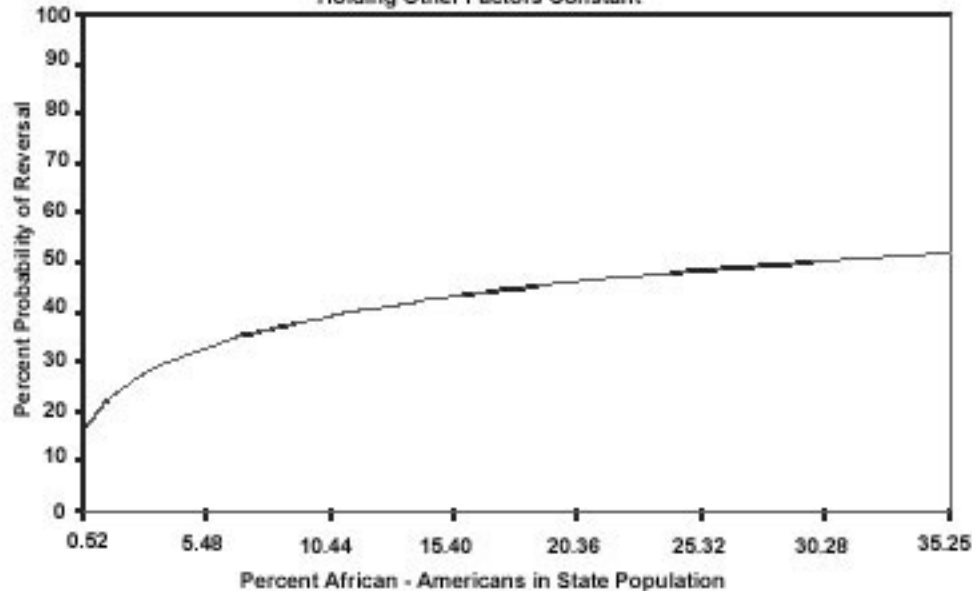
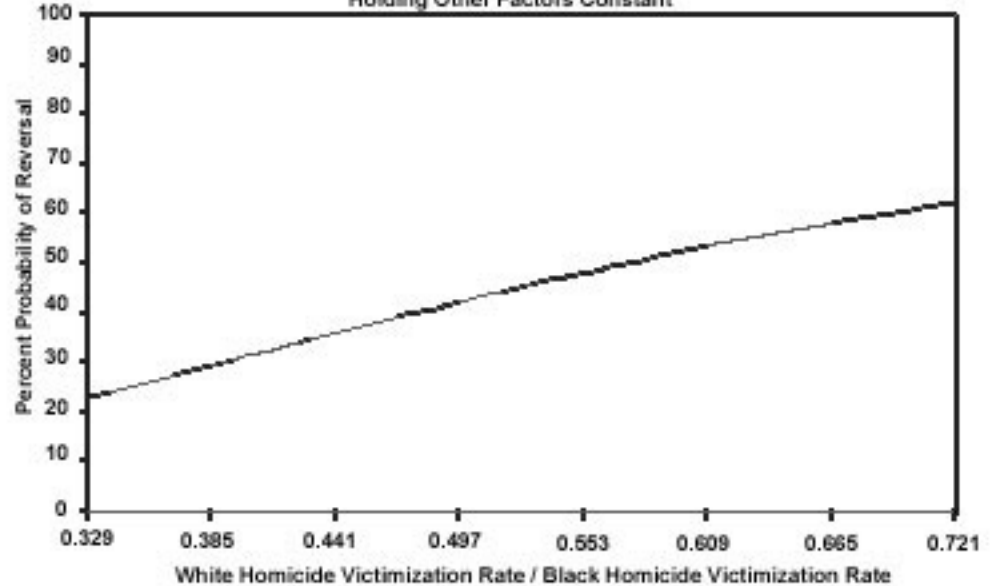


Figure 44D (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Risk of Homicide to Whites Versus Blacks, Holding Other Factors Constant



Effect Size: Method of Selecting State Judges; Population Structure; Rate of Arrest, Conviction and Imprisonment Per Serious Crime (Analysis 16A)

Figure 44E (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Method of Selecting State Judges, Holding Other Factors Constant

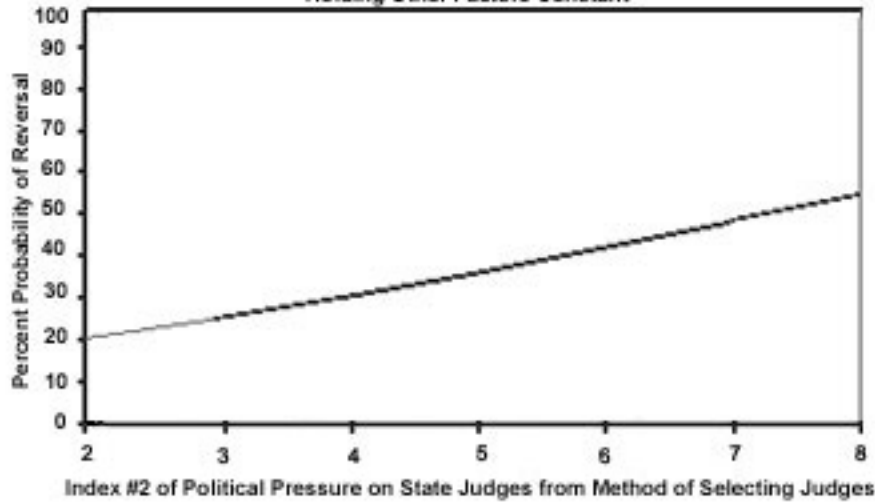


Figure 44F (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on Rate of Arrest, Conviction and Imprisonment Per Serious Crime, Holding Other Factors Constant

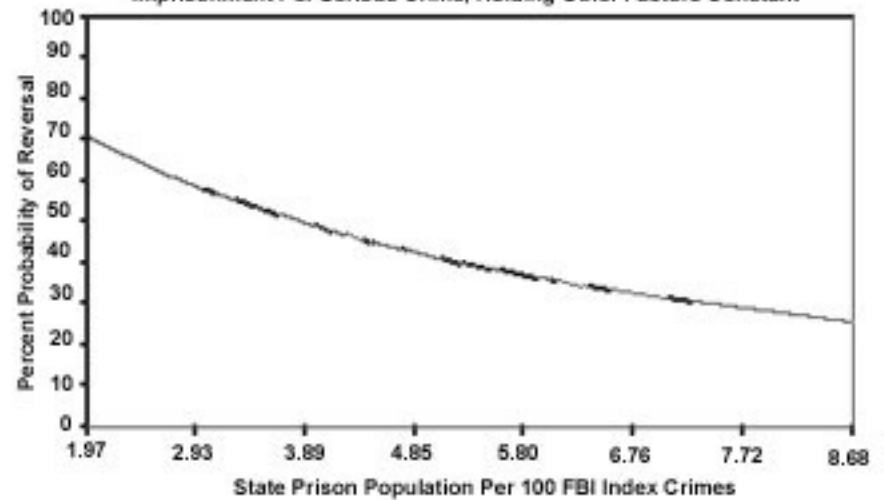
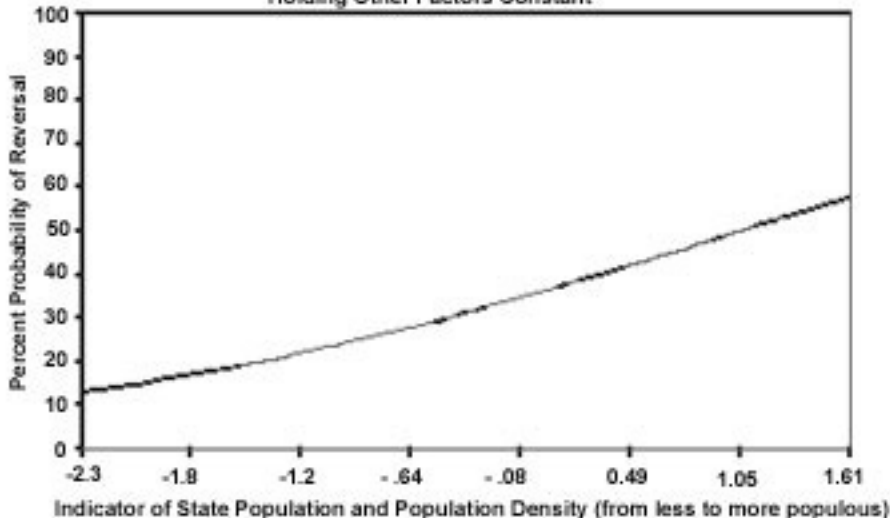


Figure 44G (Analysis 16A): Probability of Reversal (as proportion of imposed verdicts; all 3 review stages) Based on State Population Structure, Holding Other Factors Constant



4. Summary: additional support for results of state analyses.

The main message of county-state Analyses 11-13, 16 and 17 is the same as that of county-state Analyses 8-10. The explanations for differences in rates of serious capital error among *states* identified by Analysis 1 and the other state analyses also are the best explanations for differences in rates of reversible capital error among *counties*. The only county-level conditions that sometimes appear significant in the county-state analyses are county analogues of two significant state factors: death-sentencing rates, and population size and density. Overall, therefore, **understanding the factors related to state capital error rates goes a long way towards predicting county reversal rates in capital cases.**

To seek additional clues about the relative roles of state and county factors in explaining capital reversal rates, we next study differences in capital error rates among counties in each of the three states with the largest pools of capital verdicts and capital-sentencing counties.

E. Results of Analysis 18: County-Only Analysis of Florida, Georgia and Texas

Florida, Georgia and Texas were among the first states to reinstate the death penalty after 1972, when the Supreme Court overturned all pre-existing sentences.⁵⁷¹ During the 1973-1995 study period, those states accounted for one-fourth of the 1002 counties that imposed at least one death verdict, one-third of the death verdicts imposed, 38% of the death verdicts reversed and 40% of the verdicts finally reviewed. Florida led the nation on all these measures, save the number of capital counties.⁵⁷² Each of the three states, and particularly Florida, thus provides a potentially useful laboratory for testing the impact of county-level factors on county reversal rates in a setting in which comparative state effects are not relevant.

During the study period:

- 53 Florida counties imposed at least one death verdict during the 23-year study period. The total number of counties and years with at least one death verdict is 394 (about 7.5 *observations*—*i.e.*, years in which at least one death verdict was imposed—per capital county, on average).
- 88 Georgia counties imposed at least one death verdict during the study period, and there were 251 county-years with at least one death verdict (just under 3 death-sentencing years per capital county, on average).
- 105 Texas counties imposed one or more death verdicts during the study period, and there were 353 county-years with at least one death verdict (3.36 observations per county, on average).

On all these measures, it is possible to say that **Florida and its counties led the nation in death sentencing and the number of reversals during the study period.**

1. Except in Florida, too little county variance for informative analysis.

For each state, we began with both a binomial and Poisson regression analysis of variance among county reversal rates calculated as proportions of imposed death verdicts that were reversed.

The analyses treated county and year as random effects and time trend as a fixed effect. Those factors comprised the baseline analysis. In two of the three states, we encountered a result that helps explain why variance in capital reversal rates by county and year is not as fruitful a focus of analysis as state-by-year variance. Although the Analysis 18 baseline inquiry for Florida left some minimal amount of county-to-county variance to be explained,⁵⁷³ the baseline analyses for Georgia and Texas left no significant variance to be explained beyond that expected as a result of random differences among counties.⁵⁷⁴ None of the baseline analyses left significant unexplained year-to-year variance beyond that expected as a result of random variation.

2. Factors related to higher Florida county reversal rates: higher death-sentencing rates, population density and homicide rates.

Given the above results, we limited Analysis 18 to a Poisson analysis of variance in Florida county reversal rates, with county and year as random effects. Those factors plus time trend comprised the baseline analysis. The diagnostic tests indicate that the results are useful. The reversal rates predicted by the two best analyses (18A and 18B) of specific explanatory factors fit actual reversal rates significantly better than the reversal rates predicted by the baseline analyses, and both analyses left less unexplained variance than the baseline analyses.⁵⁷⁵

Individually and as a set, **the significant explanations for Florida county reversal rates largely track those identified in our main state analysis.** Controlling for other factors:

- **Florida verdicts imposed in later years were less likely to be reversed than earlier verdicts**—at least in part because delays in reviewing cases means that later verdicts were less likely to have finished being reviewed by the study’s end point, and thus that errors were less likely to have been found, and faulty verdicts less likely to have been reversed, by that point.⁵⁷⁶ Effect size is small, however—a predicted decline of only 2 to 3% per year⁵⁷⁷—**suggesting that an upward tug from higher rates of reversible error being detected over time was largely neutralizing the downward pull of delayed review.**

- **Florida counties with higher numbers of death verdicts awaiting review have lower reversal rates**—another result, at least in part, of delays in review.⁵⁷⁸
- **Florida counties with higher death-sentencing rates have higher capital error rates.**⁵⁷⁹ In given years, Florida counties had death-sentencing rates per 1000 homicides ranging from .43 to 400, averaging 16. For each doubling of the death-sentencing rate—*e.g.*, from 2 to 4, to 8, to 16, to 32—Analysis 18 predicts that county reversals will rise by about 15%.
- **More heavily and densely populated Florida counties have higher capital error rates than more sparsely populated counties.**⁵⁸⁰ Effect size is large with predicted reversal rates nearly quadrupling across the range of counties from the least to the most urbanized.
- **In one of the two best analyses, counties with higher homicide rates have higher capital error rates.** The group of factors that includes homicide rates does not improve fit as much as the alternative set of factors, however.⁵⁸¹ Contributing to a pattern noted earlier, homicide rates are significant when two usually important factors are not significant—the size of the black population, and the homicide risk to whites relative to blacks. The county-level versions of these factors were not significant predictors of reversal rates in Analysis 18. (The state-level versions—which are significant in nearly all of our state and county-state analyses—cannot be used in this county-only study of a single state.⁵⁸²) Effect size is modest—about a 10% increase in reversal rates for every doubling of homicide rates.

F. Principal Findings of County Analyses: The Influence of Previously Identified State-Level Explanations for Capital Reversal Rates, Plus County Death-Sentencing Rates and Population Structure

Together with the eight state analyses discussed above, the 10 county analyses described here reveal that **state, not county, conditions are the more informative focus of analysis**. This is mainly because there is more measurable variance to explain at the state than at the county level.⁵⁸³ A possible cause of lower county variability is the smaller number of death verdicts and reviewing decisions in each county and year than in each state and year. Or, this might occur because aggregate state data on factors related to reversal rates better captures the sum of local experiences than do less frequently available and less reliable county data.⁵⁸⁴ At least in part, however, the result seems to reflect the fact that it is **state rather than local policies that are more strongly related to differences in reversal rates**. This latter interpretation is especially likely to be true of two

consistently significant factors that are almost entirely traits of *states*:

- State laws establish the method for selecting state judges, which in turn determines how much pressure judges are under to conform their rulings to popular opinion.
- Capital and general caseloads and delays mainly reflect traits of uniform state court systems.

As is noted above, other significant state-level conditions substantially reflect state policies.⁵⁸⁵

- Rates of death verdicts per 1000 homicides are substantially a result of the narrowness or breadth of state laws defining the range of cases in which the death penalty may be used.
- Rates of apprehension, conviction and incarceration, and the extent of the threat of crime influential citizens face, are strongly affected by states' laws and crime-fighting policies.

Second, **analyses of county reversal rates and county explanations for those rates do not identify significant explanations for state or county capital reversal rates that were not previously identified by main state Analysis 1 and supporting Analyses 2-6, 14-15.** Although the county analyses provide new information—chiefly, that some important factors measured at the state level are also sometimes related to reversal rates when measured at the county level—this result supplements, without altering or neutralizing, state study findings. With the exception of population structure in Analysis 8, there was no suggestion that the effect of any of the important factors when measured at the *county* level deprived the same factor of significance when measured at the *state* level. With that one exception, **significant county factors simply add a county dimension to factors already identified as—and that remain—important when measured at the state level.**

Third, as is clear when Table 8 below is compared to Table 7 above (p. 238), the eight analyses in which state-level as well as county-level conditions were studied point to virtually the same set of clearly important state-level factors as the state analyses had previously identified, supplemented by a smaller number of related but less consistently significant county-level factors.

Table 8: State-Level Factors That Were Sometimes Significant in County-State Analyses, and How Often They Were Significant⁵⁸⁶

Which state-level explanatory factor?	Significant in how many of 8 analyses?*	Significant in what types of analyses?†
% of Population that is African-American	8	all four types
White/Black Homicide Victimization Rate	8	all four types
Index 1, 2 of Political Pressure on Judges	8	all four types
-Prison Population/100 FBI Index Crimes	8	all four types
-Death Verdicts Awaiting Review, 3 Stages	8	all four types
Population Size and Density	7	all four types
Death Verdicts/1000 Homicides	6 (+ 2 just above .05)	cnty-in-st, predval, da st=comp/cntys
% of Population that is African-American x White/Black Homicide Victimization Rate	5 (+1 just above .05)	cnty-in-st, predval, da
Death Verdicts Awaiting Decision x 4- Factor Measure of All State Court Cases Per Capita Awaiting Decision	5	cnty-in-st, predval, st=comp/cntys
-4-Factor Measure of All State Court Cases Per Capita Awaiting Decision	3	cnty-in-st, predval
+4-Factor Measure of All State Court Cases Per Capita Awaiting Decision	1	cnty-in-st
County Reversal Rates Predicted by Full Set of Important Factors in Best State Analyses	2	predval
+Passage of Time (reliable)^	1^	da
-Passage of Time (unreliable)^	2^ (+1 just above .05)	predval
-Homicide Rate+	1	cnty-in-st

As is true of the state-only analyses, the county-state analyses catalogued in Table 8 identify seven factors that are significantly related to capital reversal rates in a large majority of the analyses, and two related factors that sometimes are important:

- **High numbers of capital verdicts awaiting review are related to low county rates of capital reversal—at least in part because they are related to low rates of review.**⁵⁸⁷
 - High per capita rates of backlogged court cases (noncapital as well as capital) had the same effect in some analyses, but effect size is very small.
 - On the other hand, the interaction of both large capital and large general backlogs is associated with *higher* county reversal rates in a majority of analyses.
- **High state death-sentencing rates are associated with higher capital error rates at the county level.**
- **High proportions of African-Americans in a state’s population are associated with higher county capital error rates.**
- **A high risk of homicide to whites in the state, relative to the risk to blacks, is associated with higher capital error rates at the county level.**
 - When this and the preceding racial condition—the proportion of the state’s population comprised of African-Americans—interact, reversal rates are especially high.
- **State judicial selection techniques that increase political pressures on judges are associated with high county capital error rates.**
- **Low levels of arrest, conviction and incarceration of serious criminals, measured at the state level, are associated with high capital error rates at the county level.**
- **Highly and densely populated states tend to have higher county reversal rates in analyses of reversal rates at all three review stages combined and at the direct appeal stage.**
- In addition, **the infrequency with which the passage of time is significantly related to declining reversal rates over time, despite the delay-related downward force on reversal rates in most analyses, plus the strong increase in reversal rates over time in Analysis 10, suggest that, after controlling for other factors, *error* rates increased over time, neutralizing the downward effect on reversal rates of delay.**

The county-state analyses serve two purposes. First, they test the power of each important state-level factor as an explanation for county-level reversal rates—finding them to be important explanations of county, as well as state reversal rates. County-state [Analyses 11](#) and [12](#) also test the power of the *entire set* of state-level factors to predict county-level reversal rates. These latter analyses find that **the county reversal rates predicted by the best set of explanations for state reversal rates are highly significant predictors of actual county reversal rates.**

As Table 9, below, shows, two county-level factors are also significantly related to county capital reversal rates with some degree of consistency:

- **In six of the 10 county analyses, high county capital-sentencing rates are significantly associated with high county capital error rates, and that relationship fell just above the .05 significance level in a seventh analysis.** In each analysis, the contribution this factor made towards explaining county capital error rates was in addition to the contribution made in the same analysis by high *state-level* death-sentencing rates.
- In four analyses, **highly and densely populated counties have higher reversal rates.**

One other county factor was significant often enough to deserve mention:

- In two analyses—both, however, analyzing only county factors—**counties with high homicide rates had high capital reversal rates.** As is noted above, this factor seems to lose power when the relative size of states’ black population, and states’ rates of white relative to black homicide victimization, are included as possible explanations for reversal rates.⁵⁸⁸

Table 9: County-Level Factors That Were Sometimes Significant Explanations for County Reversal Rates, and How Often They Were Significant⁵⁸⁹

Which county-level explanatory factor?	Significant in how many of 10 analyses?*	Significant in what types of analyses?†
Death Verdicts/1000 Homicides	6 (+ 1 just above .05)	cnty, cnty-in-st, da, fl
Population Size and Density	4 (+ 1 just above .05)	cnty, cnty-in-st, fl, (da)
Homicide Rate	2	cnty, fl
-Death Verdicts Awaiting Review, 3 Stages [‡]	2	cnty, fl
-Passage of Time (unreliable) [^]	2 [^]	cnty, fl

G. One Implication and One Illustration of the Relationship Between High County Capital Error Rates and High County Capital-Sentencing Rates

1. An additional explanation for fairly uniform capital error rates among counties in the same state.

The finding that county death-sentencing rates sometimes predict capital reversal rates above and beyond the effect of state death-sentencing rates may help explain why there is relatively little variation among study counties within each state. Recall that study counties are only those using the death penalty at least once during the 23-year study period. As Figures 42A and 42B (pp.248-49 above) demonstrate, however, not all counties in capital states are death-sentencing counties. On the contrary, **nearly 60% of the counties in the 34 study states imposed *no* death verdicts during the 23-year study period.** If a major factor in explaining county-to-county differences in capital error rates is the counties' death-sentencing rates per 1000 homicides, then **an even more important difference among counties in capital states may be between those that never use the death penalty and those that use it sometimes.** Because our analyses examine error rates among imposed or reviewed death verdicts, they exclude counties with no death verdicts. The *entire* set of counties in capital states thus may be more diverse than a study of only death-sentencing counties reveals.⁵⁹⁰

To test this hypothesis, we conducted a simple analysis of whether there is more variability in the use of the death penalty among counties in the 34 death-sentencing states than one would expect by chance (*i.e.*, as a matter of random variation), if one assumes that (1) death-sentencing rates vary *among* states, but (2) within a state, there is a constant probability that any homicide will lead to a death sentence. Given these assumptions, one would expect that 1325 of the 2341 counties in the 34 death-sentencing states would not have imposed a death verdict during the 23-year study period. In fact, the number of counties without a death verdict was 1339—very close to what one

would expect under the assumption of variability *among* states but constancy within them. This result tends to confirm that states and not counties are the main source of capital-sentencing variation, and thus that it is appropriate to focus our main analysis (Analysis 1) on explaining state, not county, differences in capital reversal rates. We intend to pursue this question further in a subsequent phase of our work.

2. County comparisons illustrating the relationship between high death-sentencing rates and high rates of serious error.

What are the top death-sentencing counties in the U.S.? And how often do they make serious capital mistakes? This section addresses these questions with a series of tables comparing the capital-error profiles of comparable counties with higher and lower death-sentencing rates.

At least 1004 counties in the United States imposed one or more death verdicts during the 1973-1995 study period.⁵⁹¹ To identify top death-sentencing counties among those jurisdictions, we used two criteria—number of death verdicts, and rate of verdicts per 1000 homicides. Counties in the 34 active death-sentencing states imposed anywhere from 0 to 190 death verdicts during the study period, with most imposing none or just 1 or 2.⁵⁹² We began our study of high death-sentencing counties by excluding all with fewer than five death verdicts, believing that it is not reasonable to identify such counties as heavy users of the death penalty, no matter what their death-sentencing rates may be.

Although the regression analyses used elsewhere in this Report can do so reliably, it is difficult in a simple comparison of counties like that in this section to meaningfully compare the death-sentencing and error *rates* of counties with fewer than five death verdicts. A county with only one homicide leading to one death verdict during the study period has the highest possible death-

sentencing rate. But if the homicide was highly aggravated—on a par with crimes for which counties that only rarely use the death penalty would have imposed it—this hypothetical county is not really similar to another county with the same 100% death-sentencing rate that sentenced 10 people to death for 10 homicides of widely varying degrees of aggravation. Moreover, the former county has only two possible error rates for its one death verdict—0 or 100%—not because it is a perfectly reliable or perfectly unreliable death-sentencer but because its single death verdict allows only a small set of possible reversal rate outcomes.⁵⁹³ By contrast, the latter county, with 10 death verdicts, has 11 possible reversal rates—0, 10%, 20%, etc. Its reversal rate thus is a more sensitive measure of its actual death-sentencing reliability. By including only counties with five or more death verdicts, we partially avoid the difficulties involved in comparing death-sentencing rates and error rates in counties with very low numbers of homicides and death verdicts.

Among the 1004 death-sentencing counties, 244 (24%) imposed five or more death verdicts during the study period.⁵⁹⁴ We ranked these counties by their rates of verdicts per 1000 homicides during the period when the state in which the county is located had a valid capital statute.⁵⁹⁵ Tables 10A and 10B below list the 15 counties in the U.S. with *50 or more* death verdicts during the relevant part of the study period. Table 10A lists those counties in order of their number of death verdicts. Table 10B lists the same counties in order of their death-sentencing rates. The purpose of the different organizing principles is illustrated by the first two entries in Table 10A. Harris County (Houston), Texas imposed 190 death verdicts during the relevant period, compared to the next closest county, Los Angeles, which imposed 150. But because Houston had many fewer homicides than Los Angeles in the relevant period, the difference between them in death-sentencing rates—*19* death verdicts per 1000 homicides for Houston, versus only 8 per 1000 for Los Angeles (*2.4 to 1*)—is

much greater than the raw number of verdicts would suggest (190 vs. 150, or only *1.27 to 1*).

As is further developed below,⁵⁹⁶ Tables 10A and 10B reveal **wide disparity in death-sentencing rates**. For example, among cities in counties with 50 or more death verdicts, Pima County (Tucson), Arizona had the highest rate of death verdicts per 1000 homicides: *64*. **Pima County homicides were:**

- *21 times* more likely to result in a death verdict than homicides in St. Louis, Missouri;
- *almost six times* more likely to be punished by death than homicides in Dallas, Texas;
- *and from 5 to 13 times* more likely to be punished capitally than homicides in Charlotte, North Carolina (14/1000), Austin, Texas (10/1000) and Richmond, Virginia (5/100)—all of which had about the same number of homicides as Tucson during the relevant period.

When the data are rearranged in order of death-sentencing *rates*, not numbers, an important pattern appears: **Among counties that imposed at least 50 death verdicts during the study period, those imposing more death verdicts per 1000 homicides had appreciably higher rates of serious capital error—and condemned to die much larger proportions of people later shown to be factually or legally innocent—than counties with lower death-sentencing rates:**

- **On average, counties with more than 30 death verdicts per 1000 homicides had combined capital reversal rates at the direct appeal and federal habeas stages⁵⁹⁷ that were 43% higher than the reversal rates of counties with 30 or fewer death verdicts per 1000 homicides.** All six highest death-sentencing counties had overall capital reversal rates at the two review stages of 64% or more; five of the six had overall error rates over 70%.
- **Persons sentenced to die by the higher death-sentencing counties were about 67% more likely to be proven innocent thereafter than those sentenced to die by the lower death-sentencing counties.**
- **The six highest death-sentencing counties account for about 11% of all demonstrably innocent people sentenced to die but are only about six-tenths of 1% of all death-sentencing counties, and account for only 7% of all death verdicts imposed in the study period.**

Table 10A. The 15 Counties With 50 or More Death Verdicts, 1973-1995⁺

County (City), State	Death Verdicts	Homicides	Death Verdicts / 1000 Homicides	Error Rate [†]	# Not Guilty	% Not Guilty
Harris (Houston), TX	190	9,829	19	32%	2	1.1
Los Angeles, CA	150	17,998	8	37%	0	0
Cook (Chicago), IL	138	12,586	11	57%	8	5.8
Philadelphia, PA	127	4,698	27	25%	0	0
Maricopa (Phoenix), AZ	114	2,782	41	84%	5	4.4
Dade (Miami), FL	103	6,936	15	67%	1	.9
Clark (Las Vegas), NV	71	1,288	55	64%	2	2.8
Oklahoma (City), OK	68	1,361	50	75%	3	4.4
Hillsborough (Tampa), FL	67	1,839	36	72%	0	0
Duval (Jacksonville), FL	66	2,232	30	51%	0	0
Pima (Tucson), AZ	63	986	64	71%	1	1.6
Dallas, TX	61	5,682	11	67%	1	1.6
Jefferson (Birmingham), AL	55	2,161	25	55%	0	0
Broward (Ft. Lauderdale), FL	55	2,599	21	84%	2	3.6
Pinellas (St. Petersburg), FL	51	1,018	50	89%	0	0
<i>All 15 Counties</i>	<i>1379</i>	<i>73,995</i>	<i>19</i>	<i>62%</i>	<i>25</i>	<i>1.8</i>
		<i>avg. 4,933</i>		<i>(avg.)</i>		

⁺ Death verdicts, homicides and death-sentencing rates ((death verdicts/homicides) x 1000) are those occurring during the portion of the 1973-1995 period when the state in which the county is located had a valid post-*Furman* capital statute. See supra note 595.

[†] Error rates are the overall capital reversal rates at the state direct appeal and federal habeas stages. See supra n.597.

Sources: DRC en, DADB, HCDB, Vital Statistics.

**Table 10B. The 15 Counties With 50 or More Death Verdicts, 1973-1995:⁺
High vs. Low Death-Sentencing Counties**

County (City), State	Death Verdicts	Homicides	Death Verdicts / 1000 Homicides	Error Rate [†]	# Not Guilty	% Not Guilty
Pima (Tucson), AZ	63	986	64	71%	1	1.6
Clark (Las Vegas), NV	71	1,288	55	64%	2	2.8
Pinellas (St. Petersburg), FL	51	1,018	50	89%	0	0
Oklahoma (City), OK	68	1,361	50	75%	3	4.4
Maricopa (Phoenix), AZ	114	2,782	41	84%	5	4.4
Hillsborough (Tampa), FL	67	1,839	36	72%	0	0
<i>All 6 Counties</i>	<i>434</i>	<i>9,274</i>	<i>47</i>	<i>avg: 76%</i>	<i>11</i>	<i>2.5</i>

— VS. —

Duval (Jacksonville), FL	66	2,232	30	51%	0	0
Philadelphia, PA	127	4,698	27	25%	0	0
Jefferson (Birmingham), AL	55	2,161	25	55%	0	0
Broward (Ft. Lauderdale), FL	55	2,599	21	84%	2	3.6
Harris (Houston), TX	190	9,829	19	32%	2	1.1
Dade (Miami), FL	103	6,936	15	67%	1	.9
Cook (Chicago), IL	138	12,586	11	57%	8	5.8
Dallas, TX	61	5,682	11	67%	1	1.6
Los Angeles, CA	150	17,998	8	37%	0	0
<i>All 9 Counties</i>	<i>945</i>	<i>64,721</i>	<i>15</i>	<i>avg: 53%</i>	<i>14</i>	<i>1.5</i>

⁺ Death verdicts, homicides and death-sentencing rates ((death verdicts/homicides) x 1000) are those occurring during the portion of the 1973-1995 period when the state in which the county is located had a valid post-*Furman* capital statute. See supra note 595.

[†] Error rates are the overall capital reversal rates at the state direct appeal and federal habeas stages. See supra n.597.

Sources: DRC en, DADB, HCDB, Vital Statistics.

Tables 11A and 11B in Appendix B list the 244 counties with at least five death verdicts in order of their death-sentencing rates. Table 11A lists the counties in the order of their ranking from 1st to 244th most frequent users of the death penalty. To assist readers in locating counties, Table 11B groups the same counties alphabetically by state and by counties within states, with a notation of each county's death-sentencing ranking from 1st to 244th. For comparative purposes Table 12, Appendix B, lists all 1004 counties with at least one death verdict during the study period, alphabetically by state and county. **The 244 counties with five or more death sentences that are ranked by death-sentencing rates in Table 11A reflect the same pattern as in Table 10:**

- **Among counties in the top quarter in terms of their death-sentencing rates, 23% had direct appeal/federal habeas error rates of 90% or more.** *One-fifth* of these highest death-sentencing counties had reversal rates of 100%.
- **Among counties in the bottom quarter in terms of their capital-sentencing rates, only 10% had error rates of 90% or more.**

We next examined three sets of counties that are similar to each other in terms of the number of homicides each had to select from in deciding when to use the death penalty during the study period.⁵⁹⁸ As above, we divided each set of comparable counties into two groups—those with relatively high, and those with relatively low, death-sentencing rates. We then compared the capital error rates of the two groups of counties. All three comparisons strongly confirm the pattern noted above: **High death-sentencing counties have high capital error rates.**

Among the top third of capital counties⁵⁹⁹ based on their death-sentencing rates are four with the most homicides: Pima (Tucson), Arizona; Clark (Las Vegas), Nevada; Pinellas (St. Petersburg), Florida; and Oklahoma (City), Oklahoma. These counties had from 986 to 1381 homicides during the study period. Among the bottom third of capital counties given their death-sentencing rates are

12 counties with the same range of (950 to 1400) homicides.⁶⁰⁰ Table 13A, p. 294 below, lists the death-sentencing and capital error rates for the four high death-sentencing counties. Table 13B, p. 295, has the same information for the 12 low death-sentencing counties. The two groups of counties have very similar homicide profiles. The high-sentencing counties averaged 1163 homicides; the low-sentencing counties averaged 1121 homicides. But the high death-sentencing counties imposed over *five times* more death verdicts per homicide than the low death-sentencing counties: *54* verdicts for every 1000 homicides for the high death-sentencing counties, compared to *10* verdicts per 1000 homicides for the low-sentencing counties. These large differences in per-homicide use of the death penalty correspond to large differences in capital error rates:

- **The average capital error rate for the high death-sentencing counties is 67% higher than the average capital error rate for the low death-sentencing counties.**
- **Three of the four high-death sentencing counties have sentenced at least one innocent person to die—two have done so more than once. None of the 12 comparable low death-sentencing counties have been found to have done so.**

One can also compare the *top four* death-sentencing counties, in Table 13A, to the *bottom four* death-sentencing counties in Table 13B. In this more compact comparison, see Table 13C, p. 295 below, the two subsets of counties are even more closely matched. The four highest death-sentencing counties in the group—encompassing Tucson, Las Vegas, St. Petersburg, and Oklahoma City—averaged 1163 homicides per county. The four lowest death-sentencing counties in the group—Little Rock, Arkansas; Nashville, Tennessee; Prince George’s County (suburban Washington, D.C.), Maryland; and Richmond, Virginia—averaged 1156 homicides per county. Despite similar homicide profiles, the two subsets of four counties have very different capital-sentencing profiles: **The four top death-sentencing counties in the group imposed *nine times***

more death verdicts per 1000 homicides than the bottom four counties (54 vs. 6). Associated with these drastically different *capital-sentencing* profiles are, again, drastically different *capital-error* profiles:

- The top four counties in terms of their death-sentencing rates had an average capital error rate nearly twice as high as the bottom four counties (75% vs. 39%).
- Three of the top four death-sentencing counties condemned innocent people to death. None of the bottom four counties did so.

**Table 13A. Capital Error Rates in Top-Third Death-Sentencing Counties*
With Highest Number of (986-1361) Homicides, 1973-1995[†]**

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
Pima (Tucson), AZ	64	986	63	71%	1	1.6
Clark (Las Vegas), NV	55	1,288	71	64%	2	2.8
Pinellas (St. Petersburg), FL	50	1,018	51	89%	0	0
Oklahoma (City), OK	50	1,361	68	75%	3	4.4
<i>All 4 Counties</i>	<i>54</i>	<i>4,653</i> <i>avg.</i> <i>1,163</i>	<i>253</i> <i>avg. 63</i>	<i>75%</i> <i>(avg)</i>	<i>6</i>	<i>2.4</i>

Notes to Tables 13A-C

* Top-third counties are the 81 counties, among the 244 counties with five or more death verdicts, that have the highest rates of death verdicts to homicides. Bottom-third counties are the 81 counties, among the 244 counties with five or more death verdicts, that have the lowest rates of death verdicts to homicides.

[†] Death verdicts, homicides and death-sentencing rates ((death verdicts/homicides) x 1000) are those occurring during the portion of the 1973-1995 period when the state in which the county is located had a valid post-*Furman* capital statute. See supra note 595.

[†] Error rates are the overall capital reversal rates at the state direct appeal and federal habeas stages. See supra note 597.

Sources: DRC en, DADB, HCDB, Vital Statistics.

Table 13B. Capital Error Rates in *Bottom-Third* Death-Sentencing Counties* with Comparable Number of (950-1400) Homicides, 1973-1995⁺

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
DeKalb (sub. Atlanta), GA	17	1,065	18	100%	0	0
Fresno, CA	14	1,256	18	40%	0	0
Mecklenburg (Charlotte), NC	14	1,013	14	64%	0	0
Santa Clara (San Jose), CA	13	1,161	15	22%	0	0
Jefferson (Louisville), KY	12	1,201	15	53%	0	0
Allegheny (Pittsburgh), PA	12	1,145	14	64%	0	0
Travis (Austin), TX	10	975	10	44%	0	0
Contra Costa, CA	9	1,015	9	0%	0	0
Pulaski (Little Rock), AR	7	1,157	8	60%	0	0
Davidson (Nashville), TN	6	1,323	8	29%	0	0
Prince George's (sub. Washington), MD	6	1,074	6	50%	0	0
Richmond, VA	5	1,071	5	17%	0	0
<i>All 12 Counties</i>	<i>10</i>	<i>13,456</i> <i>avg. 1,121</i>	<i>140</i> <i>avg. 12</i>	<i>45%</i> <i>(avg)</i>	<i>0</i>	<i>0</i>

Table 13C. *Bottom-Four* Death-Sentencing Counties⁺⁺

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
Pulaski (Little Rock), AR	7	1,157	8	60%	0	0
Davidson (Nashville), TN	6	1,323	8	29%	0	0
Prince George's (sub. Washington), MD	6	1,074	6	50%	0	0
Richmond, VA	5	1,071	5	17%	0	0
<i>All 4 Counties</i>	<i>6</i>	<i>4,625</i> <i>avg. 1,156</i>	<i>27</i> <i>avg. 7</i>	<i>39%</i> <i>(avg)</i>	<i>0</i>	<i>0</i>

The other identifiable groups of top-third and bottom-third death-sentencing counties that can be compared reliably are counties with 200 to 700 homicides. The eight counties with homicides in this range that are also in the *top* third of all counties based on death-sentencing rates are listed in Table 14A, p. 297. They averaged 409 homicides in the study period, and about 55 death verdicts per 1000 homicides. Figure 14B, pp. 298-99, lists the 36 counties with 200-700 homicides that are in the *bottom* third of counties based on death sentencing rates. The latter counties averaged 438 homicides but only about 15 death verdicts per 1000 homicides. Again, **despite similar homicide profiles, the high and low death-sentencing counties have starkly different error profiles:**

- **Capital error rates are about 63% higher in the high death-sentencing counties than in the comparable subset of low death-sentencing counties.**
- ***Half* of the eight high death-sentencing counties sentenced someone to die who later proved to be innocent. Only 6% (2 out of 36) low death-sentencing counties did so.**
- **Peoplesentenced to die in the high death-sentencing counties were 3½ times more likely to be shown to be innocent than those sentenced in the low death-sentencing counties.**

We can also compare the eight top death-sentencing counties with 200 to 700 homicides to the bottom eight death-sentencing counties in the same group. The top eight, in Table 14A, averaged 409 homicides during the study period. The bottom eight, Table 14C, p. 299, averaged 566 homicides. **The top eight death-sentencing counties imposed nearly 6 times more death verdicts per 1000 homicides than the bottom eight counties (55 vs 10), and had correspondingly greater tendencies towards capital error:**

- **The top eight death-sentencing counties in the group had an average capital error rate nearly 80% higher than the bottom eight counties (78% vs. 44%).**
- ***Four* of the top eight death-sentencing counties sentenced innocent people to die at least once. *None* of the bottom eight counties did.**

**Table 14A. Capital Error Rates in *Top-Third* Death-Sentencing Counties*
With Next Highest Number of (238-612) Homicides, 1973-1995⁺**

County (City), State	Death Verdicts 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
Pasco (sub. Tampa-St. Petersburg), FL	72	279	20	100%	2	10
Robeson (Lumberton), NC	62	340	21	76%	0	0
Baltimore County (suburbs), MD	56	612	34	100%	1	2.9
Bay (Panama City), FL	55	238	13	83%	0	0
Escambia (Pensacola), FL	55	513	28	87%	1	3.6
Horry (Myrtle Beach), SC	54	261	14	82%	0	0
Brevard (Melbourne), FL	50	482	24	54%	1	4.2
Volusia (Daytona Beach), FL	49	546	27	44%	0	0
<i>All 8 Counties</i>	55	3,271 <i>avg. 409</i>	181 <i>avg. 23</i>	78% <i>(avg.)</i>	5	2.8

Notes to Tables 14A-C

* Bottom-third counties are the 81 counties, among the 244 counties with five or more death verdicts, that have the lowest rates of death verdicts to homicides. Top-third counties are the 81 counties, among the 224 counties with five or more death verdicts, that have the highest rates of death verdicts to homicides.

⁺ Death verdicts, homicides and death-sentencing rates ((death verdicts/homicides) x 1000) are those occurring during the portion of the 1973-1995 period when the state in which the county is located had a valid post-*Furman* capital statute. See supra note 595.

[†] Error rates are the overall capital reversal rates at the state direct appeal and federal habeas stages. See supra note 597.

Sources: DRCen, DADB, HCDB, Vital Statistics.

**Table 14B. Capital Error Rates in *Bottom-Third* Death-Sentencing Counties*
with Comparable Number of (200-700) Homicides, 1973-1995⁺**

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
Lauderdale, MS	20	246	5	80%	0	0
Lucas (Toledo), OH	20	498	10	17%	0	0
Lubbock, TX	20	609	12	60%	0	0
Buncombe (Asheville), NC	19	259	5	50%	0	0
Lafayette, LA	19	265	5	25%	0	0
Jefferson (Pine Bluff), AR	18	327	6	100%	0	0
Ventura, CA	18	545	10	14%	0	0
Brazoria, TX	18	273	5	33%	0	0
Cumberland (Fayetteville), NC	18	602	11	63%	1	9
Calcasieu (Lake Charles), LA	18	330	6	100%	0	0
Knox (Knoxville), TN	18	499	9	100%	0	0
Clayton (suburban Atlanta), GA	18	279	5	80%	0	0
Seminole (Orlando), FL	18	335	6	33%	1	17
Virginia Beach, VA	18	335	6	0%	0	0
St. Lucie, FL	18	395	7	71%	0	0
Wichita (Falls), TX	17	287	5	80%	0	0
Santa Barbara, CA	17	287	5	0%	0	0
Douglas (Omaha), NE	17	658	11	68%	0	0
Franklin (Columbus), OH	16	497	8	17%	0	0
Fayette (Lexington), KY	16	315	5	40%	0	0
Tulare, CA	16	515	8	25%	0	0
Bell (Killeen), TX	15	388	6	67%	0	0
Alachua (Gainesville), FL	15	388	6	20%	0	0
Spartenburg, SC	15	453	7	50%	0	0
Gaston (Gastonia), NC	14	347	5	33%	0	0

Table 14B (cont'd). Capital Error Rates in *Bottom-Third* Death-Sentencing Counties* with Comparable Number of (200-700) Homicides, 1973-1995⁺

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
Gregg (Longview), TX	14	348	5	75%	0	0
Bibb (Macon), GA	13	595	8	56%	0	0
Fairfax (sub. Washington), VA	13	376	5	14%	0	0
Hidalgo (McAllen), TX	12	409	5	50%	0	0
Delaware (sub. Philadelphia), PA	12	491	6	0%	0	0
Greenville, SC	11	555	6	40%	0	0
Camden, NJ	11	559	6	100%	0	0
Guilford, NC	11	564	6	60%	0	0
Galveston, TX	11	664	7	44%	0	0
Richland (Columbia), SC	9	634	6	40%	0	0
Salt Lake, UT	8	655	5	20%	0	0
<i>All 36 Counties</i>	<i>15</i>	<i>15,782</i> <i>avg. 438</i>	<i>239</i> <i>avg. 6.6</i>	<i>48%</i> <i>(avg.)</i>	<i>2</i>	<i>.8</i>

Table 14C. Bottom-Eight Death-Sentencing Counties*⁺

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate	# Not Guilty	% Not Guilty
Hidalgo (McAllen), TX	12	409	5	50%	0	0
Delaware (sub. Philadelphia), PA	12	491	6	0%	0	0
Greenville, SC	11	555	6	40%	0	0
Camden, NJ	11	559	6	100%	0	0
Guilford, NC	11	564	6	60%	0	0
Galveston, TX	11	664	7	44%	0	0
Richland (Columbia), SC	9	634	6	40%	0	0
Salt Lake, UT	8	655	5	20%	0	0
<i>All 8 Counties</i>	<i>10</i>	<i>4,531</i> <i>avg. 566</i>	<i>47</i> <i>avg. 6</i>	<i>44%</i> <i>(avg.)</i>	<i>0</i>	<i>0</i>

As a last comparison of this type, we considered all 16 capital counties with 1500 to 3000 homicides during the study period. Table 15, p. 301, compares the seven counties in the group with death-sentencing rates of 20 or more death verdicts per 1000 homicides to the nine counties with death-sentencing rates below 20 per 1000 homicides. (The death-sentencing rates in the former group ranged from 20 to 41 verdicts per 1000 homicides, with an aggregate rate of 28 per 1000 homicides; the corresponding rates in the latter group ranged from 3 to 16 death sentences per 1000 homicides, with an aggregate rate of 11. The former group of counties averaged 2201 homicides per year; the latter group averaged 2075 homicides per year.) **Again, the high death-sentencing counties in the group have higher capital error and innocence rates than the low death-sentencing counties.**

**Table 15. Capital Error Rates in Counties with 1500-3000 Homicides, 1973-1995:
Low Versus High Death-Sentencing Counties***

County (City), State	Death Verdicts / 1000 Homicides	Homi- cides	Death Verdicts	Error Rate [†]	# Not Guilty	% Not Guilty
Maricopa (Phoenix), AZ	41	2,782	114	84%	5	4.4
Hillsborough (Tampa), FL	36	1,839	67	72%	2	5.6
Duval (Jacksonville), FL	30	2,232	66	51%	0	0
Jefferson (Birmingham), AL	25	2,161	55	55%	0	0
Cuyohoga (Cleveland), OH	22	2,053	45	24%	1	2.2
Broward (Ft. Lauderdale), FL	21	2,599	55	84%	3	4.5
Orange, CA	20	1,738	35	100%	0	0
<i>All 7 Counties</i>	28	15,404	437	67%	11	2.5
		<i>avg. 2,201</i>	<i>avg. 62</i>	<i>(avg.)</i>		
— VS. —						
Tarrant (Fort Worth), TX	16	2,636	42	54%	0	0
Alameda (Oakland), CA	15	2,010	31	0%	0	0
San Bernadino, CA	15	1,950	30	100%	0	0
Lake, IN	15	1,500	23	100%	1	4.5
Shelby (Memphis), TN	14	2,219	32	23%	0	0
San Diego, CA	10	2,322	23	60%	0	0
Jackson (Kansas City), MO	6	1,827	11	33%	0	0
Essex, NJ (Jersey City)	4	1,905	8	88%	0	0
St. Louis City, MO	3	2,306	7	0%	0	0
<i>All 9 Counties</i>	11	18,675	207	51%	1	.5
		<i>avg. 2,075</i>	<i>avg. 23</i>	<i>(avg.)</i>		

* Only counties with five or more death sentences. Sources: DRCen, DADB, HCDB, Vital Statistics.

[†] Death verdicts, homicides and death-sentencing rates ((death verdicts/homicides) x 1000) are those occurring during the portion of the 1973-1995 period when the state in which the county is located had a valid post-*Furman* capital statute. See supra note 595.

[†] Error rates are the overall capital reversal rates at the state direct appeal and federal habeas stages. See supra note 597.

Finally, Table 16, p. 304 below, compares the 73 counties with 600 or more homicides during the relevant portion of the study period, ranked by their overall capital error rates at the direct appeal and federal habeas stages.⁶⁰¹ Death-sentencing rates are included so that reversal and death-sentencing rates may be compared. The first page of Table 16 lists the 37 counties with the highest reversal rates; the second page lists the 36 counties with the lowest reversal rates. Table 16 again reveals **a strong association between high capital-error rates and high capital-sentencing rates:**

- **Among these counties, Pima County (Tucson), Arizona has the highest death-sentencing rate: 64 death sentences per 1000 homicides. The City of St. Louis has the lowest rate: 3 death sentences per 1000 homicides.**
- **The 37 counties with relatively *high* error rates include 91% of the counties that imposed more than 30 verdicts per 1000 homicides. The 36 counties with relatively *low* error rates include 76% of the counties with fewer than 10 verdicts for every 1000 homicides.**
- Five counties in Table 16 have death-sentencing rates of 50 or more per 1000 homicides during the study period, all with 34 or more death sentences and 600 or more homicides. **These five top death-sentencing counties had overall capital reversal rates at the direct appeal and federal habeas stages of anywhere from 64% on the low end to 100% on the high end, with an average reversal rate of 80% at those stages.⁶⁰² All but one of the counties put men on death row who later proved to be not guilty.** The five counties are:
 - Pima County (Tucson), Arizona, with a death-sentencing rate per 1000 homicides of 64 and an overall direct appeal and federal habeas reversal rate in the study period of 71%; 1 death row prisoner later found not guilty.
 - Suburban Baltimore County, Maryland, with a death-sentencing rate of 56 and an overall reversal rate of 100%; 1 death row prisoner later found not guilty.
 - Clark County (Las Vegas), Nevada, with a death-sentencing rate of 55 and an overall reversal rate of 64%; 2 death row prisoners later found not guilty.
 - Pinellas County (St. Petersburg), Florida, with a death-sentencing rate of 50 and an overall reversal rate of 89%.
 - Oklahoma County (Oklahoma City), with a death-sentencing rate of 50 and an overall reversal rate of 75%; 3 death row prisoners later found not guilty.

- Five counties in Table 16 have the lowest death-sentencing rates: San Francisco, California; Richmond, Virginia; Fulton County (Atlanta), Georgia; Essex County (Newark), New Jersey; and St. Louis City, Missouri. **These five lowest death-sentencing counties had an average error rate of only 43% at the first and last review stages. None sentenced anyone to death who has since been found not guilty.**

- **The 10 top death-sentencing counties with 600 or more homicides during the study period—in order, Pima County (Tucson), Arizona; suburban Baltimore County, Maryland; Clark County (Las Vegas), Nevada; Pinellas County (St. Petersburg), Florida; Oklahoma (City), Oklahoma; Maricopa County (Phoenix), Arizona; Hamilton County (Cincinnati), Ohio; Hillsborough County (Tampa), Florida; Polk County, Florida; and Muscogee County (Columbus), Georgia—have an average error rate at the direct appeal and habeas stages of 71%. 8 of the 10 counties put a total of 16 people on death row during the study period who were later found innocent.**
 - **In contrast, the 10 lowest death-sentencing counties with 600 or more homicides—San Francisco, California; Richmond, Virginia; Fulton County (Atlanta), Georgia; Essex County (Newark), New Jersey; St. Louis City, Missouri; Pulaski County (Little Rock), Arkansas; Bernalillo County (Albuquerque), New Mexico; Davidson County (Nashville), Tennessee; Jackson County (Kansas City), Missouri; and Prince George’s County (suburban Washington), Maryland—had an average error rate at the direct appeal and habeas stages of 41%, and *none* sentenced anyone to death during the study period who was later found not guilty.**

**Table 16: Overall Error Rates and Death-Sentencing Rates
for All Counties With 600 or More Homicides, 1973-1995***

County (City), State	Overall Reversal Rate (Dir. App. + Fed. Hab. Stage)	Death-Sentencing Rate (for every 1000 homicides)	Homicides
Baltimore County (suburbs), MD	100%	56	612
Orange, CA	100%	20	1738
De Kalb (suburban Atlanta), GA	100%	17	1065
Tulsa, OK	100%	16	794
San Bernardino, CA	100%	15	1950
Lake, IN	100%	15	1500
Richmond (Augusta), GA	100%	10	705
Pinellas (St. Petersburg), FL	89%	50	1018
Multnomah (Portland), OR	88%	13	760
Essex (Newark), NJ	88%	4	1905
Chatham (Savannah), GA	85%	22	787
Maricopa (Phoenix), AZ	84%	41	2782
Broward (Ft. Lauderdale), FL	84%	21	2599
Hinds (Jackson), MS	81%	24	907
Polk, FL	78%	35	894
Oklahoma (City), OK	75%	50	1361
El Paso, TX	73%	18	734
Orleans (New Orleans), LA	73%	9	3126
Hillsborough (Tampa), FL	72%	36	1839
Fulton (Atlanta), GA	71%	4	3314
Pima (Tucson), AZ	71%	64	986
Orange (Orlando), FL	71%	32	1241
Douglas (Omaha), NE	68%	17	658
Dade (Miami), FL	67%	15	6936
Dallas, TX	67%	11	5682
East Baton Rouge, LA	67%	11	857
Muscogee (Columbus), GA	66%	33	607
Clark (Las Vegas), NV	64%	55	1288
Meckleburg (Charlotte), NC	64%	14	1013
Allegheny (Pittsburgh), PA	64%	12	1145
Cumberland (Fayetteville), NC	63%	18	602
Lubbock, TX	60%	20	609
San Diego, CA	60%	10	2322
Pulaski (Little Rock), AR	60%	7	1157
Cook (Chicago), IL	57%	11	12586
Jefferson, LA	56%	16	869
Mobile, AL	56%	28	1298

— VS. —

County (City), State	Overall Reversal Rate (Dir. App. + Fed. Hab. Stage)	Death-Sentencing Rate (for every 1000 homicides)	Homicides
Jefferson (Birmingham), AL	55%	25	2161
Tarrant (Ft. Worth), TX	54%	16	2636
Jefferson (Louisville), KY	53%	12	1201
Duval (Jacksonville), FL	51%	30	2232
Palm Beach, FL	50%	12	1461
St. Clair (Belleville), IL	50%	7	945
Prince George's (sub. Wash.), MD	50%	6	1074
Bexar (San Antonio), TX	48%	13	3275
Travis (Austin), TX	44%	10	975
Galveston, TX	44%	11	664
Fresno, CA	40%	14	1256
Richland (Columbia), SC	40%	9	634
San Francisco, CA	40%	5	1444
Los Angeles, CA	37%	8	17998
St. Louis County (suburbs), MO	37%	26	1387
Kern (Bakersfield), CA	36%	23	961
Jefferson (Beaumont), TX	35%	26	685
Jackson (Kansas City), MO	33%	6	1827
Harris (Houston), TX	32%	19	9829
Riverside, CA	31%	18	1477
Sacramento, CA	29%	22	1329
Davidson (Nashville), TN	29%	6	1323
Philadelphia, PA	25%	27	4698
Cuyahoga (Cleveland), OH	24%	22	2053
Shelby (Memphis), TN	23%	14	2219
Santa Clara (San Jose), CA	22%	13	1161
Nueces (Corpus Christi), TX	20%	20	770
Salt Lake, UT	20%	8	655
Bernalillo (Albuquerque), NM	20%	6	814
Marion (Indianapolis), IN	18%	10	1433
San Joaquin (Stockton), CA	17%	12	769
Richmond, VA	17%	5	1071
Hamilton (Cincinnati), OH	8%	40	727
Alameda (Oakland), CA	0%	15	2010
Contra Costa, CA	0%	9	1015
St Louis (City), MO	0%	3	2306.00

* Includes only counties with five or more death verdicts during the study period. Sources: DRCen, DADB, HCDB, Vital Statistics.

Tables 10-16 provide strong support for two conclusions. The first tracks consistent findings of the regression analyses discussed above. The second follows logically from the first:

- **Aggressive death sentencing is a recipe for high rates of reversible error that undermine the reliability of capital verdicts and the death penalty's effectiveness as a law enforcement tool.**⁶⁰³
- **Jurisdictions with high capital-sentencing, and thus typically high capital-error, rates also have a dangerous propensity to convict and condemn the innocent.**

VI. Results of a Case-Level Study (Analysis 19) of Factors Related to Federal Habeas Reversal: Weak Case for Death, Low Quality State Court Procedures, High Quality Federal Lawyers and Politics

A. Reasons for Doing Case-Level Analysis of Capital Federal Habeas Outcomes

So far, our analyses have considered the effect on rates of serious capital error of attributes of states and counties that use the death penalty—who lives there; how big a threat homicide is, and to whom; how well their law enforcement systems function; how well they support their courts and how much they burden the courts with capital and non-capital cases; how urbanized they are; what political pressures they put on the people who run their capital systems; and how often they use the death penalty. As we have seen, studying the relationship of capital error rates to the traits of the places, people and institutions that adopt and support the death penalty system, operate it, rely and make demands on it as a response to crime, and provide law enforcement alternatives to it, goes a long way towards explaining why the system makes more mistakes in some places than others.

Our final study, Analysis 19, asks a different question: What are the traits of the *particular cases* in which death verdicts are approved or reversed? Above, we discuss the ways in which addressing this question supplements our state and county analyses.⁶⁰⁴ Case-level analysis is extremely labor intensive, because it requires researchers to do all the data collection, as well as data analysis, themselves. There is no public or private source in the nation that systematically and comprehensively collects and distributes such information for even a single county, state or stage of review of capital cases. This is not surprising, given how difficult and painstaking the data-collection task is. The only comprehensive source of information (apart from the many informed participants at each capital trial) are the transcripts and files in each case. But those records run to thousands, or even hundreds of thousands, of pages in each case and are spread throughout tens of thousands of

cities, towns and rural areas in the 34 states where death verdicts were finally reviewed during the study period. Gaining access to all those records for enough cases, states and years to provide an objective and statistically sound basis for comparison lies beyond the capacity of the research community as currently funded, and has never been attempted.⁶⁰⁵

The impossibility of tapping this first source of information forces researchers to rely on a second, less complete but more publicly and centrally available, source of information on each case: the published decision or decisions in which state and federal judges explain why they approved or reversed capital verdicts under review. Even here, the data collection task is enormous, given the more than 7500 lower, intermediate and high court decisions reviewing death verdicts during the 1973-1995 study period. And each such decision may contain hundreds of discrete items of information about the offense, defendant, victim, offense, lawyers, prosecutors, judges, procedures and the like, most of which, however, cannot be counted on to be reported in all or even most comparable decisions in other cases. Thus, apart from naming the defendant, appellate lawyers (with affiliations and locations), the trial judge or court, appellate judges, and (in most states) the aggravating and mitigating circumstances in the case, capital appellate decisions are not expected to report any uniform set of information about the case, but only the information the judge writing the opinion considers important enough to mention in deciding the legal issues he or she chooses to address. Moreover, many legal claims defendants present on appeal are not listed in published opinions, much less discussed. When capital verdicts are reversed, courts often address only the issue requiring reversal, because nothing more depends on the resolution of the other claims. In other cases, groups of legal claims are often decided summarily with phrases such as “all other claims were considered and rejected.” Researchers can moderate this latter problem somewhat by reading every

published decision at every review stage in every case. But doing so multiplies the number of decisions that must be read in each case—and is difficult in some states because of the absence of published state post-conviction decisions.⁶⁰⁶

Together with finite resources, three factors noted above led us to focus our case-level study on the federal habeas stage: the large number of verdicts reviewed at the state direct appeal stage, the far less complete information on cases that were reviewed at only one or the first two review stages, and the absence of published opinions in many state post-conviction cases. A fourth consideration is that our state-level regression analysis covering the federal habeas stage (Analysis 6) had the smallest number of data points to study. Although 598 final federal habeas decisions is a sizeable pool of cases when the outcome of each can be compared to that of the others, those cases provide a smaller basis for judgment in our state analyses, in which they are divided among 28 study states and 23 study years, leaving only 161 reversal rates for particular states and years to compare. That number is less than half the number of observations in our state-level analysis of the state post-conviction stage, and about a third the number of observations in our state-level analysis of the direct appeal stage. Fortunately, therefore, the most feasible focus for a case-level analysis is the review phase that is least comprehensively analyzed in our state-level analyses.

B. The Study

Analysis 19 studies a binary outcome: whether each of 598 death verdicts reviewed on habeas during the 1973-1995 study period was or was not reversed. We use logistic regression to see whether particular factors are significantly related to reversals of death verdicts on federal habeas review when other factors are considered at the same time.

We collected data on the 598 federal habeas cases from 1577 separate court decisions. As

is indicated by the flow chart on p. 94 above, a federal habeas court generally is not permitted to review a death verdict that has not previously been reviewed and approved by at least one, and typically by several, state court decisions at the direct appeal and (in most cases) the state post-conviction stages of review.⁶⁰⁷ An average of about three decisions were read in each case, because we extracted information, not only from the final federal habeas decision, but also from every prior published decision in which a state court approved the same verdict on direct appeal or state post-conviction review and in which a lower federal court reviewed the verdict and either approved or reversed it. We gathered data on 11 categories of case-level traits—sentencing state and county; date and timing of the various milestones and procedures in the case; characteristics of the offense, defendant, victim, defense lawyers and state’s attorneys at trial and on appeal, and judges at the various review stages; procedural history of the case; and legal claims and defenses. See Table 3, pp. 138-40, above. We supplemented this information with publicly available biographical data on practicing lawyers and judges, and with demographic data (race of defendant and victim, juvenile status, executions) from the *Death Row U.S.A.* publication described above.⁶⁰⁸

After collecting and checking the information, then coding and checking the coding, we identified the traits of particular cases on which we had information in most or all cases, and as to which there was enough variance to analyze.⁶⁰⁹ We then conducted simple bivariate tests of each trait to see if it, by itself, was correlated with habeas reversals of capital verdicts—meaning that the trait was present when death verdicts were reversed (or when they were approved) with sufficient regularity that there was only a small probability that the relationship appeared by chance.⁶¹⁰ Because this was a preliminary procedure for culling purposes, we provisionally retained traits if they were statistically significant or close to significance, or if logic or experience suggested they were

important. (A trait that is not significantly related to reversals when considered by itself can turn out to be significant when considered with other factors.) Because some traits are not as important by themselves as they are in combination with other traits that play a similar role in capital cases, we created three indexes, or groups, of traits, used to measure how many traits of a particular type were present in each case:

- aggravating factors in the case that the state's statute expressly identified as a basis for a death sentence minus mitigating factors in the case that were expressly identified as such in a judicial opinion,⁶¹¹
- seven other factors about the defendant (prior criminal record, history of drug abuse, history of alcohol abuse, intoxication at the time of the offense and connection to the community where the crime occurred) and victim (gender and high or low status in the community) that while not always enumerated as aggravating circumstances in the relevant state statute, can have the effect of making the offense appear to be more aggravated,⁶¹² and
- the types of evidence introduced to prove guilt.⁶¹³

The final step was to measure the relationship of all potentially important factors at the same time, seeing which continued to be significantly related to federal habeas reversals of capital verdicts after accounting for the other factors. In comparing factors and sets of factors, we use several diagnostic tests:

- Is the factor's association with federal habeas reversals of capital verdicts statistically significant?⁶¹⁴
- How large is the factor's effect size, as indicated by the size of its exponentiated coefficient estimate?⁶¹⁵
 - The further a factor's exponentiated coefficient is above or below 1, the greater is the factor's effect size. The value is greater than 1 if an increase in the factor is associated with an increased probability of reversal, and less than 1 if an increase in the factor is associated with a decreased probability of reversal. Values for different factors cannot always be directly compared, because factors are scaled differently.⁶¹⁶
 - Effect size is predicted based on the assumption that other factors remain constant

at their average.

- Effect size is an estimate. In the tables below, we indicate the confidence interval for each effect size, which is the range of exponentiated coefficients within which there is a 95% probability that the actual effect size falls.
- How well do the outcomes (reversal vs. affirmance) that are predicted by the entire set of factors fit the actual 598 outcomes being studied?⁶¹⁷

Information on statistical significance, effect size (exponentiated B's) and fit is reported here.

The regression tables are reprinted in full in Appendix G. Scores on some tests (*e.g.*, fit) cannot be directly compared from one analysis to another, because they are sensitive to the number of cases being analyzed and because not all analyses study the same number of cases. For example, some federal habeas decisions are issued “per curiam,” meaning the deciding judges are not named and that the political party of the President who appointed them cannot be determined. Those cases thus must be dropped from any analysis of the “party of appointing President” factor, making it inappropriate to directly compare the fit and related scores for that analysis to the scores for analyses in which the “party of appointing President” factor is not tested and more cases are studied.

C. Factors Related to High Probability of Federal Habeas Reversal: Low Quality State Court Proceedings, High Quality Lawyer, Weak Case for Death, Timing and Politics

As indicated by these tests, four analyses did the best job of explaining capital federal habeas reversals during the study period. Analysis 19A provides a useful benchmark because information on all factors it tests is available for nearly all cases in the analysis (595 out of 598), each of the significant factors it identifies is susceptible to a single, straightforward interpretation, and, among factors with those characteristics, it performs best on the various diagnostic tests. Each successive

analysis (Analyses 19B-19D) adds an additional, theoretically important and statistically significant factor, which seems to improve the overall power of the analysis, but covers fewer cases or is susceptible to multiple interpretations, or both.

1. Analysis 19A: Quality of proceedings and weak case for death.

Analysis 19A identifies the five conditions related to federal habeas reversals of capital verdicts that are listed in Table 17A below.⁶¹⁸ The p-value indicates significance. Effect size and its confidence interval are discussed above.⁶¹⁹ As is noted there, if the effect size is less than 1, an increase in the factor is associated with a decrease in the probability of federal habeas reversal; if effect size is greater than 1, an increase in the factor is associated with an increase in the probability of reversal.

Table 17A: Summary of Results of Analysis 19A (595 Cases)⁶²⁰

Explanatory Factor	p-value	Effect Size	95% Interval
State Evidentiary Hearing Held	.012	.58	(.38, .89)
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.008	1.65	(1.14, 2.38)
# of Statutory Aggravating Factors - # of Mitigating Factors	.026	.86	(.76, .98)
Index of 7 Other Aggravating Factors	.000	.76	(.65, .88)
Federal Evidentiary Hearing Held	.023	1.64	(1.07, 2.52)

a. Quality of court proceedings

Two significant Analysis 19A factors measure the quality of the proceedings that were used to determine the validity of the defendant’s capital verdict, and a third tests the strength of the

defendant's challenges to the verdict. The relationship of all three factors to reversals is as we predicted.

i. Low quality state court review procedure for finding the facts. If the state court review process that precedes federal habeas was of relatively high quality, given that before approving the verdict, the state reviewing court held a hearing and received evidence about its validity, the probability that the verdict will thereafter be reversed on federal habeas is lower. When the state court held a hearing and other factors are held at their averages, the probability of federal habeas reversal declines by over 40%.⁶²¹

Often, the validity of death verdicts turns on factual questions that are not answered by the record made at trial. Borrowing examples from four Supreme Court decisions reversing death verdicts, a verdict's validity may turn on:

- whether the defense lawyer incompetently failed to investigate the case before trial;⁶²²
- whether police officers had, but suppressed, evidence that the crime was committed by someone other than the defendant;⁶²³
- whether officials designed their method of calling possible jurors for trials to keep African-Americans from serving;⁶²⁴ or
- whether a state psychiatrist who examined the defendant before trial misinformed him about the purpose of the psychiatric interview in the process of eliciting damaging statements.⁶²⁵

Federal courts generally are not permitted to consider arguments such as these in favor of reversing death verdicts that were not first made to state courts,⁶²⁶ and must accept state court findings about what happened if the process the state court used to find the facts was reasonable or fair.⁶²⁷ When a challenge to a verdict raises a decisive and unresolved factual question, a reasonable and fair process for resolving the question usually requires an evidentiary hearing at which the state court

hears testimony and other relevant evidence.⁶²⁸ Such hearings are not always required by state law, however, and state courts sometimes fail to hold them, lowering the quality of the state court review process and any findings it makes. Not surprisingly, **that decrease in the quality of the state court review process increases the probability that a federal court will reverse a death verdict.**

Together with results from the state and county analyses, this case-level result suggests that **lower quality state court proceedings—as indicated by state courts’ low funding,⁶²⁹ high caseloads of capital and other cases,⁶³⁰ and unreliable methods of finding out what happened—** increase the probability that death verdicts will later be reversed due to serious error.

ii. High quality federal habeas lawyer. **If the capital defendant’s federal habeas lawyer probably provided high quality, well funded representation—as indicated by the lawyer’s status as an out of state volunteer—it is more likely that a federal habeas court will find serious error and reverse the verdict. When this condition is present and other factors are held at their averages, the probability of federal habeas reversal increases by about *two-thirds*.**

Most capital prisoners are too poor to hire their own lawyers and pay for needed investigation and expert DNA, psychiatric and other analyses.⁶³¹ Those prisoners thus are represented at the federal habeas stage by lawyers appointed or who volunteer to represent them and who must use public or donated funds to pay for investigators and experts. These lawyers fall into four categories, the first two including only in-state lawyers, the second two including mainly out-of-state lawyers:

- Lawyers appointed from the local private bar by federal courts before the federal proceeding has commenced. These lawyers almost always have little or no funding for investigators and experts apart from the resources the federal court provides.
- Lawyers employed by publicly funded capital defense offices that represent capital defendants sentenced by the funding state. These lawyers typically receive lower salaries than they could earn as private attorneys, but their offices sometimes employ investigators and

have budgets for experts more generous than the funds federal courts otherwise provide.

- Lawyers in private for-profit law firms who represent capital prisoners on a volunteer, *pro bono publico* basis. These lawyers typically have high salaries and access to funds for use in employing investigators and experts.⁶³²
- Lawyers employed by private, national non-profit civil rights firms with capital punishment projects who represent capital prisoners across the country on a volunteer basis. These lawyers have fairly low salaries, but often have access to funds for investigators, experts and other support services.

The word “volunteer” in the last two categories refers to lawyers who are not chosen by the court to represent an indigent prisoner, but who instead volunteer their services to capital prisoners who retain them. Volunteer work is not entirely uncompensated. Most lawyers who volunteer are paid salaries or partner shares by their non-profit or for-profit firms using funds from other sources. Moreover, volunteer lawyers sometimes are partially paid by the federal court, which may appoint the lawyers on request by the capital defendant after the federal case is filed.⁶³³ Volunteer lawyers thus often supplement their own resources with modest amounts of government compensation and reimbursement for investigative, expert and other expenses.

There is no rating system for federal habeas lawyers, and the records from which we collected data provide little or no direct information on the quality of the lawyers in each cases. That information thus must be inferred based on the type of lawyer who represented the defendant. Quality is mainly a function of three traits—capital habeas and related experience, training, and resources available for litigating cases.

Experience. Most private lawyers, whether appointed (category 1) or volunteer (category 3), lack capital habeas experience. Public appointed lawyers eventually gain that experience but are subject to funding constraints and high turnover, so cases often are staffed by a single, inexperienced

lawyer. Only private non-profit volunteer lawyers are routinely experienced or work on teams containing at least one experienced lawyer.

Training. Private in-state appointed lawyers typically have no special training or expertise as capital federal habeas lawyers. Although they often have experience as criminal trial lawyers, federal habeas cases are civil proceedings with many more similarities to complex civil litigation than to routine criminal trial work.⁶³⁴ Private volunteer lawyers do not always have capital expertise, but they almost always are experts in complex civil litigation. Moreover, their law firms have the resources to, and often do, pay for their lawyers to attend capital training conferences. And they also often seek advice from publicly funded or non-profit capital punishment projects, which, at the least (public offices) train their lawyers to do capital habeas work and, at best (private non-profits), employ highly experienced capital lawyers who conduct much of the available training for other lawyers from around the country.

Funding. The best funded of the four categories of lawyers are ones from private for-profit firms. Next are private non-profit lawyers, who adhere to professional standards calling for the provision of high-quality legal services but do not always have the funding to fully meet those standards. Then come publicly funded lawyers, who typically have high caseloads and stretched resources that can support only the basic services, but who sometimes employ investigators—an important extra resource. Private, appointed lawyers usually have only the base amount of resources.

Although federal habeas decisions rarely give the professional affiliation of federal habeas lawyers representing capital prisoners, they almost always list the city where the lawyers work. This is a useful indicator of the likely quality of habeas lawyers, because as the table below shows, there

is a distinct tendency for relatively higher quality capital federal habeas lawyers to work out-of-state.

	Capital Habeas Experience?	Special Training/ Expertise?	High Resources?
In state (almost always):			
Private Appointed	No	No	No
Public Appointed	Sometimes	Yes	Sometimes
Out of State (usually):			
Volunteer, For-Profit Firm	No	Yes	Yes
Volunteer, Non-Profit Firm	Yes	Yes	Sometimes

Despite the indirectness of this measure of the quality of representation—which certainly misses some high quality, well-funded in-state lawyers⁶³⁵ and may include some low quality or poorly funded out-of-state lawyers⁶³⁶—the status of the habeas lawyer representing the capital prisoner is a powerful indicator of the probability of federal habeas reversal. **Defendants who are fortunate enough to find an out-of-state lawyer are much more likely to have death verdicts reversed due to serious error than are other defendants. Evidently, the flaws in death verdicts under federal habeas review are serious and common enough—but also sufficiently hard to expose—that the quality of the appointed or retained lawyer matters a great deal.**

Together with prior findings, these two Analysis 19 factors—that a state evidentiary hearing was not held, and that the defendant’s federal habeas lawyer was from out of state—indicate that **the quality of proceedings at each court stage is crucial: Low quality trials generate serious errors; low quality review procedures keep flaws from being found and corrected.**

b. Weak case for death.

Analysis 19A's two remaining significant explanations for federal habeas reversals of capital verdicts reveal that **the less aggravated or more mitigated a capital offense is by two distinct measures, the more likely it is that a federal habeas court will find serious error and reverse.**

i. Low number of statutory aggravating factors minus the number of mitigating factors. As we note above, published court decisions in nearly all capital cases include a list of aggravating circumstances the sentencer formally relied on in sentencing the defendant to death.⁶³⁷ Typically, those circumstances are chosen from a list of qualifying aggravating traits of the crime or offender in the state's capital statute, and must be formally found to be present beyond a reasonable doubt. Generally speaking, at least one such trait must be present before a capital sentence *may* be imposed, with the sentencer then being required to consider all such circumstances in deciding *whether* to impose the penalty. Common aggravating factors are the defendant's prior history of violent crime, the fact that more than one victim was killed or threatened with death, the commission of other crimes such as robbery or rape at the time of the murder, the torturous method of killing, and a finding that the defendant is likely to commit violent crimes in the future.

Published decisions in most capital cases also list the mitigating circumstances the sentencing jury or judge formally found present in the case or, at least, those the defendant relied on as a basis for a sentence less than death that the reviewing court found present or found was supported by enough evidence that the sentencer could have relied on them. All mitigating factors supported by the evidence must be considered in deciding whether to sentence a capital defendant to die.⁶³⁸ Common mitigating circumstances are that the defendant was a juvenile at the time of the killing, acted under extreme emotional disturbance, had no criminal record, or could be rehabilitated.

Most capital statutes require sentencers to balance the aggravating circumstances against the mitigating ones in deciding whether to impose the death penalty, forbidding the death penalty if the mitigating circumstances outweigh the aggravating ones. The remaining statutes require the sentencer to consider both sets of circumstances at the same time, inviting some process by which the impact of one set of circumstances is discounted by the other set. Although this weighing or discounting process considers the quality of the aggravating or mitigating factors under the circumstances, and not just their numbers, the relative numbers provide researchers with the most objectively and consistently measurable estimate of the strength of the case for death—*i.e.*, the degree of aggravation, or what we sometimes call “aggravating net of mitigation.”⁶³⁹

Analysis 19A reveals that **death verdicts federal habeas courts find seriously flawed and reverse tend to be ones where the case for death is weak. The stronger the case for death, the less likely it is that serious error is found and the verdict is reversed. Holding other factors at their averages, Analysis 19A predicts that with each decrease of one in the number of aggravating factors in the case, or each increase of one in the number of mitigating factors, the probability of reversal due to serious error rises by about 14%.**

ii. Low number of other potentially aggravating factors. In our initial bivariate analyses, we found that seven other traits of the offender or victim correlated with federal habeas affirmances: that the defendant had a prior criminal record,⁶⁴⁰ abused drugs or alcohol, was drunk at the time of the offense, or committed the killing in his home community; or that the victim was a woman or had a high status in the community.⁶⁴¹ These circumstances all tend to make the offense seem more aggravated, given the defendant’s bad behavior before or at the time of the crime, his offense against members of his home community or the victim’s special vulnerability or place in that community.

Although as we note below, it might also be appropriate to treat some of these circumstances as neutral (the victim's gender) or as mitigating (*e.g.*, reduced culpability due to intoxication), each is associated with moral blame and has a capacity to aggravate the offense.⁶⁴² We accordingly combined the seven circumstances into an index, scored each case on the number of the factors present in the case, and tested the score as a possible explanation for capital federal habeas reversals.

On this measure like the previous one, **the less aggravated the offense, the more likely it is that reversible capital error was found on federal habeas review. With each decrease of one in the number of the seven factors that is present in the case, the probability of reversal increases by about 24% in Analysis 19A, holding other factors at their average.**⁶⁴³

iii. Summary: reviewing judges serve as substitute (but sometimes unreliable) sentencers. The significant aggravation-related factors support a common finding: **Federal habeas judges tend to find serious, reversible error when the case for the death penalty is weak—*i.e.*, when there are few aggravating factors relative to the mitigating ones.** This finding has several implications.

First, the relationship between reversible error and weak evidence for a death verdict supports an interpretation offered above:⁶⁴⁴ **Error leading to the reversal of death verdicts tends to occur in close cases where officials may be more tempted to cut corners to secure a death verdict, and where doing so is especially likely to change the outcome.**

Second, this result provides additional evidence that **the effects discovered in our prior state and county analyses operate mainly at the level at which capital-sentencing policy is set, not at the more local and particularized levels at which capital policy is implemented.** Our state and county analyses tend to suggest that pressures to utilize the death penalty aggressively in response to high rates of serious crime and low rates of non-capital criminal enforcement lead to high

error rates. This raises the question of the level at which those pressures operate. They might operate on particular cases, causing actors at relatively low levels of the official hierarchy—judges, lawyers and jurors involved in individual cases, but not the actors who set policy at the level of entire courts, prosecuting and defense offices and capital laws—to overreach or be careless in particular cases in which the pressure to punish is greatest. Or, alternatively, the pressures might operate at the level at which policy is set, causing state legislators, appellate and trial judges and district attorneys and police chiefs to write and interpret capital statutes so broadly or to design and implement capital procedures so carelessly that *most or all* capital cases in the jurisdiction are affected. In the former event, one would expect serious error to occur most often in cases with particularly aggravated offenses, because those are the cases where the pressures described above would be the strongest. In the latter event, on the contrary, one would expect serious error to occur when the case for a death verdict is the weakest—*i.e.*, in cases that are *not* very aggravated, and were swept into the capital category only because the threshold for what counts as a capital case is set so low.

Our finding that habeas reversals tend to occur in weak cases for a death verdict supports the latter interpretation: **Overarching death-penalty policy evidently invites officials in some states to use the penalty aggressively in marginal cases. The factually weaker the case for death is, the greater the need to overreach and commit errors in order that police can convince the district attorney to seek the penalty, the district attorney can convince the judge to allow it, and the state’s case and court’s instructions will convince the jury to convict and impose it.**

Earlier, we addressed a similar issue, reaching a similar conclusion. We found that pressures to use the death penalty in response to crime are evidently related to the size of a state’s black population relative to its general population, and to the risk of homicide to whites relative to the risk

to blacks. We then asked whether these factors operate at the level at which capital-sentencing policy is set, or where policy is implemented, concluding that they operate at the policy-making level. **It is not when individual cases are being tried, but when overall death-sentencing policy is set, that the two racial factors seem to generate pressure to overuse the death penalty, and thus to commit serious error. Once those pressures arise, they increase the chance of error in *all* cases in proportion to how weak the evidence for a capital verdict is, not just in cases with black defendants or white victims.**⁶⁴⁵ Analysis 19 supports this interpretation: Neither the race of the particular defendant, nor the race of the victim, nor even a combination of the two, is significantly related to the probability of federal habeas reversal.

Third, the fact that federal habeas judges are more likely to approve death verdicts where overall aggravation is high suggests that **appellate judges act as capital sentencers: They allow execution when the case is particularly aggravated, but bar it when aggravation is low. More crucially, the fact that reversible error is found so often—in 40% of all federal habeas cases, bringing the total reversed at all three court stages to 68%—suggests that federal reviewing judges act not merely as *back-up* capital sentencers in the occasional case in which an improper death sentence has slipped through, but as *substitute* sentencers, who determine the appropriate outcome in *many* cases. And that in turn indicates that the winnowing process at the trial stage—where it is supposed to occur—has failed.**

Regrettably, there are three ways in which these substitute sentencers are not a reliable replacement for the real thing. As is noted above, legal rules generally bar federal habeas judges from reversing due to clear legal error unless it rendered the trial outcome unreliable.⁶⁴⁶ But a finding that the outcome is unreliable does not, by itself, require reversal. On the contrary, **reversals are**

expressly forbidden where the outcome of trial was wrong but no legal error is found.⁶⁴⁷ As the examples in Part III show, one result of rules barring reversal where courts know the evidence is unreliable but no technical legal violation is found is the affirmance of death verdicts imposed on innocent people.⁶⁴⁸ In some cases, that is, appellate judges are poor substitutes for trial jurors, because legal rules keep them from fulfilling jurors' most crucial function: **Barring execution where the evidence for a death verdict is weak.**

In addition, reversal rarely bars retrial and a new death verdict. Although the hope is that retrial will lead to a lesser conviction, a less severe punishment or acquittal if the evidence turns out on retrial to be weak—as probably occurred in many of the retrials that we know ended in sentences less than death⁶⁴⁹—reversal does not *assure* the non-capital outcome that a properly functioning trial system would assure.

Lastly, the seven-circumstance index of supplemental aggravating factors that Analysis 19A finds significantly related to federal habeas affirmances includes some circumstances that might more properly be treated as either neutral (the victim's gender) or as mitigating (the defendant's drug or alcohol impairment at the time of the offense). That federal judges may mis-evaluate such circumstances suggests again that they are poor replacements for properly instructed jurors.

c. Factors that control for the strength of the federal habeas claim.

Analysis 19A finds that **capital verdicts are more likely to be reversed by federal habeas judges who first granted the defendant an evidentiary hearing to inquire into one or more alleged flaws in the verdict than if no federal evidentiary hearing was held. Holding other factors at their averages, a verdict scrutinized at a federal evidentiary hearing is nearly 65% more likely to be reversed on federal habeas review than a verdict that is not scrutinized at**

such a hearing. In one sense, this factor bears out what we discovered above in regard to the quality of federal lawyers: **The higher the quality of the federal proceeding—as indicated here by whether the court heard all the available evidence—the more likely reversal is.** But because legal rules determine when federal judges may and may not hold federal evidentiary hearings, the factor also demonstrates a relationship between the *reason* evidentiary hearings are granted and both higher quality proceedings and the larger number of reversals they produce. We conclude that **an important reason hearings are granted is the strength of the defendant’s claim that his capital verdict is seriously flawed: The stronger the claim that the verdict is flawed, the more likely a federal court is to grant a hearing. And if a hearing is held, the federal proceeding is more likely to be reliable and to result in reversal.** In the discussion that follows, we explain these conclusions and why it was important to include a factor that seems to prove the obvious: stronger claims for reversal more often lead to reversal.

As we have noted above, some claims tend to require proof of facts not already clear from the trial record, while other claims can be resolved based entirely on that record.⁶⁵⁰ As we also have noted, two claims that account for most federal habeas reversals usually require an evidentiary hearing to prove non-record facts: that the defendant’s trial lawyer incompetently failed to find exculpatory evidence, and that police or prosecutors withheld such evidence.⁶⁵¹ Despite this link between evidentiary hearings and certain kinds of claims, the “federal evidentiary hearing” factor is not a measure of whether the capital prisoner raised one of these claims. This is because the vast majority of the 595 cases in Analysis 19A included claims of incompetent lawyering, state suppression of evidence, or both—most of which were denied because they were found to lack merit. Although incompetent lawyering at trial is the most common basis for habeas reversal, most such

claims do not result in reversal. Only the strongest do—because reversible error must be serious error. Nor is there any significant correlation between reversal and having raised particular claims—or at least one claim—requiring an evidentiary hearing.⁶⁵²

Instead of linking the probability of reversals to the *kind* of claims raised, the **“federal evidentiary hearing” factor appears to provide a rough estimate of the *strength* of the claims raised**. The factor thus stands for the expected proposition that verdicts with stronger factual and legal grounds for reversal due to serious error are more likely to be reversed. Including such a factor in the analysis is useful, because, by controlling for the effect of the strength of the claims, the factor permits more confident conclusions that other significant factors have important explanatory power *beyond* the strength of individual claims.⁶⁵³

Federal evidentiary hearings are not legally required, and occurred in only 19% of the cases in our study. Typically, a federal habeas hearing will not be ordered unless four things are true:

- The state courts did not hold an adequate hearing.⁶⁵⁴
- The defendant asked for a hearing in state court, so it is not his fault that a state hearing was not held.⁶⁵⁵
- The claim based on which a capital defendant seeks a hearing is legally valid, meaning reversal is required if the facts the defendant wants to prove at the hearing are as he claims.⁶⁵⁶
- And the defendant can show that there is a reasonable chance that the witnesses he wants to call to testify and other evidence he wants to present will prove that the facts are as he claims.⁶⁵⁷

Item (1) depends on the quality of the evidentiary proceedings in state court. Analysis 19A already controls for that consideration with the “state evidentiary hearing” factor. Moreover, in the cases under study, there is no significant correlation between the fact that a state court held an evidentiary hearing and that the federal court did not.⁶⁵⁸ The “federal hearing” factor thus is

independent of the “no state hearing” factor and reflects the other two conditions listed above: whether the defendant is at fault that no state court hearing was held, and the legal and factual strength of the federal claims. Because of the key role strong claims play in decisions to hold federal evidentiary hearings, because the stronger a claim is, the more likely it is that a federal court will conclude that the state courts’ failure to hold a hearing was not the defendant’s fault,⁶⁵⁹ and because federal courts during the study period sometimes exercised discretion to hold hearings even if the defendant was to blame for the absence of a state hearing but almost never held hearings on claims that were factually or legally weak,⁶⁶⁰ we conclude that the holding of a federal hearing is a fair indication of the legal and factual strength of the claims raised in the petition. This, in turn, suggests that **the other significant factors in Analysis 19A are related to federal habeas outcomes independently of the effect of the strength of the defendant’s claims.**⁶⁶¹

2. Analysis 19B: The added effect of the passage of time.

Analysis 19B adds one factor to the five considered by Analysis 19A: the year the death verdict was imposed.⁶⁶² At a cost of only three cases dropped from the analysis because the published decisions did not say when the verdict was imposed, Analysis 19B performs better on the fit and other diagnostic tests discussed above. Results are summarized below.

Table 17B: Summary of Results of Analysis 19B (592 Cases)⁶⁶³

Explanatory Factor	p-value	Effect Size	95% Interval
State Evidentiary Hearing Held	.026	.61	(.40, .94)
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.024	1.55	(1.06, 2.28)
# of Statutory Aggravating Factors - # of Mitigating Factors	.010	.84	(.74, .96)
Index of 7 Other Aggravating Factors	.005	.80	(.69, .94)
Federal Evidentiary Hearing Held	.036	1.60	(1.03, 2.49)
Year Death Verdict Was Imposed	.000	.86	(.81, .91)

Adding sentence-year in Analysis 19B does not alter Analysis 19A's results. All previously identified factors remain significant, with similar effect size. As for sentence year, Analysis 19B confirms what Analysis 6 already found: Controlling for other factors, death verdicts imposed later in the study period were significantly less likely to have been reversed on federal habeas by the end of the study period than earlier verdicts. As in Analysis 6,⁶⁶⁴ this result clearly occurs in part because of atypically long delays in deciding federal habeas appeals involving flawed verdicts. It is not, however, a reliable indicator of declining amounts of error.

During most of the study period, and all of its later years, federal habeas reversals of verdicts imposed in a given year took one to two years longer to occur than federal habeas affirmances of verdicts imposed in the same year.⁶⁶⁵ As of the study end date, therefore, a disproportionate share of verdicts from particular sentence years whose federal habeas outcomes were delayed beyond the study end date were flawed verdicts. On the other hand, verdicts that were finally reviewed by the end date were disproportionately *without* reversible flaws. Because many more later verdicts were

awaiting review as of the study cut-off date than is true of verdicts imposed earlier in the study period, the bias in favor of counting unflawed verdicts but missing flawed ones has a larger effect on later than on earlier verdicts. So, even if every death-sentencing year has equal shares of flawed and unflawed verdicts, cases finally resolved by the study end date and thus counted in the study will include increasingly larger proportions of affirmances and smaller proportions of reversals for each successive sentencing year—leading the reversal rate reflected by the data to decline over time.⁶⁶⁶ At least the extent, therefore, and possibly the fact, that federal habeas reversals decline over time (taking other factors into account) is a reflection of the longer delays in reviewing flawed verdicts and not of the increasing quality of death verdicts over time.

Some, but not all, of the relationship between later verdicts and fewer federal habeas reversals may also be due to the fact that later verdicts reaching the federal habeas stage were less often flawed than earlier verdicts. Results from Analyses 3, 4 and 10 call for care in assessing this possibility. Those analyses much more accurately measure changes in the quality of death verdicts over time, and reveal that, after controlling for other factors, later verdicts reviewed on direct appeal—where about 80% of all capital reversals during the study period occurred—were found to be flawed more often than earlier verdicts.⁶⁶⁷ **These results bar any confident conclusion that capital verdicts became less flawed over time after controlling for other factors.** They could, however, suggest that federal habeas reversals declined over time (controlling for other factors) because state courts came to shoulder more of the burden of reversing flawed death verdicts, which remained stable or increased over time. Above we discuss evidence that state courts may indeed have done this to compensate for a series of legal changes that hurt the ability of federal habeas courts to cure serious capital error.⁶⁶⁸ This leaves us where Analyses 3, 4, 6 and 10 did: **Taking other factors into**

consideration, state direct appeal courts found significantly more serious reversible error in later capital verdicts than in earlier ones.⁶⁶⁹ There is some evidence that state courts reversed more later verdicts than earlier ones to take compensate for a decreasing federal capacity to cure serious error on habeas.⁶⁷⁰ There is no clear and reliable evidence that, after controlling for other factors, capital verdicts had fewer flaws over time.⁶⁷¹

3. Analysis 19C: An additional measure of the strength of claims.

Analysis 19C adds another significant factor: the number of claims raised at the final federal habeas stage, which is another rough measure of the strength of the claims raised.⁶⁷² Although there is no indication of how many claims were raised at that stage in 34 cases, leaving 559 to be studied,⁶⁷³ the factor is worth considering as another means of controlling for the strength of the claim. Table 17C summarizes Analysis 19C's results.

Table 17C : Summary of Results of Analysis 19C (559 Cases)⁶⁷⁴

Explanatory Factor	p-value	Effect Size	95% Interval
State Evidentiary Hearing Held	.063	.66	(.42, 1.02)
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.016	1.63	(1.09, 2.41)
# of Statutory Aggravating Factors - # of Mitigating Factors	.014	.84	(.73, .97)
Index of 7 Other Aggravating Factors	.023	.83	(.71, .97)
Federal Evidentiary Hearing Held	.020	1.72	(1.09, 2.70)
Year Death Verdict Was Imposed	.000	.87	(.82, .92)
Number of Claims Raised	.000	.86	(.81, .92)

Including the number of claims raised at the final federal habeas stage noticeably affects one significant factor from prior analyses: State evidentiary hearings are still negatively related to federal habeas reversals, but significance is now at the intermediate .06 level, and effect size drops some.⁶⁷⁵

The “number of claims” factor is significant and negatively related to reversal. The more claims raised, the lower the probability of reversal. Holding other factors at their averages, each additional claim raised at the final federal habeas stage is associated with a 14% drop in the probability of reversal. Federal habeas practice explains why this factor roughly reveals capital lawyers’ assessment of the strength of their clients’ best challenges to the death verdict.⁶⁷⁶

There is no limit on how many claims capital prisoners may raise in their petitions that commence federal habeas proceedings in the lowest federal court (the district court). Many district judges also permit briefs of any length in support of those claims. Things change dramatically once the lower court has ruled on a federal habeas petition. At that point, the losing party is permitted to appeal to a U.S. Court of Appeals, and thereafter, in rare cases, is granted permission to be heard on appeal by the U.S. Supreme Court. Because the first of these appeals takes place in the vast majority of capital habeas cases, the appellate decision is almost always the “final federal stage” at which we measured the number of claims raised.

If the capital defendant is *denied* relief at the lower court level, he may appeal to a higher court as long as the lower or higher court believes the appeal raises at least one substantial federal claim in support of reversal.⁶⁷⁷ At this stage, however, the rules tightly limit the number of pages the lawyer may file with the appellate court in support of the client’s claims. This forces lawyers to choose between two strategies: Raise a small number of claims, devoting relatively many pages to a convincing argument on each. Or, raise a larger number of claims, giving appellate judges a wider

array of possible reasons to reverse, but devote fewer pages to each argument. Over the run of cases, it is likely that lawyers will choose between these strategies based on the absolute and relative strength of the claims: If one or two claims are very strong, the best strategy is to focus on them, devoting as many pages as are needed to reveal how clearly they warrant reversal, while omitting weaker claims that might suggest a lack of confidence in the best claims. If the petition contains no particularly strong claim or claims, the preferred strategy is to choose a larger number of relatively weaker claims, thus maximizing the chance that each judge needed to form a majority will find at least one convincing claim. Over the long run, therefore, prisoner appeals raising a smaller number of claims are likely to include stronger claims than appeals raising many claims.

A similar effect arises when the capital defendant *wins* reversal at the lower level. In that event, the state may appeal. Because there usually is only a single basis for reversal, or at most a few, the appeal is limited to that small number of claims.⁶⁷⁸ Because a lower court judge's decision granting relief is a good indication of the strength of the small set of claims on which relief is granted, the small number of claims raised in most state appeals again is associated with the relative strength of the claims.

There are two important ways in which this indication of the strength of claims supplements the information provided by whether a federal evidentiary hearing was held. First, this indication applies to claims that do not, as well as ones that do, require factual development. Second, this factor mainly reflects an assessment of strength of claims by the capital defendant's lawyer (who decides how many claims to raise), not the federal district judge (who decides whether to hold a hearing). For these reasons, including both the "federal evidentiary hearing" and the "number of claims" factors gives us confidence that the other factors identified as important by Analyses 19A-19C (and

19D below) are important even after controlling for the strength of particular claims.

4. Analysis 19D: The role of federal judges’ political affiliations.

Analysis 19D adds one final factor: whether a majority of judges who ruled on the case at the final federal habeas stage were appointed by Republican Presidents.⁶⁷⁹ This factor cannot be measured in 54 cases, because the judges responsible for the final federal habeas decision issued it “*per curiam*” (for the court), meaning they were not named in it.⁶⁸⁰ Including this factor in Analysis 19D leaves 535 cases to be studied⁶⁸¹ but appears to improve performance on the diagnostic tests detailed in Appendix G. Table 17D summarizes results.

Table 17D: Summary of Results of Analysis 19D (535 Cases)⁶⁸²

Explanatory Factor	p-value	Effect Size	95% Interval
State Evidentiary Hearing Held	.079	.66	(.42, 1.05)
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.003	1.84	(1.23, 2.76)
# of Statutory Aggravating Factors - # of Mitigating Factors	.006	.82	(.71, .94)
Index of 7 Other Aggravating Factors	.034	.83	(.70, .99)
Federal Evidentiary Hearing Held	.003	2.04	(1.28, 3.24)
Year Death Verdict Was Imposed	.000	.88	(.83, .94)
Number of Claims Raised	.000	.87	(.81, .93)
Majority of Judges Appointed by Republican Presidents	.055	.68	(.46, 1.01)

Earlier we showed that most federal habeas judges on most federal panels that voted to reverse capital verdicts during the study period were appointed by Republican Presidents, which,

given the Republican Party's strong stand against judicial interference with any but intolerably flawed executions, suggests the seriousness of the error federal habeas courts found.⁶⁸³ Supporting this suggestion is Analysis 19D's finding that **judges appointed by Republican Presidents are in fact less likely to reverse death verdicts than judges appointed by Democratic Presidents. This relationship is significant at the .055 level and predicts a one-third decline in the probability of reversal if a majority of the deciding judges were appointed by Republican Presidents, holding other factors constant.** (In an analysis in which sentence-year was omitted because of its sensitivity to delay, as opposed to more interesting explanations for a higher or lower probability of reversal,⁶⁸⁴ party affiliation of deciding judges was significant at the .01 level.)

The other factors considered in Analysis 19C remain significant in Analysis 19D and have approximately the same effect sizes, except that the effects of being represented by an out-of-state lawyer and receiving a federal evidentiary hearing increase. In this analysis (holding all other factors constant at their average as each factor is tested), **the predicted probability of reversal:**

- **decreases between 10 and 15% for each successive sentence year, and each additional claim raised in the final federal habeas decision;**
- **decreases between 15 and 20% for each additional statutory aggravating factor net of mitigating factors, and for each additional supplementary aggravating factor;**
- **decreases by *about a third* if a state court held an evidentiary hearing, although this factor is significant at only the .08 level;**
- **decreases by about that *same amount* if most members of the final panel of reviewing federal judges were appointed by Republican Presidents;**
- ***almost doubles* if the capital defendant is represented by an out-of-state lawyer; and**
- ***more than doubles* if the defendant is afforded a federal evidentiary hearing.**

D. Conclusion: Support for Prior Results Linking Reversals to Broad Use of the Death Penalty, Low Quality State Court Proceedings and Politics

Apart from the legal and factual strength of the defendant's claims (as indicated by whether a federal evidentiary hearing was granted and by the number of claims raised at the final habeas stage), four factors are reliably associated with an increased probability of federal habeas reversal:

- the trial jury's or judge's imposition of death for an offense that was not very aggravated;
- the low quality of the state review proceedings, as indicated by the denial of a state evidentiary hearing;⁶⁸⁵
- the high quality of the federal review proceedings, as indicated by relatively skilled and well-funded, out-of-state lawyers and also by the grant of a federal evidentiary hearing; and
- the political affiliations of federal habeas judges (judges appointed by Republicans being less likely to reverse).⁶⁸⁶

Analysis 19's findings about factors related to federal habeas reversals of death verdicts overlap a number of findings from our state and county analyses:

- The first Analysis 19 finding above echoes the most consistent finding of the state and county analyses: **When jurisdictions and officials use the death penalty expansively, extending it to offenses that are not highly aggravated, the probability that verdicts they impose will be seriously flawed increases significantly.**⁶⁸⁷
- Like some state and county analyses, **case-level Analysis 19 links reversals to low quality state court proceedings**—as indicated in other analyses by heavy capital and non-capital caseloads⁶⁸⁸ and low funding⁶⁸⁹ and here by a failure to use fair procedures to find facts (the second finding above).
- Just as high quality state review proceedings leave less serious error to be cured on later federal review, higher quality federal review is associated with more reversals due to findings of serious error. As the third finding above reveals, even after controlling for the strength of the claims presented—*i.e.*, **even among cases in which the factual and legal reasons for reversal are equally strong—higher quality federal review procedures and personnel are associated with the discovery and cure of more serious capital error.**
- Three Analysis 19 findings—the first finding as applied to some of the seven supplemental aggravating factors, as well as the third and fourth findings—support another earlier

conclusion: **Appellate review is by no means a failsafe substitute for flawed capital sentencing at trial.**⁶⁹⁰ **On the contrary, federal judges:**

- **seem to treat some neutral and mitigating factors as aggravating ones;**⁶⁹¹
- **fail to reverse verdicts due to serious error they would have reversed if the indigent prisoner had been lucky enough to have a more skilled and better funded, out-of-state lawyer;**⁶⁹² **and**
- **are susceptible to political influences related to their affiliated political parties,** tracking earlier findings that a fear of controversy may keep state court reviewing judges from reversing flawed rural and small town death verdicts,⁶⁹³ and that state post-conviction judges and to a lesser extent state direct appeal judges are susceptible to political pressures from judicial selection methods.⁶⁹⁴
- As does state-level habeas regression Analysis 6,⁶⁹⁵ Analysis 19 measures fewer reversals of later than earlier verdicts. In both analyses, however, this effect is at least in part a function of longer delays for habeas reversals than affirmances, which kept us from counting disproportionate numbers of habeas reversals by pushing them beyond the study end date. Given this bias against counting flawed verdicts, and given more reliable Analysis 3, 4 and 10 findings that direct appeal reversal rates are significantly higher for later than for earlier verdicts, Analysis 6 and 19 provide no reliable evidence that later verdicts are less flawed than earlier ones.⁶⁹⁶

VII. Summary and Interpretation of Results: The Strongest Predictor of Serious Capital Error Is Aggressive Use of the Death Penalty, Extending to Weakly Aggravated Homicides, in Response to Political, Race-Related and Law-Enforcement-Related Fears and Pressures

A. Summary of Methods

Parts IV-VI of this Report describe the results of 19 separate statistical analyses of state- and county-level factors related to high state and county rates of reversible capital error, and of case-level factors associated with a probability of federal habeas reversal of death verdicts. The analyses use a variety of statistical techniques, including classic logistic, over-dispersed binomial logistic and over-dispersed Poisson logarithmic regression analyses to identify factors that explain why some states and counties have more capital error than others and why some capital verdicts are reversed on federal habeas review and others are not. To assure that they are comprehensive, conservative and reliable, the analyses:

- use a variety of statistical methods with different assumptions about the arrangement of the condition being studied—capital reversals and reversal rates—to ensure that it is relationships in the data, not statistical methods, that drive the results;
- analyze reversals and reversal rates at each of three stages of court review of capital verdicts—state direct appeal, state post-conviction and federal habeas—and at the three stages combined;
- use different methods to analyze the simultaneous effect on reversals and reversal rates of important general factors, such as state, county, year and time trend, and specific conditions that may explain capital reversals and reversal rates;
- examine explanatory factors operating at the state, county and case level;
- were all subjected to tests for statistical significance, variance left unexplained, fit between predicted and actual results, and effect size; and
- were tested for consistency within analyses and across analyses to determine which form of analysis, which individual factors identified as statistically significant, and which interlocking sets of significant factors are the most robust and warrant the most confidence.

The 19 regression analyses were supplemented with two sets of case studies, each of which broadened the focus from serious, reversible capital error, to another kind of serious error: the capital conviction and sentencing of people later shown to be innocent of a capital crime. The first set of case studies examine why full sets of reviewing courts at all three review stages *approved* the execution of four innocent men who thereafter were saved only by the fortuitous, eve-of-execution discovery of exculpatory DNA, the reinterpretation of an exonerative video tape after a decade of apparently minor discoveries cumulated to discredit false testimony that had emarginated the tape at the original trial, and an actual perpetrator's confession to intrepid college students taking part in a class project.⁶⁹⁷ The second set of analyses examines the capital-error records (including for convicting and condemning people later shown to be not guilty) of paired sets of American counties with similar numbers of homicides but different rates of using the death penalty.⁶⁹⁸

Our basic approach in using this array of statistical methods and case studies, explanatory factors and controls, and diagnostic tests was to start with one analysis reflecting our best judgment about the most reliable way to study conditions associated with serious, capital error (Analyses 1), then systematically to address possible objections to that analysis with alternative methods that evaluate or avoid the objection. Our choice of results to treat as worthy of attention and analysis, and to carry forward to this section's interpretation of all results as a whole, is conservative: Unless there is a substantial basis for confidence in a result, given the methods used to reach it, its statistical significance, its performance on the other diagnostic tests, its consistency with results of other analyses and its consistency with logic and experience, we omitted it from further consideration. We use the same approach here in analyzing the factors and interpretations that have survived this

gauntlet of tests and comparisons.

As we show in *Broken System, Part I*, and in Part III of this Report, high rates and amounts of serious, reversible capital error have broken the nation's death penalty system. We begin here with our single, principal conclusion about the condition most strongly and consistently associated with high rates and amounts of reversible capital error:

- *The more aggressively officials use the death penalty—the more often they use it and the more frequently they apply it to homicides that are not highly aggravated—the greater is the risk that any death verdict they impose will be seriously flawed.*

We also reach **five supporting conclusions grounded in the study results that expand our understanding of the principal conclusion:**

- *Several conditions that are strongly associated with serious capital error have a common tendency to increase pressure on officials to use the death penalty aggressively:*
 - *the risk of homicide to the entire community, especially when the risk to politically influential citizens approaches or exceeds that to other citizens—as measured here by how close the homicide risk to whites comes to equaling or surpassing the homicide risk to blacks;*
 - *crime fears associated with racial and possibly economic conditions—as measured here by the proportion of the population that is African-American, and by the amount of spending and number of residents on welfare;*
 - *well-founded doubts about the ability of the state's law-enforcement system to deal effectively with crime through arrest, conviction and incarceration; and*
 - *state trial judges' susceptibility to being harmed politically if their capital rulings do not conform to popular sentiment.*
- *Overuse of the death penalty causes harms beyond serious, reversible error, including cost, delay and the system's inability to achieve its most basic goals.*
- *Poor quality trial proceedings—which are in part a function of heavy use of the death penalty—also appear to increase the risk of serious, reversible error.*
- *After controlling for other factors, conditions leading to capital reversals at the state direct appeal stage of review—which accounts for 79% of all reversals—have gotten*

substantially worse over time, given the strong association between later verdicts and higher reversal rates. The same may be true at the other review stages. There is no reliable evidence that conditions creating serious capital error have improved over time.

- *State and federal reviewing judges are themselves susceptible to political pressure and mistake, and thus are not a reliable substitute for careful and accurate capital trials.*

The principal conclusion and most of the supporting conclusions are obvious implications of strong and consistent study results requiring little interpretation. The first and last supporting conclusions rely additionally on logic and experience. We are confident in the reliability of all of these conclusions and their strength and sufficiency as bases for changes in policy. All of them inform our sense of urgency about the need for serious policy reforms. The principal conclusion drives most of the policy suggestions in Part VIII below.

B. Principal Conclusion: Heavy Use of the Death Penalty Extending Beyond Highly Aggravated Homicides Substantially Increases the Risk of Serious Capital Error

Recently, the *Washington Post* quoted a statement by Joshua Marquis, District Attorney of Clatsop County, Oregon, and a Board Member of the National District Attorneys Association, that “[t]here is a growing acknowledgment generally that the death penalty should be reserved for the worst of the worst.”⁶⁹⁹ A few weeks earlier, Virginia’s Governor, James Gilmore, expressed the same sentiment on CNN: The death penalty should be “reserved only for the worst possible cases.”⁷⁰⁰ **The state-, county- and case-level results underlying our major finding reveal the wisdom of these views, and the need to enforce the “worst of the worst” principle strictly in order to bring serious capital error under some sort of control.**

1. High state-level capital-sentencing and high capital-error rates.

States vary widely in how often they punish homicides with death. During the 23-year study

period from 1973 to 1995, 34 active capital states imposed death verdicts in one or more of those years, totaling 519 sets of states and years. **The average rate of death verdicts imposed in all 519 states and years was 18 per 1000 homicides. But rates ranged from about 1 death verdict per 1000 homicides (e.g., in Illinois in 1977) to 208 in Idaho in 1982.**

Figure 11, p. 121 above, compares states based on how often they used the death penalty during the entire study period. **Death-sentencing rates per 1000 across that period homicides ranged:**

- **from *less than 5* in Connecticut, Colorado and New Mexico, and *between 5 and 10* in Maryland, New Jersey and Washington;**
- **to *around 10* in California, Kentucky and Louisiana, and *around 12* in Illinois, Indiana, and Virginia;**
- **to from *32 to 37* in Alabama, Florida and Montana, and *around 45* in Arizona, Delaware, Nevada and Oklahoma;**
- **to *60* in Idaho.**

The most consistent finding of our 19 analyses is that these **disparities in capital-sentencing rates are strongly associated with disparities in capital-error rates. The more death verdicts jurisdictions impose per 1000 homicides, the more likely it is that any single death verdict they impose will later be reversed due to serious capital error.** This is a significant finding of:

- our main regression Analysis 1,⁷⁰¹
- most of the 17 confirming state and county regression analyses;⁷⁰²
- analyses of all three review stages combined,⁷⁰³ and of two of the three review stages individually,⁷⁰⁴ with supporting results from our case-level study of the remaining (federal habeas) stage;⁷⁰⁵

- analyses designed to explain county reversal rates, as well as state reversal rates;⁷⁰⁶
- analyses identifying explanatory conditions operating and measurable at the county level—death-sentencing rates being the main, significant county-level explanation for county reversal rates⁷⁰⁷—as well as analyses identifying explanatory conditions operating and measurable at the state level;⁷⁰⁸ and
- our county case studies of capital-sentencing and capital-error rates.⁷⁰⁹

This explanatory factor has a large predicted effect on rates of serious capital error.

Analysis 1—the most complete analysis of our detailed data on capital reversal rates⁷¹⁰—predicts that capital-error rates will increase from less than 15% to more than 75% as death-sentencing rates rise from the lowest to the highest levels among states and years in our study, holding other explanatory factors at their averages.⁷¹¹ Predicted increases in error rates are especially steep around the average death-sentencing rate, meaning small changes in death-sentencing practices within the range where most states operate are predicted to have large payoffs in terms of reduced capital error.⁷¹²

Table 18, p. 344 below, ranks the 34 states based on the degree of risk each faces from six conditions associated with higher rates of serious capital error. The risk posed by all but one factor, holding other factors constant at their averages, is based on the results of main Analysis 1A.⁷¹³ The comparative risk posed to each state is based on each state’s weighted average value on the relevant condition during the study period, with weights assigned based on the state’s yearly contribution to the pool of capital verdicts being studied.⁷¹⁴ In addition to each state’s rank and weighted average value on each factor, Table 18 indicates whether the capital error rate Analysis 1 predicts for the state based on the explanatory factor is above or below the predicted 34-state average error rate based on that factor (holding other factors constant at their averages), and *how far*—how many percentage

points—above or below the 34-state average each state’s predicted error rate falls.

In using Table 18, a strong caveat is in order. Because the data in each column are based on a single explanatory factor, holding other factors constant at their averages, and because our results indicate that capital error rates are a function of several significant factors, and also because of the statistical methods used to generate the information there, **Table 18 is most appropriately used to identify conditions in each state that pose a particularly large risk of serious capital error and might be an important target of reform efforts there. No single column in Table 18, nor the table as a whole, may appropriately be used to assign a particular overall predicted reversal rate to a given state.**

Column A in Table 18, p. 344 below, compares states’ predicted risk of capital error based on their capital-sentencing rates, holding other factors constant at their averages. **Based *only* on states’ death-sentencing rates**—and with the above caveat in mind—**Analysis 1A indicates that:**

- **The states with the highest weighted average number of death sentences per 1000 homicides—Idaho and Delaware—are at risk of capital error rates 23 percentage points higher than the 34-state average, and as much as 45 percentage points higher than the error rates predicted for the lowest death-sentencing states.**
- States in the next cohort in terms of their risk of serious capital error given their high death-sentencing rates are Utah, Wyoming, Nevada, Oregon, Oklahoma and Arizona—which are at risk of capital error rates from 10 to 18 percentage points above the predicted 34-state average rate.
- Three states prominently associated with the death penalty in the public mind because of their high numbers of *executions*—Texas, Virginia and Louisiana—face a lower risk of error based on this factor—suggesting that their relative success in carrying out the death verdicts they impose may be due in part to their comparatively low death-sentencing rates and thus their lower expected reversal rates. (“Success” in this regard is only relative, however, given that no state carried out even 30% of its verdicts during the study period, and the national average was 5%.⁷¹⁵)

Table 18: States' Rank, and Difference from Predicted 34-State Average Error Rate, Based on Six Explanatory Factors, Holding Other Factors at the 34-State Average*

State	A			B			C		
	Rank	Value	Difference from 34-State Avg. Error Rate	Rank	Value	Difference from 34-State Avg. Error Rate	Rank	Value	Difference from 34-State Avg. Error Rate
Connecticut	34	5.5	-21.6%	19	8.3	-6.9%	21	.170	-2.7%
Kentucky	22	16.9	-7.0%	23	7.1	-8.7%	5	.280	+1.2%
Maryland	27	14.2	-9.6%	6	23.7	+5.9%	20	.190	+2.2%
Tennessee	21	18.4	-5.7%	11	15.7	+0.7%	18	.230	-0.4%
Mississippi	15	27.0	+0.2%	1	35.2	+11.0%	6	.270	+0.9%
Oregon	6	53.2	+11.1%	30	1.6	-22.2%	23	.170	-2.9%
California	31	10.1	-14.3%	21	7.5	-8.2%	9	.250	+0.3%
New Jersey	26	14.2	-9.6%	14	12.9	-1.7%	24	.170	-3.0%
Idaho	2	113.8	+22.9%	33	.6	-28.2%	32	.070	-9.1%
Montana	9	43.5	+7.9%	34	.5	-29.5%	33	.001	-29.4%
Georgia	18	24.3	-1.4%	4	26.6	+7.3%	12	.250	+0.2%
Arizona	8	47.7	+9.4%	27	3.0	-17.3%	10	.250	+0.3%
Alabama	11	38.9	+6.0%	5	25.3	+6.7%	13	.240	-0.1%
Colorado	32	8.5	-16.4%	25	3.9	-14.9%	11	.250	+0.3%
Washington	33	5.8	-20.9%	28	3.0	-17.4%	22	.170	-2.7%
Wyoming	4	64.0	+14.1%	31	1.2	-23.9%	34	.001	-29.4%
Florida	12	32.7	+3.3%	13	13.7	-1.0%	17	.230	-0.3%
Oklahoma	7	49.7	+10.0%	22	7.2	-8.6%	8	.260	+0.7%
Indiana	23	16.6	-7.3%	20	7.7	-7.9%	29	.130	-5.1%
Arkansas	17	25.3	-0.8%	10	16.2	+1.1%	19	.210	-1.1%
North Carolina	16	26.9	+0.1%	7	22.0	+5.0%	7	.270	+0.8%
Nebraska	10	41.4	+7.1%	26	3.2	-16.8%	31	.080	-8.8%
Nevada	5	55.4	+11.8%	24	6.6	-9.5%	14	.230	-0.2%
South Carolina	13	28.0	+0.8%	3	29.8	+8.8%	2	.340	+2.8%
Utah	3	81.7	+17.9%	32	.7	-27.3%	28	.130	-5.1%
Louisiana	24	15.0	-7.9%	2	29.8	+8.8%	16	.230	-0.3%
Illinois	28	14.0	-9.8%	12	14.7	-0.2%	25	.150	-3.8%
Pennsylvania	14	27.5	+0.5%	18	9.1	-5.9%	27	.140	-4.6%
Texas	25	15.8	-8.0%	15	11.9	-2.7%	3	.330	+2.8%
Missouri	20	19.0	-5.3%	16	10.7	-4.0%	30	.120	-5.4%
Delaware	1	116.4	+23.2%	9	16.7	+1.4%	4	.280	+1.2%
New Mexico	30	12.0	-11.9%	29	2.1	-20.2%	1	.590	+7.7%
Ohio	19	23.9	-1.7%	17	10.5	-4.2%	26	.150	-4.0%
Virginia	29	13.4	-10.3%	8	18.8	+3.0%	15	.230	-0.2%

Table 18: States' Rank, and Difference from Predicted 34-State Average Error Rate, Based on Six Explanatory Factors, Holding Other Factors at the 34-State Average*

State	D			E			F		
	Rank	Value	Difference from 34-State Avg. Error Rate	Rank	Value	Difference from 34-State Avg. Error Rate	Rank	Value	Difference from 34-State Avg. Error Rate
Connecticut	31	6.3	-8.3%	32	3	-15.5%	34	3.1	-4.5%
Kentucky	22	5.0	-2.3%	15	7	+2.6%	8	1.4	+2.0%
Maryland	23	5.1	-2.8%	2	8	+8.0%	29	2.5	-2.8%
Tennessee	15	4.0	+4.2%	23	6	-2.5%	21	2.1	-1.2%
Mississippi	33	6.7	-9.5%	26	5	-7.3%	11	1.5	+1.5%
Oregon	7	3.1	+12.1%	2	8	+8.0%	22	2.1	-1.2%
California	13	3.7	+5.9%	26	5	-7.3%	24	2.2	-1.7%
New Jersey	14	3.9	+4.5%	23	6	-2.5%	30	2.6	-3.2%
Idaho	11	3.5	+7.5%	2	8	+8.0%	6	1.3	+3.1%
Montana	2	2.5	+18.8%	15	7	+2.6%	4	1.0	+4.9%
Georgia	21	5.0	-2.2%	2	8	+8.0%	3	0.9	+5.9%
Arizona	12	3.7	+6.6%	15	7	+2.6%	13	1.5	+1.3%
Alabama	30	6.1	-7.6%	2	8	+8.0%	16	1.6	+0.8%
Colorado	3	2.5	+18.6%	15	7	+2.6%	23	2.2	-1.5%
Washington	4	2.5	+18.1%	2	8	+8.0%	28	2.4	-2.5%
Wyoming	8	3.1	+11.3%	2	8	+8.0%	20	1.9	-0.5%
Florida	10	3.5	+7.9%	15	7	+2.6%	15	1.6	+1.0%
Oklahoma	24	5.3	-4.1%	2	8	+8.0%	18	1.8	+0.2%
Indiana	16	4.4	+1.3%	2	8	+8.0%	9	1.4	+1.9%
Arkansas	19	4.8	-1.0%	15	7	+2.6%	5	1.1	+4.5%
North Carolina	27	5.9	-6.4%	15	7	+2.6%	33	3.0	-4.4%
Nebraska	5	2.5	+17.7%	2	8	+8.0%	1	0.6	+9.7%
Nevada	28	5.9	-6.6%	2	8	+8.0%	31	2.7	-3.3%
South Carolina	32	6.7	-9.5%	30	4	-11.6%	14	1.5	+1.3%
Utah	1	1.5	+33.7%	2	8	+8.0%	2	0.9	+5.9%
Louisiana	25	5.4	-4.3%	30	4	-11.6%	17	1.7	+0.4%
Illinois	9	3.2	+10.9%	26	5	-7.3%	19	1.8	+0.0%
Pennsylvania	20	4.8	-1.1%	23	6	-1.5%	26	2.2	-1.8%
Texas	17	4.4	+0.9%	15	7	+2.6%	10	1.5	+1.6%
Missouri	18	4.6	+0.0%	26	5	-7.3%	7	1.4	+2.1%
Delaware	34	8.2	-14.0%	32	3	-15.5%	27	2.3	-1.9%
New Mexico	6	2.9	+14.1%	1	9	+13.5%	12	1.5	+1.4%
Ohio	26	5.8	-6.0%	2	8	+8.0%	25	2.2	-1.7%
Virginia	29	6.0	-7.2%	34	2	-18.9%	32	2.7	-3.3%

* Data on all explanatory factors in this table are based on [Analysis 1A](#) except for the data on per capita spending on state courts, which are based on [Analysis 3A](#).

2. High county-level capital-sentencing and high capital-error rates.

The above discussion focuses on *state* differences in capital-sentencing and capital-error rates. **Similar disparities exist at the county level. Most counties in most active capital states imposed *no* death verdicts in particular study years.**⁷¹⁶ Those localities may be contrasted with the six American cities that imposed over 100 death verdicts, and the nine additional cities that imposed between 50 and 100 verdicts, during the study period—listed in Table 19 below in order of death-sentencing rates, to show the wide variation even among high death-sentencing cities. Even here, however, the influence of states is felt. The top city in each cohort is in Arizona. Five of the top 15 death-sentencing localities measured in this way are located in Florida.

Table 19: Cities with More than 100, and with 50-100, Death Verdicts, 1973-1995, by Death Sentencing Rate per 1000 Homicides⁷¹⁷

City	# Death Verdicts	Rate/1000 Homicides
Phoenix (AZ)	114	41
Philadelphia (PA)	127	27
Houston (TX)	190	19
Miami (FL)	103	15
Chicago (IL)	138	11
Los Angeles (CA)	150	8
Tucson (AZ)	63	64
Las Vegas (NV)	71	55
St. Petersburg (FL)	51	50
Oklahoma City (OK)	68	50
Tampa (FL)	67	36
Jacksonville (FL)	66	30
Birmingham (AL)	55	25
Ft. Lauderdale (FL)	55	21
Dallas (TX)	61	11

Variation in county capital-sentencing rates is the rule, not the exception. Death-sentencing

rates for counties with five or more death verdicts during the study period⁷¹⁸ ranged from:

- 0 per 1000 homicides in, e.g., Denver (0 out of 1057 homicides) and Baltimore City (0 out of 2933 homicides);
- 3 per 1000 in St. Louis City, Shreveport, and Dayton;
- 4 per 1000 in Newark (NJ) and Atlanta; and
- 5 per 1000 in San Francisco and Richmond (VA);

to:

- 40 to 49 per 1000 in Phoenix, Cincinnati, Montgomery (AL), Columbus (MS), DuPage County (IL), and four Florida counties;⁷¹⁹
- 50 to 59 per 1000 in Oklahoma City, Las Vegas, Reno, suburban Baltimore County and eight Florida counties;⁷²⁰
- 60 to 75 per 1000 in Tucson, two other Arizona counties, and five additional Florida counties;⁷²¹
- 90 to 200 per 1000 in Kent County (DE), Lexington County (part of Columbia, SC), Randall County (part of Amarillo, TX), Coos Bay (OR), Carson City (NV), six Georgia counties, five Alabama counties, one additional Arizona county and four additional Florida counties;⁷²² and
- 267 per 1000 homicides in Missouri's capital, Jefferson City.⁷²³

Included on this list of high capital-sentencing counties are Nevada's three most populous counties with nearly 90% of the state's population, five of Arizona's six most populous counties with 85% of its population, and 21 of Florida's 67 counties with over a quarter of its population.⁷²⁴

As recent commentaries have highlighted, these and other death-sentencing disparities from one locality to the next often occur within the same state.⁷²⁵ Examples are in Table 20 below (sources: DRCen, Vital Statistics).

**Table 20: Examples of High and Low Death-Sentencing Counties in the Same State
(Death Verdict per 1000 Homicides Indicated in Parentheses)**

	<u>Relatively High Death-Sentencing City/County</u>	vs.	<u>Relatively Low Death-Sentencing City/County</u>
California:	Redding/Shasta (62) Modesto/Stanslaus (35) Bakersfield/Kern (23)		San Francisco (5) Los Angeles (8) Richmond/Contra Costa (9)
Florida	Pensacola/Escambia (55) St. Petersburg/Pinellas (50) Tampa/Hillsborough (36)		Palm Beach (12) Miami/Dade (15) Gainesville/Alachua (15)
Georgia	Atlanta suburbs/Gwinnett (47) Atlanta suburbs/Cobb (36) Columbus/Muscogee (33)		Atlanta/Fulton (4) Augusta/Richmond (10) Macon/Bibb (13)
Maryland:	Baltimore suburbs/Baltimore County (56)		Baltimore City (0) Washington suburbs/Prince George's (6)
Missouri:	Jefferson City/Cole (267) St. Louis suburbs/Jefferson (46) St. Louis suburbs/St. Louis County (26)		St. Louis City (3) Kansas City (Jackson) (6)
Oklahoma:	Muskogee (52) Oklahoma City (50)		Tulsa (16)
Ohio	Akron/Summit (54) Cincinnati/Hamilton (40)		Dayton/Montgomery (3) Columbus/ Franklin (16)
Oregon	Coos Bay (94)		Portland/Multnomah (13)
Pennsylvania:	Scranton/Lackawanna (76) Philadelphia suburbs/Bucks (33) Philadelphia (27)		Pittsburgh/Allegheny (12) Philadelphia suburbs/Delaware (12)
So. Carolina:	Columbia (pt.)/Lexington (93) Charleston (23)		Columbia (pt.)/Richland (9) Greenville (11)
Tennessee:	Johnson City (pt.)/Washington (88) Chattanooga/Hamilton (28)		Nashville/Davidson (6)
Texas:	Lubbock (20) Corpus Christi/Nueces (20) Houston/Harris (19)		Austin/Travis (10) Dallas (11) Galveston (11)
Virginia:	Danville City (53)		Richmond (5)

As is true of state-level death-sentencing disparities, these **county-level disparities are associated with county-level capital error rates**. Our county case studies above⁷²⁶ and several of our regression analyses (Analyses 7-10 and 18)⁷²⁷ indicate that **the more death verdicts per homicides a county imposes, the higher its capital-error rates are likely to rise. This county factor operates independently of, and in addition to, the effect of state death-sentencing rates.**

3. Low or modest aggravation and a high case-level probability of reversal.

Analyses 1-5 and 7-18 and the county case studies thus lead to the conclusion that **excessive use of the death penalty is associated with high rates of capital error**. A final study, Analysis 19 of case-level federal habeas outcomes, helps answer a question this conclusion poses: Excessive by what measure? Given that the probability of error, reversals and retrials is decreased by less frequent, more judicious capital-sentencing, how should policy makers and officials go about narrowing the category of potentially capital cases?

Analysis 19 finds that **the cases that present the greatest risk of federal habeas reversal, and thus that policy makers and officials would be best advised to exclude from death-eligibility, are those in which the degree of aggravation, offset by mitigation, is not high.**⁷²⁸ **As the case for death gets weaker—i.e., as aggravation net of mitigation or the quality of the evidence decreases—the probability of reversal due to serious error rises.** Holding other factors at their average, Analysis 19 predicts that **the probability of federal habeas reversal due to serious capital error decreases by 15% or more for each additional statutory or supplemental aggravating circumstance in the case, and increases by 15% for each additional mitigating factor in the case.**⁷²⁹ As indicated by the decisions of federal habeas judges—and, on this common sense point, there is no reason to expect judges at other stages to evaluate serious capital error

differently—*uses of the death penalty are excessive, creating a high risk of serious capital error, when they extend the penalty to cases that are not very highly aggravated.*

Our principal conclusion thus strongly supports the statements of District Attorney Marquis and Governor Gilmore quoted above: **Jurisdictions that reserve the death penalty for only the very worst offenses do the best job of avoiding serious, capital error and the risks and costs that go with it. By contrast, states and counties that use the death penalty aggressively (*i.e.* relatively more often per every 10, 100 or 1000 homicides) and extend it to homicide offenses that are not extremely aggravated, are likely to have the worst records of serious, capital error.**

Our analyses also indicate that the harmful effect of a propensity to overuse the death penalty in cases that are not highly aggravated occurs at the level where capital-sentencing policy is made, not where policy is applied. Federal habeas reversals are most common in close or marginal cases judged by the amount of aggravation net of mitigation—*i.e.*, in non-highly aggravated cases that get swept into the capital net by broad death-sentencing *policies*—rather than in especially egregious cases where *case-level* pressures to sentence capitally might be highest.⁷³⁰ This suggests that **it is state or local policies setting a low threshold of seriousness or aggravation for the kinds of crimes that trigger capital prosecutions and verdicts, and not pressures to use the death penalty in particular cases, that are most associated with high rates and amounts of error.**

C. Supporting Conclusions

1. **High capital-error rates are associated with four conditions that create pressure to use the death penalty in weakly aggravated cases where the risk of error is great—high crime rates, low punishment rates, race and politics.**

For many policy purposes it is enough to conclude based on reliable and consistent study

findings that **heavy use of the death penalty is associated with high capital-error rates**. But our regression analyses reveal **four additional factors associated with high rates of serious capital error whose common attributes suggest something more about the forces leading to heavy capital sentencing and a high risk of error**. High capital error rates are significantly related to:

- **well-founded doubts about the ability of state law enforcement policy and officials to deal effectively with crime;**
- **state judges' susceptibility to being harmed politically, given how they are selected and promoted, if their rulings do not conform to popular sentiment;**
- **the homicide risk to whites, particularly when that risk approaches or exceeds the high risk of homicide that African-Americans typically face; and**
- **the size of the state's black community relative to its overall population (and to a lesser extent the proportion of its population receiving welfare).**

As we develop below, each of these factors is a potential indicator of the threat of crime felt by politically influential members of the community, or of the pressure on capital policy makers and officials to respond forcefully to that threat. We conclude that **each factor is an indicator of the pressure felt by capital jurisdictions and officials to respond to influential citizens' fear of serious crime by extending the death penalty to cases where its use is not warranted by the especially aggravated nature of the offense and instead invites serious error**. After discussing each factor, we address attributes they share that invite the extension of the death penalty to weakly aggravated cases where the need to commit error to secure a death verdict is high.

- a. **Well-founded doubts about the ability of state law enforcement policy and officials to deal effectively with crime.**

Main Analysis 1 and nearly all other analyses find that **states which arrest, convict and punish fewer serious criminals (as indicated by the number of incarcerated criminals per 100 FBI Index Crimes) have significantly higher capital-error rates.**⁷³¹ This relationship is highly significant, and the size of its predicted effect on capital reversal rates is large. **Typically, predicted capital reversal rates (holding other factors constant) increase 5- to 7-fold as rates of apprehending, convicting and imprisoning serious criminals fall from their highest to their lowest levels among states in our study.**⁷³² In the same way as poorly funded and overburdened *court* systems generate more serious capital error (as we discuss below⁷³³), **ineffective state law enforcement systems—those with the worst records of arresting, convicting and incarcerating serious criminals—are the most likely to conduct seriously flawed investigations, prosecutions and trials of capital charged defendants.**

When considered with our principal finding, this result supports a further conclusion. The less effective law enforcement is at capturing, prosecuting and punishing criminals, the more pressure is likely to be placed on officials to do more to fight crime. This is especially the case when the crime that people and neighborhoods fear is homicide, and when those in fear have the political influence to translate their concerns into public action. One response such political pressure invites is expanded use of the death penalty as a visible demonstration of officials' intolerance for crime and their commitment to punishing it severely. Because expanding the death penalty costs little at first—although eventually it triggers lengthy appeals that often end in costly reversals and retrials—and because that response is available to any jurisdiction, no matter how poor its crime-

fighting capacity may be, expanding the death penalty is an especially attractive response by states with the worst crime-fighting records. Where pressures generated by well-founded doubts about the effectiveness of state law enforcement systems trigger expanded death sentencing, our principal finding predicts that higher capital error rates will result as officials cast the capital net more widely, pulling in more cases where the evidence of a highly aggravated crime is weak.⁷³⁴ **Lower crime-fighting competence thus is associated both with heightened pressures to expand the death penalty in response to ineffectively controlled crime, and with lower competence in investigating and prosecuting those progressively weaker capital cases. The mutually reinforcing effect is the one our study documents: Higher rates and amounts of serious capital error.**⁷³⁵

Column D in Table 18, p. 345 above, compares states based on their rates of arresting, convicting and incarcerating criminals per 100 FBI Index Crimes, and based on whether and by how much the capital reversal rates this factor predicts for each state diverge from the average reversal rate predicted for all 34 states. States with the lowest law enforcement scores and the highest risk of error considering only this factor are Utah, Montana and several other western states. Nebraska, Illinois and Florida round out the top 10 states with the highest predicted capital error rates based on this factor alone. According to our best analysis, **states in this low-law-enforcement category risk capital error rates anywhere from 8 to 34 percentage points higher than the 34-state average—and 22 to 48 percentage points higher than the state with the best record in this one regard.**

Comparing state risk rankings based on this factor to rankings based on high death-sentencing emphasizes the caveat given above.⁷³⁶ Although Colorado and Washington are at the high

end of the spectrum of risk based on this low-law-enforcement factor, they are at the low end of the risk spectrum when it comes to their death-sentencing rates. The opposite is true of Delaware, Alabama, and Nevada, which have a relatively low risk of capital error judged by their law enforcement record, but a high risk of error based on their death-sentencing rates. Because our analyses reveal that all these factors are important, it is inappropriate to base an assessment of a state's overall proneness to capital error on state comparisons that are attentive to only one factor. What these figures instead identify are different high-risk factors for each state, which could become a focus of local reforms. **Given that nearly all states have disturbingly high (50%-plus) overall capital error rates,⁷³⁷ all have room for improvement, whether or not they do comparatively well on one or another measure.**

b. State judges' susceptibility to being harmed politically if their rulings do not conform to popular sentiments.

Another study finding identifies a political mechanism through which public fears about crime, and doubts about the effectiveness of a state's response to it, can pressure officials into adopting policies that increase capital error. This result is found in main [Analysis 1](#), and in confirming analyses of all three review stages combined, and the state direct appeal and federal habeas stages by themselves.⁷³⁸ **States, and counties in states, with judicial selection methods that make judges more vulnerable to political discipline if their rulings are not consistent with popular sentiment have higher capital-error rates.⁷³⁹ In other words, courts in states that directly elect judges from the outset—or subject judges to more frequent, more often contested and more partisan elections—more often produce seriously flawed capital verdicts than courts**

whose judges are insulated from direct political influence from voters and contributors.

This finding is important. **It reveals a way in which politically influential members of the public who are threatened by serious crime and doubt the effectiveness of their state's response to it can pressure policy makers to demonstrate their resolve to respond the problem aggressively—including by extending the death penalty to more cases where the risk of error is greater.** Judges, however, are not the only actors whose decisions affect the breadth of the state's death penalty. Governors, legislators, attorneys general and district attorneys also have an important impact on death-sentencing policy.⁷⁴⁰ Unfortunately, the effect of political pressures on those officials is harder to demonstrate statistically, because doing so requires measurable variation among states in the kinds of political pressure their officials feel, and there is little variation from state to state in how and how often they select governors, legislators, attorneys general and district attorneys.⁷⁴¹ Thus, the sizeable effect of judicial selection techniques on capital error rates—**2- to 6-fold increases in predicted error rates as selection methods change from placing the least to the most political pressure on state judges** (other factors held constant)⁷⁴²— probably underestimates the effect of all types of political pressures on all capital officials.

Column E in Table 18, p. 345 above, compares states based on the amount of political pressure their judicial selection techniques put on state judges, and based on the difference between the reversal rate for each state that is predicted by this factor alone, and the average rate it predicts for all 34 states. Because there are only nine possible scores on the political pressure index—only eight of which actually apply to any of the 34 study states—a number of states are in a tie for most rankings.⁷⁴³ Only two states are tied with no other: Virginia, with the lowest rank on this risk factor, given that its judges are appointed,⁷⁴⁴ and New Mexico, with the highest rank. **This top ranking in**

terms of the pressure on judges to conform their rulings to public sentiment puts New Mexico at risk of capital error rates 14 percentage points higher than the 34-state average. The 13 states with judicial selection techniques that place the next highest level of political pressures on their judges—including, for example, Alabama, Georgia, Oklahoma and Ohio—are at risk of capital error rates 8 percentage points above the 34-state average, based on this factor. **On the other hand, judicial selection techniques that immunize state judges entirely from regular or potential elections by the public at large are associated with predicted capital reversal rates nearly 20 percentage below the 34-state average, and over 30 percentage points below the predicted reversal rates of states that put judges under the most pressure to conform their rulings to popular sentiment.**

c. A high risk of homicide to politically influential citizens.

By taking each state's homicide rate among whites and dividing it by the state's homicide rate among blacks, it is possible to determine whether—and how closely—the homicide risk to whites in each state approaches the typically high homicide rates that afflict African-Americans communities in this nation. Put another way, this factor compares states based on whether homicides there mainly threaten blacks, or whether the homicide risk also falls fairly heavily on whites.⁷⁴⁵

In main Analysis 1, and in most other analyses, **the greater the share of the homicide risk that is borne by whites relative to blacks, the higher the state's rate of serious capital error.**⁷⁴⁶ **Effect size is moderate. Holding other factors at their averages, predicted reversal rates double or triple across the spectrum of conditions among states and years in our study.**⁷⁴⁷ Likewise, Column C of Table 18, p. 344 above, shows that in our best analysis this factor predicts capital reversal rates for New Mexico that are 17 percentage points higher than the predicted reversal rate

for Nebraska, given that in New Mexico the risk that a white person will be killed by homicide comes the closest to equaling the risk that a black person will be killed by homicide (the white risk is 60% of the black risk), while in Nebraska the homicide risk faced by whites is only 8% as high as the risk faced by blacks.⁷⁴⁸ At p. 365 below, we explain why the share of the homicide risk borne by whites as opposed to blacks may have an even bigger predicted impact on reversal rates, when the interaction of that factor and the racial makeup of the general population is considered.

In a minority of analyses, high homicide rates by themselves are significantly associated with high error rates, over and above the effect of a high homicide risk to whites relative to blacks.⁷⁴⁹ In some other analyses, homicide rates by themselves were significantly associated with error rates *until* the white-compared-to-black homicide rate was introduced, at which point the white/black homicide rate was significant (and fit and other diagnostic measures improved), and homicide rates by themselves became non-significant. Similarly, in nearly all analyses, the homicide rate exclusively among whites was not as powerful a predictor of error rates as the homicide threat to whites compared to blacks.⁷⁵⁰ This reveals that, although high homicide rates by themselves predict high capital error rates, a better predictor of high error rates is the *distribution* of the risk of homicide between whites and blacks—more specifically, whether the homicide risk to whites approaches or surpasses that to blacks (in which case capital error rates are higher), or on the other hand, whether blacks bear the brunt of the homicide risk (in which case capital error rates are lower).

We included this factor based on strong evidence in a number of studies, and the recent conclusions of two highly regarded legal scholars representing a wide spectrum of political views, that law enforcement officials are more responsive to the threat of crime to white as opposed to black communities.⁷⁵¹ These observers offer two explanations for their findings. The first is that law

enforcement officials and policy makers pay more attention to the law enforcement needs of affluent and politically influential people and communities, and less attention to people and communities with fewer resources and political influence, because the latter groups are less organized, have fewer resources and less time to devote to the civic and political mobilization needed to secure the attention of law enforcement officials or to fund contributions to political campaigns, and have lower social status. In this view, African-American communities are one of a number of communities that tend on average to be less organized and wealthy and to have lower status with officials, and thus are less well served by law enforcement policy and officials. Because reliable data are kept on the race of crime victims, but not always on other indicators of low political influence, it is easier to detect and measure under-enforcement of the criminal laws in the black than in other, similar communities.

The other explanation is that race discrimination leads officials to pay less attention to the threat of crime to blacks as opposed to whites, explaining why the race of victims strongly predicts how well they are served by law enforcement policies and officials. There is substance to both explanations. For our purposes it is unnecessary to choose between them.

A central finding of these prior studies is that, after controlling for degree of aggravation and other variables, death verdicts are substantially more likely for homicides against white victims than for those against black victims.⁷⁵² This finding predicts that jurisdictions with a relatively large homicide risk to whites, or to members of other influential communities that tend to get more law enforcement attention, are likely to have higher per-homicide rates of capital *prosecution* and *sentencing*. But why would states with a relatively high homicide risk to whites have significantly higher rates of serious *error* in those verdicts?

Our study's principal finding suggests an answer to this question: Jurisdictions that use the

death penalty more often per homicide have higher capital error rates. The strong association between high error rates and greater use of the death penalty predicts that conditions prompting aggressive use of the death penalty may also be associated with high error rates.⁷⁵³ This, then, helps explain why states in which a relatively heavy share of the homicide risk is borne by whites as well as blacks have higher capital error rates. The greater the share of the homicide threat borne by whites or other politically influential communities, the more pressure officials may feel to broaden the death penalty to demonstrate a resolve to deal forcefully with homicides. Resolve is just as vividly demonstrated when the death penalty is used for weakly aggravated homicides as when it is limited to highly aggravated cases—indeed, it may be *more* vividly demonstrated when aggravation is weak. And in any event, in any given jurisdiction, there are likely to be many more medium-range than extremely aggravated cases through which to demonstrate a determination to fight crime. **Expanded capital sentencing in response to crime fears thus invites capital verdicts in weakly aggravated cases where the probability of serious error is the greatest.**

A homicide risk that is not borne almost entirely by blacks, and also falls fairly heavily on whites, thus appears to pressure officials to set a low threshold on when the death penalty can be imposed. Low capital thresholds in turn prompt high capital error rates, by inviting prosecutions where the offense is not “the worst of the worst”—where the evidence of an offense warranting the death penalty is weak enough that corner-cutting and other errors may be needed to assure a death verdict.

There is one sense in which our study qualifies the conventional wisdom about the link between race and the death penalty. The conventional understanding might suggest that, given the link some studies have found between the race of the victim of a particular murder and an increased

probability of a death sentence, our finding of a link between higher death-sentencing rates and higher error rates would lead to higher error rates in death verdicts imposed for homicides against white victims. As we show above, however, capital error occurs just as often in black-victim as in white-victim cases.⁷⁵⁴ This is part of a pattern of results indicating that high capital-error rates are mainly associated with broad capital-sentencing policies, not individual decisions in particular (*e.g.*, white-victim, or especially aggravated) cases.⁷⁵⁵ **Once factors like high concentrations of homicides in politically influential communities lead to aggressive capital laws and policies, those policies—and associated increases in capital error—evidently affect defendants of all races equally. The people most adversely affected by broad capital-sentencing policies and resulting error thus are defendants of *all races* who happen to be tried in jurisdictions with high death-sentencing rates, and particularly defendants of all races as to whom the evidence of an offense warranting the death penalty is the *weakest*.**⁷⁵⁶

The results discussed here and in the previous section have a further implication. As a matter of principle, law enforcement officials must do everything the law permits to lessen the threat of homicide to all residents of the jurisdiction. Our regression results reveal that **expanded use of the death penalty against an ever-widening set of homicides is *not* an effective strategy because it increases the likelihood of mistake, including that innocent people are caught in the net and perpetrators go free. Nor is it a strategy the law permits, because it multiplies reversible capital error. Nor, finally, is it a strategy designed to protect all communities because it is more responsive to concentrations of homicide in the white community.** The results in the previous section reveal **an alternative strategy for lowering the homicide threat that *is* an effective response to crime, is permitted by law, and protects all communities. Rather than applying the**

death penalty to an ever-expanding set of arrested suspects for whom the evidence of an offense aggravated enough to warrant the death penalty is fairly weak, the better strategy is to leave the death penalty focused on “the worst of the worst” and to divert the resources saved by a more judicious use of the death penalty to apprehending, convicting and incarcerating a wider array of perpetrators of a broader set of serious crimes.

d. Large numbers of African-Americans and welfare recipients.

In main Analysis 1, and nearly all supporting analyses, **the larger the proportion of a state’s population that is African-American, the larger the state’s rate of serious capital error.**⁷⁵⁷ **At the federal habeas stage, the same thing is true of the proportion of the state’s population receiving welfare and its per capita cost.**⁷⁵⁸ Effect size is considerable. In our main analysis, predicted capital error rates more than quadruple as the size of the black population rises from its lowest to its highest levels among states in our study, holding other factors constant.⁷⁵⁹ Likewise, in our federal habeas regression, predicted reversal rates more than quadruple as welfare recipients and costs rise from their lowest to highest levels among study states and year.⁷⁶⁰

We explain above why there is no clear link between the proportion of blacks in the state population and the number of black state policy makers, judges, prosecutors, jurors and the like, and why those conditions are unlikely to explain high capital error rates.⁷⁶¹ Instead, given that the explanatory condition is the racial makeup of the state’s overall population, not that of participants at particular trials or even of the county where the crime and trial took place,⁷⁶² and given extensive research documenting powerful, inaccurate stereotypes linking contact with African-Americans to a perceived threat of violent crime,⁷⁶³ we conclude that the size of a state’s African-American population is a strong indicator of the intensity of crime fears among politically influential citizens.

Like the race of homicide victims discussed just above, **this racial factor is a powerful indicator of the pressure officials face to respond forcefully to crime. This explains why the factor strongly predicts high capital-error rates, which are strongly associated with the broad and indiscriminate use of the death penalty that can occur when officials face pressure to expand the penalty as a forceful demonstration of their resolve to fight crime.**

As we note above, **the problem is not with officials who are determined to fight crime.**⁷⁶⁴ **The problem is with expanded and indiscriminate use of the death penalty, which is not an effective solution to the problem. When that response is adopted, the result is not more successful law enforcement, but instead a greatly increased risk of serious capital mistake, reversal and costly retrials. At the extreme—as has demonstrably occurred on just short of 100 occasions in the modern death-sentencing era—it means convicting the innocent, while actual killers remain at large.**⁷⁶⁵

We reach this conclusion sadly, given what it suggests about race relations. But we reach it with confidence. To begin with, the conclusion follows from those above. Higher death-sentencing rates are associated with higher capital error rates—with the biggest risk factor being the indiscriminate extension of the penalty to cases where aggravation levels are not extremely elevated. And high error rates are linked to two indicators of crime fears among politically influential individuals that can pressure officials to extend the death penalty to weakly aggravated cases as a way of demonstrating a firm resolve to fight crime: (1) low rates of apprehension, conviction and incarceration of serious criminals, and (2) a high risk of homicide borne by whites as well as blacks. It is an unfortunate but demonstrated fact that the race of people in the community is yet another, powerful indicator of crime fears, given the association people report and display between the race

of people they encounter and a perceived threat of violent crime.⁷⁶⁶ This association is partly based on actual crime and homicide rates, which are higher among African-American and poor communities than among others.⁷⁶⁷ But as the literature demonstrates, the association is also due to stereotypes that lead people to greatly overestimate the threat of cross-racial violent crime.⁷⁶⁸ (In fact, most crime occurs among members of the same race, community and class.⁷⁶⁹) Our analyses provide important new evidence of this effect. When examined separately, higher homicide rates indeed have the same relationship to higher reversal rates as our two racial measures of the actual and perceived threat of homicide. But when all these factors are examined together, it is the racial measures and not homicide rates themselves that are significantly and powerfully related to serious capital error. The condition related to pressure to use the death penalty that most strongly predicts high capital error rates thus is not the actual threat of homicide (the homicide rate), but instead the perceived as well as actual threat of homicide to whites and other influential residents from African-Americans and poor people.⁷⁷⁰ Given the linkage between the size of the black (and the poor) population and the perceived threat of crime, and given our consistent finding that indicators of crime fears predict high rates of capital error, it is reasonable to explain the strong association between capital error rates and the size of the black (and poor) population as another instance of the effect on capital error rates of the real and perceived threat of crime.

Second, our analyses of factors that increase the risk of capital error reveal that the size of the black population is significantly connected to two other recognized indicators of the intensity of fears of crime, particularly among politically influential citizens. We already have noted the relationship between homicide rates and the relative size of the black population as potential explanations for reversal rates. Those two factors are correlated, given the relatively higher rate of

homicide committed by blacks than by whites. And tested separately, both factors are significantly associated with capital-error rates. But when tested together, the size of the black population remains a powerful predictor of capital error rates, while the homicide rate is no longer significant.⁷⁷¹ From this we conclude that it is not so much the actual rates of homicide as a perceived threat of homicides by blacks that is associated with higher capital-error rates.

High African-American populations also interact with another established indicator of crime fears among politically influential citizens—the distribution of homicide risk between whites and blacks. In main Analysis 1, and in several other analyses of state and county error rates, **states with a combination of homicide risks concentrated relatively heavily on whites compared to blacks and large black populations relative to the total population had significantly higher capital error rates than either of the two factors by itself or the two together would predict.**⁷⁷² This indicates that the two factors have a similar effect on reversal rates that is magnified when both are present. Given a strong consensus about the pressure the threat of crime to the white community puts on law enforcement officials to respond forcefully to crime,⁷⁷³ and given the interaction of that factor and the relative size of the black population, it is reasonable to understand all three effects (each factor by itself and the two together) as indicators of crime fears that put pressure on officials to broaden the availability of the death penalty, and in the process increase capital error rates.

Column B in Table 18, p. 344 above, ranks states based on their weighted average proportion during the study period of residents who were African-American. Column B then compares states based on the difference between the reversal rate predicted for each state, and the 34-state average predicted reversal rate, based on this factor alone, holding other factors at their average. As Column B shows, **main Analysis 1A predicts that states with large black populations such as Mississippi**

and South Carolina are at risk of capital error rates over 10 percentage points higher than the average predicted reversal rate, and as much as 40 percentage points higher than predicted reversal rates in states with low African-American populations.

As we just pointed out, in main Analysis 1A and in a majority of others, the explanation for high reversal rates based on the racial makeup of the general population, and the separate explanation based on the racial makeup of homicide victims, interact: **States where blacks make up a higher proportion of the population and where the homicide risk to whites comes the closest to equaling the (typically higher) homicide risk to blacks have an especially high risk of serious capital error.** Predicted reversal rates cannot reliably be calculated for interaction effects of this sort, but the states may be ranked based on their comparative risk from this factor, as is done in the accompanying note. States that are most at risk from this factor, holding others constant at their average, are South Carolina, Mississippi, Louisiana, Georgia, Alabama, North Carolina, Delaware, Maryland, Virginia and Texas. Appropriately assessing the risk from each of the two racial factors requires that the risk from the interaction of the two also be considered.⁷⁷⁴

* * * * *

States for which these racial factors create an especially high risk of serious capital error cannot very well change their demographic profile, and thus may wonder how they can reduce the risk of error. As we develop above, however, it is not the demographic realities, but the pressures they create to apply the death penalty broadly, including in cases that are not highly aggravated, that appear to be linked to a high risk of serious capital error. And as we develop in Part VIII below, therefore, there are ways in which capital-sentencing policy may be changed to decrease the incentive and capacity to impose the death penalty in cases that are not highly aggravated where the risk of

error is great. The main point for now is to catalogue the risk factors for each state as a prelude to the policy discussion below.

- e. **Summary: conditions with a common capacity to pressure policy makers to extend the death penalty to cases that are not highly aggravated, where the risk of error is great.**

The four factors discussed here have **two common attributes which explain why they invite policies that extend the death penalty to cases that are not highly aggravated, where the risk of error is high.** First, the fears and pressures the four factors create seem to operate at the level at which state and county capital-sentencing policy is made—*i.e.*, where the threshold level of aggravation sufficient to trigger a capital prosecution and sentence is set for all cases—rather than at the level where policy is applied to particular cases.⁷⁷⁵ **The higher the level of government at which policy is set, and the broader and more divorced the decision is from particular cases, the less likely it is that the policy will be sensitive to the nuances of aggravating and mitigating circumstances in individual cases, and the greater the chance that the policy will encompass less aggravated cases.**

Second, all four conditions reflect either generalized fears about serious crime, or the capital system's vulnerability to pressures generated by such fears. Some of the fears and pressures are empirically well-founded—those based on high homicide rates and low rates of apprehending and punishing criminals. Others are less justifiable, or even illegitimate—the influence of political considerations on judicial outcomes, and the role of race in gauging the threat of crime. What is crucial, however, is that all four factors prompt fears and pressures that are far removed from the facts and circumstances of each case and invite responses—including broadened use of the death penalty—that demonstrate officials' intolerance for crime in general, and not just for offenses where

close inspection of the circumstances and evidence reveal high levels of aggravation. **Particularly in states with poor crime-fighting records, a desire to demonstrate a determination to fight crime is no less well-served—and may even be better served—by a threshold level of evidence and aggravation for the death penalty that sweeps in marginal cases where the evidence is weak and where as a result the risk of error is large.**

Our findings indicate that **it is not every additional use of the death penalty, but only those uses where the crime is not “the worst of the worst,” that especially enhance the risk of serious capital error. The four factors discussed here encourage this indiscriminate use of the penalty.** So may other conditions that are harder to measure, such as political pressure on district attorneys.⁷⁷⁶ The relationship between high death-sentencing rates and high capital-error rates thus serves as a residual explanation for capital error rates, which captures the effect of pressures to use the death penalty broadly that, unlike the four pressures discussed here, cannot be measured more directly.⁷⁷⁷

2. Aggressive use of the death penalty is also linked to heavy court congestion and delay.

Main Analysis 1 and most supporting analyses find **a significant relationship between high numbers of capital verdicts awaiting appeal and low rates of progress in moving capital verdicts through the system either to approval and execution, or reversal.**⁷⁷⁸ Effect size is large. Analysis 1 predicts that **the process of moving capital verdicts from trial to a decisive result on appeal essentially comes to a halt in states with 20 or more capital verdicts awaiting review at one time.**⁷⁷⁹

This finding is predictable: **Capital verdicts caught in the review process cannot serve the**

purpose for which they were imposed—and those that are flawed cannot be corrected. The findings have added significance in conjunction with our principal finding that higher death-sentencing rates lead to higher rates of serious capital error. Higher rates of death verdicts also mean *more* death verdicts, each of which makes an inordinate contribution to court congestion, and even a fairly small number of which can effectively clog and close down the system.⁷⁸⁰ States with fewer death verdicts not only limit the risk that any verdict will be found seriously flawed, but also increase the probability that verdicts that are not flawed will get through the review process quickly.

The table in note 788 below compares states based on their weighted average number of death verdicts awaiting review at one of the three review stages during the study period.⁷⁸¹ **States vary substantially in this regard, from California with an average of about 27 capital verdicts awaiting review each year, and Texas, Florida, Pennsylvania and Ohio with average capital backlogs of 12 to 18, to Nebraska, Montana, Washington, Connecticut and Wyoming, with fewer than 1 backlogged capital case on average.**

Consideration of this factor reveals a hidden cost of the current capital system. Delayed appeals limit the amount of completed review, generating lower numbers of reversals.⁷⁸² Delayed appeals also lead to lower *rates* of reversal. First, when reversal rates are calculated as proportions of all imposed verdicts, lower rates of review automatically mean lower reversal rates—even if verdicts remain equally flawed—because there are fewer outcomes of *any* sort.⁷⁸³ Although that rate is not the true error rate, which is the number of reversals as a proportion of *reviewed*, not imposed, verdicts,⁷⁸⁴ members of the public sometimes mistakenly think that fewer reversals per imposed verdicts means fewer errors.⁷⁸⁵ Second, reversals take a year or two longer than affirmances to occur at the federal habeas stage, artificially increasing the number of affirmances and decreasing the

number of reversals that have occurred as of any moment, which in turn artificially decreases the error rate.⁷⁸⁶ Third, our regression results suggest that large backlogs of delayed appeals sometimes pressure appellate courts into approving verdicts that otherwise would be found seriously flawed, further lowering reversal and error rates.⁷⁸⁷ **This means that states like California, Texas, Florida, Pennsylvania and Ohio, have fewer reversals and lower reversal rates (as a proportion of imposed verdicts)—and appear to have lower error rates (as a proportion of reviewed verdicts) than otherwise would be true—because capital verdicts move so slowly through their appeals process. From the perspective of these states’ reversal records, their inefficiency becomes a *saving grace* because it lowers their numbers and rates of reversals. But from the perspective of victims and communities seeking finality, taxpayers financing costly appeals, and wrongly convicted and sentenced defendants needing redress, that inefficiency is costly.**

The reverse holds for states like Nebraska, Montana, Washington and Connecticut. They are *penalized* for having efficient review systems: Although their reversal records accurately reflect the amount of error in their capital verdicts, those records are comparatively worse than the records of states like California, Texas and Florida, where delay artificially deflates reversals. Based on this factor alone, holding other factors at their averages, our regression analyses predict very high reversal rates for Nebraska, Montana, Washington, and Connecticut. But that prediction is based entirely on these states’ admirably *low* backlogs of pending appeals, which keep them from taking advantage of delayed appeals to obscure their true error rates.⁷⁸⁸

3. Overburdened and underfunded courts are associated with a high risk of capital error.

In main Analysis 1, and in most other analyses of capital error found at the three review stages combined, **a combination of high numbers of capital verdicts awaiting review and high per capita rates of court cases of all types awaiting decision is significantly related to high capital error rates.**⁷⁸⁹ In analyses of the initial, direct appeal review stage—where nearly 80% of capital reversals occur—**low per capita funding on the courts is also related to high capital error rates.**⁷⁹⁰ For states with below average funding for their courts, effect size is large: **Relatively small decreases in direct funding below the 34-state average of about \$1.80 per capita are associated with steep predicted increases in the amount of serious capital error state high courts discover on direct appeal, holding other factors constant.**⁷⁹¹

These findings indicate that **state court systems with below average operating budgets—or what may be the same thing, with too many capital and non-capital cases to process reliably with available resources—tend to produce more flawed capital verdicts. High proportions of flawed verdicts and the high reversal rates associated with them lead, in turn, to high retrial rates— further burdening the courts, and generating more error, more work for appellate courts, and more reversals and retrials.**

Results of particular cases reveal the same thing. **At the two phases of review where data are available, the largest single reason why courts reverse capital verdicts is egregiously incompetent representation of capital defendants by mainly state-funded lawyers—prompting close to 40% of all state post-conviction reversals, and close to 30% of all federal habeas reversals.**⁷⁹² **The main reason inexperienced, unskilled and untrained lawyers are often the**

only ones who seek capital trial assignments—the most demanding assignments lawyers can receive—and the main reason the performance of even conscientious appointed capital lawyers is often below par, is the low level of compensation and reimbursement for expenses (investigators, mental health exams, DNA testing and the like) that is available in most states.⁷⁹³

Because funds for capital trial lawyers and for necessary support services often come out of state court operating budgets, it is not surprising that our aggregate-level analyses reveal a link between financially strapped state courts and high rates of capital error.

Case-level Analysis 19 of federal habeas outcomes also reveals a link between poor quality state court proceedings and high capital reversal rates. **State court denials of evidentiary hearings on review of claimed capital errors are associated with a higher probability that federal habeas courts will reverse capital verdicts.**⁷⁹⁴ One reason state courts decline to hold hearings is that they cannot afford the accompanying costs: reimbursement of counsel for indigent prisoners, witness and court reporter fees, and salaries for judges, court clerks and security personnel.

Resources available for capital trials are a function of two conditions: the funds and personnel available to process capital cases, and the number of cases to be processed. This explains why high rates of serious capital error are linked to low funding for capital courts *and* high numbers of capital and other cases to process. This in turn reveals how closely this supporting conclusion is tied to our principal conclusion: **More capital prosecutions and sentences lead to more strain on the system, more delay and more serious error.**

Column F of Table 18, p. 346 above, compares states based on their weighted average direct expenditures on their court systems.⁷⁹⁵ States vary substantially in this latter regard, from less than \$1 of direct court funding per capita on average in Nebraska, Utah and Georgia, to over \$3 of court

funding per capita on average in Connecticut. Although as we note above, this explanatory factor has only a modest effect for differences in spending levels at or above the 34-state average, *below-average* funding of courts adds as many as 10 percentage points to predicted capital reversal rates, holding other factors constant.⁷⁹⁶

Our measure of the effect of high backlogs of capital and non-capital cases awaiting disposition by the courts is an “interaction” effect for which predicted reversal rates cannot be accurately calculated. The states, however, can be compared based on the extent to which the combination of high capital and non-capital caseloads increases their risk of serious capital error. That comparison, in the attached note, reveals that **this factor poses an especially high risk to five states: Texas, Illinois and Pennsylvania and especially California and Florida.**⁷⁹⁷

4. **Controlling for other factors, more recent death verdicts are much more likely to be reversed on state direct appeal than earlier verdicts; there is no reliable evidence that the quality of death verdicts has improved much since the early 1980s.**

Figures 2A and 2B, pp. 55-56 above, reveal that after fluctuating in the 1970s, capital reversal rates for the three review stages combined were high (50%- or 60%-plus) and fairly stable from the early 1980s through the end of the study period. Those charts plus Figures 2C-3B, pp. 57-58 and 60-61 above, and a figure in our earlier Report, reveal the same stability from the early 1980s forward for direct appeal and federal habeas reversal rates but suggest that state post-conviction reversal rates may have risen somewhat in that period.⁷⁹⁸ Our regression analyses ask a different question about changes over time: *Beyond the effect of other significant factors*, have error rates increased or decreased in a statistically significant way during the study period? What this inquiry measures is the influence of forces that are not captured by specific explanatory factors in the

analysis but whose effect is time-sensitive and thus is registered by a general measure of patterns of change over time. The question this factor poses is **whether forces *other* than those captured by the specific explanatory factors in the analysis drove reversal rates higher or lower than they would have been had the specific factors been the only ones at work.**

Our first conclusion is that in all of our analyses that calculate reversal rates as a proportion of imposed, rather than reviewed, death verdicts, a force with a downward effect on reversal rates over time *is* at work. That force, however, is not related to changing amounts of *error* over time, but to changing amounts of *unfinished appeals*. Appeals that were not completed as of the end of the study period artificially depress reversal rates, because fewer finished appeals means fewer outcomes of any sort, including reversals, as a proportion of imposed verdicts.⁷⁹⁹ Because the later a death verdict was imposed, the more likely it is that the verdict did not finish being reviewed by the end of the study period, later verdicts are automatically associated with lower reversal rates as a proportion of imposed death verdicts. Because this relationship between later verdicts and lower reversal rates holds true for flawed, as well as unflawed, capital verdicts—the relationship is sensitive to whether review occurred, not whether flaws were discovered when it occurred—the **use of time trend as an explanatory factor nicely controls for the effect of *delay* (unfinished appeals),⁸⁰⁰ but does not gauge changing rates of *error* over time.⁸⁰¹**

The downward influence of delay on reversal rates over time is exacerbated in federal habeas cases where reversals due to serious error take longer to occur than affirmances.⁸⁰² As a result, flawed verdicts are under-represented among verdicts finally reviewed by the study end date, and over-represented among verdicts remaining to be finally reviewed on that date, with the bias affecting later cohorts of verdicts more than earlier ones, because higher proportions of later verdicts

were still awaiting final review as of the study's end date.

These delay-driven biases against counting reversals and (in the latter case) in favor of counting affirmances guide the interpretation of significant changes in reversal rates over time:

- In analyses calculating reversal rates as proportions of imposed verdicts in which reversal rates decline over time, the result cannot be interpreted with any precision. We know that at least some of the decline is due to the delay-related, error-neutral effects just described. But we cannot say how much of the decline is attributable to delay. It could be that, apart from the effect of other factors, improvements in the quality of death verdicts are also causing reversal rates to decline over time—adding an error-related decline in reversal rates on top of the delay-related decline just discussed. But it could just as easily be that, after accounting for other factors, later verdicts were actually *more* flawed than earlier ones—thus counteracting some of the delay-driven decline in reversal rates that otherwise would have appeared. Thus:
 - When reversal rates calculated as a proportion of imposed verdicts drop significantly over time, it is impossible to determine whether that drop is entirely delay-related or is also affected by changes in error over time.
 - On the other hand, in analyses of reversal rates calculated as proportions of imposed verdicts in which reversal rates do not drop significantly over time, it is likely that an *increase* in error over time (after controlling for other factors) has occurred. In that event, it is only increasing error rates over time (after accounting for other factors) that can have counteracted the delay-related biases that otherwise would have caused reversal rates to decline significantly over time.
- Declining reversal rates at the federal habeas stage are also difficult to interpret. At least some part of that decline is due to systematically longer delays in federal habeas review of flawed verdicts than in habeas review of unflawed verdicts. This again makes it impossible to tell whether error-related decreases or increases in flawed verdicts reaching that stage are adding to or counteracting the delay-related decline.
- Analyses of relationships between later verdicts and reversal rates calculated as proportions of *reviewed* (as opposed to imposed) death verdicts at review stages other than the habeas stage are subject to no delay-related biases. Reversal rates in these analyses are not sensitive to delay because delay affects the base number of death verdicts (the number reviewed) as much as the number reversed.⁸⁰³ Nor do flawed verdicts take systematically more or less time to be reviewed at stages other than the federal habeas stage. As a result, any changes in reversal rates over time that these analyses find are reliable indications of the size and direction of changes in error rates that are not captured by other factors in the analysis.

Analyzed under these guidelines, our analyses reveal the following:

- **After the effect of all other factors on error rates is accounted for, state high court judges on direct appeal found substantially higher rates of serious, reversible error in recent death verdicts than in earlier ones.** Analyses 3, 4 and 10 reliably evaluate the relationship between the year death verdicts were imposed and the amount of serious reversible error found at the state direct appeal stage, without any delay-related bias. All three analyses find that, after accounting for other important factors, **the later a death verdict was imposed, the higher the probability that it would be reversed on state direct appeal based on a finding of serious error.** The result is highly significant, and the upward effect on reversal rates of a verdict's having been imposed later rather than earlier in the study period is large. **Holding other explanatory factors at their averages, Analysis 3 predicts a 9-fold increase in direct appeal reversal rates over 23 years (from about 9% to about 80%).**⁸⁰⁴ This finding is important because state direct appeal is the only stage that reviews nearly all death verdicts, and it accounted for about 8 of every 10 reversals during the study period.⁸⁰⁵
- In order to make the best use of our data on capital reversal rates, it was necessary in many of our analyses to measure reversal rates as proportions of imposed verdicts.⁸⁰⁶ Most analyses also included the federal habeas stage as at least one of the review phases being studied. As a result, most of our analyses are affected by both delay-related, error-neutral biases noted above.⁸⁰⁷ And two studies of the federal habeas stage were affected by the second bias, but not the first.⁸⁰⁸ As those biases would predict, later verdicts were associated with lower reversal rates in a number of these analyses.⁸⁰⁹ Contrary to expectations, however, the size of the effect was fairly small.⁸¹⁰ And in three analyses, there was no statistically significant relationship between later verdicts and lower reversal rates.⁸¹¹ These latter results suggest what our direct appeal studies found: that **there is an upward trend over time in the amount of serious error that is not accounted for by the other factors in the analysis**, which partially—and in some analyses entirely—neutralizes the downward force of the two delay-related biases discussed above.⁸¹²

Given these circumstances, our regression analyses modestly enhance what the raw trend of reversal rates over time—depicted in Figures 2A-3B—tells us about the effect on reversal rates of the passage of time. Those analyses are most informative as to the state direct appeal stage, because it is only at that stage that they provide a relatively accurate picture, undistorted by the effect of delay, of the relationship between the passage of time and the amount of serious error discovered by the courts after accounting for other factors. Those analyses show that **after controlling for other**

factors, death verdicts imposed later in the study period were substantially more likely to be reversed at the state direct appeal stage—where nearly four-fifths of all capital reversals occurred during the period—than verdicts imposed earlier in time. Our best analysis predicts that, if other factors had remained constant at their averages, direct appeal reversal rates would have risen 9% per year during the 23-year study period.

Other significant factors did not, of course, remain constant at their averages, and reversal rates in fact were fairly steady during the latter half the study period.⁸¹³ What increased over time, therefore, is the amount of error found on direct appeal that is not accounted for by the specific explanatory factors we have identified, and instead is registered by our general measure of time trend. This suggests that **reforms aimed at alleviating the specific conditions that our analyses have shown to be significantly related to reversals may have less effect than is desired because of the influence of other factors—picked up in our analyses by our measure of time trend—that are associated with increasing amounts of capital error over time.**

What we can say with confidence based on these results is that:

- **Overall capital reversal rates remained high and fairly steady from the early 1980s through the end of the study period, averaging about 60% of the verdicts reviewed each year.**
- **There is no evidence that conditions causing high capital error rates are curing themselves over time.**
- **Most disturbingly, at the direct appeal stage, factors beyond those specifically identified by our regression analyses are linked to increasing amounts of serious error over time.**

5. Reviewing courts do not effectively keep serious mistakes from being made or death verdicts from being carried out.

State direct appeal and post-conviction courts and federal habeas courts are the capital

system's quality control inspectors, whose job it is to detect seriously flawed death verdicts imposed at trial and to send them back to be retooled or scrapped. Our analyses examine the outcomes of thousands of these inspections mainly to identify causes of serious flaws at trial. But the analyses also shed light on the effectiveness of the inspection system. Such systems have two goals—to catch individual mistakes before they cause unintended harms, and to feed back information and sanctions to those who made the mistakes—particularly information and sanctions focused on *patterns* of problems—so that error does not occur in the future. This sections concludes that the review process is not a failsafe method of achieving either of these goals. We begin with the second.

a. The review process fails to keep high rates and amounts of serious error from recurring.

The capital review system fails utterly to keep serious mistakes from being repeated. Rates of serious capital error were disturbingly high during the entire 23-year study period—with an overall rate for the period of 68% that remained around 60% even in the last years of the study.⁸¹⁴ Although there is some evidence suggesting (among other possible conclusions) that the burden of catching error has shifted somewhat from federal courts at the third inspection stage to state courts at the first and second stages,⁸¹⁵ **there is no reliable evidence that rates and amounts of error have declined substantially since the early 1980s.**⁸¹⁶ **Moreover, for nearly two decades, the rate at which people sentenced to die have thereafter been exonerated has been fairly steady at 1 innocent death row inmate for every 7 or 8 people executed.**⁸¹⁷ Nor—at least apart from last year's incipient and scattered reforms⁸¹⁸—is there any evidence of ameliorative changes since the study period that were designed to, or can be expected to, lead to lower rates of serious error in capital cases. Instead, as we note above, the most important changes in the years

between 1995 and 2000 were designed to substantially *decrease* the level of scrutiny and feedback that appellate courts give to the capital trial process, and that federal reviewing courts give to state reviewing courts.⁸¹⁹

For this reason alone, **the capital system is broken**. This is best illustrated by asking whether decades of 50%-plus rates of serious error would be tolerated in any other public or private enterprise in this country. If goods coming off the production lines at Ford Motor Co., General Dynamics or Dell were so seriously flawed that they had to be sent back for repair or scrap 68% of the time, it is doubtful the enterprise would last a year—and it is certain that investors, regulators and consumers would shut down the operation long before its failures went on for decades.⁸²⁰ The same is true of 50%-plus rates of serious error in public operations, such as issuing social security checks, constructing schools or air traffic control. Nor would it be any consolation that the enterprise's chronic failures have not yet killed any innocent people—at least so far as can be proved.⁸²¹

Meticulous inspections or not, it is simply unreasonable—especially over the course of decades—to continue tolerating:

- **the *costs* of operating consistently failing enterprises and having to fund multiple overlapping inspections systems and repairs;**
- **the *delays* that complex, redundant and painstaking inspections require;**
- **the *inconveniences and injuries* that people suffer from persistently faulty products and outcomes; and**
- **the *risk* that a day of reckoning will arrive when inspections fail, and when a seriously flawed product or system causes an innocent person's death.**

It thus is clear that **the capital review process fails as a means of feeding back information and, where necessary, sanctions on defense and government lawyers, law**

enforcement officers, and judges who conduct flawed capital trials. The first reason for this failure, as a number of investigative journalists have recently documented, is that appellate courts understand their role as examining each case separately. They accordingly keep no aggregate data about how frequently they reverse death verdicts due to errors committed by particular lawyers and law offices, police officers and police forces and lower court judges. And they entirely pass over many errors they find as non-prejudicial, harmless or waived—even where those errors contribute to patterns of abuse that previously or subsequently have resulted in reversals. As a result, although court decisions in fact often reveal egregious patterns of error by particular defense lawyers, prosecuting offices, police forces, and trial judges, those patterns rarely are noticed, much less sanctioned in any way, by reviewing courts. Consequently, problems can fester for years.⁸²²

In addition, a review process taking 12 years on average before executions occur is unlikely to be an effective way of informing, instructing or disciplining the actors responsible for flaws the review process finds. As investigative news reports also have recently documented, by the time the capital review process is finished and a reversal occurs, the offending trial-level actors have usually moved on to other jobs.⁸²³ In most cases, moreover, trial-level actors do not have to defend flawed capital trial verdicts on appeal, because that task is handed over by defense lawyers to new appellate lawyers in a different office, and is handed over by local prosecutors to lawyers in the state attorney general's office. In neither case do the new lawyers have authority to discipline trial-level actors whose mistakes the later lawyers must defend. Instead, appellate lawyers for the state are often blamed for having “lost” the case on appeal when the verdict is reversed.⁸²⁴

Nor, as those same reports have shown, do court reversals ever lead to bar discipline for lawyers, loss of jobs for law enforcement officers or other state employees, or sanctions for

judges who repeatedly commit serious error.⁸²⁵ Rather the only “sanction” imposed is an order to retry the case—typically handed down many years after the fact. For all these reasons, **nearly the entire cost of the review process and its outcome is borne, not by the trial-level actors who committed the errors in the first place, but by taxpayers spread throughout the entire state (who fund the state court system and state attorney general’s office) and throughout the entire United States (who fund the federal court system and the lawyers who represent indigent capital defendants in those courts).** Because local taxpayers do not have to bear most of the costs of the mistakes local officials make, they have little reason to discipline local officials for their mistakes by voting them out of office. And because the state and federal taxpayers who do foot the bill are removed from the local situation, they typically have no idea what is happening and, if they do, have no recourse against the responsible officials.

Our study provides evidence of **disturbing ways in which the chronically failing capital system may actually reward actors who are responsible for many of its flaws.** Our principal finding is that excessive death-sentencing is the most crucial source of serious capital error. An important supporting finding is that serious error is especially common in states where judicial selection techniques give judges strong incentives to conform their rulings to popular sentiment. Together, these findings suggest that judges and probably other officials⁸²⁶ benefit politically from each additional death verdict they are at least partly responsible for securing, including in weak or marginal cases where the probability of reversal is great. Particularly given that most of the costs of curing the resulting errors fall on others, the clear incentive the system gives officials is to cast the net of capital punishment law and policy still wider, pulling in progressively weaker cases in which the likelihood of error is progressively higher. Added to this is the fact that higher numbers of death

verdicts mean more delays on appeal, which in turn tends to dampen and obscure reversal and reversal rates and to delay the point when the case will be sent back for retrial⁸²⁷—further weakening any disciplining force of reversals when they finally come.

An analogous process affects the work of skilled capital defense lawyers—mainly from out-of-state civil rights organizations and law firms—whom our study shows have the greatest success in overturning seriously flawed capital verdicts at the final, federal habeas stage of review.⁸²⁸ Because there are so few of these lawyers and so few resources to fund their work—a problem Congress and the states made worse when they shut down the “capital case resource centers” in 1995⁸²⁹—these lawyers cannot handle the thousands of capital trials taking place all over the country each year, and instead can only intervene at the last stage of review after state court reversals and review delays have narrowed the number of pending cases to a manageable number. Given how often their clients’ death verdicts are overturned due to persistent flaws in capital verdicts, it is not surprising that these lawyers work hard to preserve a robust three-stage review process in which they are largely responsible for the last stage. Nor is it surprising that they are mistrustful of promises to trade meaningful trial-level improvements, which thus far have not materialized, for limits on post-trial review that by themselves will make things worse.⁸³⁰ As understandable as these views are, however, they have the same counterproductive effect as the actions of the opposing camp. They divert good lawyers from the trial phase, leaving poor lawyers to contribute to high death-sentencing and high error rates, and they preserve the lengthy review process that the weak trial system requires. In other words, **they keep a broken system going, for decades, chronically generating too many death verdicts—most of which, as a result, are seriously flawed and unreliable—which in turn require an expensive review process that is so delayed that it stymies execution of valid verdicts**

and so overburdened it misses egregious mistakes.⁸³¹

b. The review process does not catch all serious mistakes.

Our results also indicate that **the capital review process has not achieved the other goal of an inspection process: catching flawed products before they harm innocent people.** Our case studies of some of the death row inmates shown to be innocent after judges at all three review stages had *approved* their verdicts for execution reveal that **the judicial inspection process has failed on several occasions to catch the most serious capital error of all—the conviction and capital sentencing of an innocent man or woman.**⁸³² Of the 99 death row inmates who have been exonerated during the modern death-sentencing era, over 60% had their capital verdicts approved by at least one set of appellate courts.⁸³³

Our results also help explain why appellate courts fail to catch even the most egregious capital errors. In each case study of an innocent man approved for execution by a full complement of state and federal courts, the courts took note of the questionable procedures later shown to have put an innocent man on death row and even acknowledged doubts about the reliability of the resulting verdict. Nevertheless, the courts refused to overturn the verdicts because the innocent defendant was unable to satisfy the strict standards for proving that he pleaded the claim properly at trial and on appeal, and that the acknowledged errors in his case had “prejudiced” him.⁸³⁴

Our regression analyses in turn reveal evidence that reviewing courts sometimes set the bar to reversal high in response to political pressures and a desire to avoid the controversy that frequently accompanies reversals but almost never accompanies affirmances.⁸³⁵ In addition to the political pressures discussed above to impose death verdicts at trial in cases that are not highly aggravated, where error rates are the highest,⁸³⁶ our results provide evidence of pressures to approve death

verdicts on appeal despite the presence of error that renders the verdicts unreliable:

- The more political pressure imposed on judges by a state's method of selecting—which usually means *electing*—judges, the higher is the risk that capital trial verdicts imposed in the state will be seriously flawed.⁸³⁷ State judicial selection techniques have the strongest association with the discovery of reversible error at the federal habeas stage, where the judges are appointed and life-tenured and thus are immune to the pressures generated by state judicial selection techniques (Analysis 6). The association between the discovery of error and state selection methods is somewhat weaker but still close to significant at the state direct appeal stage, where pressure on elected judges triggered by particularly notorious capital cases is moderated by the passage of time between the commission of the crime and the appellate ruling and by the fact that most constituents of appellate judges come from communities besides the one where the verdict under review was imposed and thus are not as interested in how the court decides the case (Analyses 3, 4, 10). There is *no* evident relationship between judicial selection techniques and the discovery of error by state post-conviction judges, who usually are the same trial judges who imposed the death verdict in the first place, and who face the most direct political pressure from cases under review because *all* their constituents come from the community where the crime occurred (Analysis 5).⁸³⁸ This suggests that **political pressures that are associated with high rates of error at trials supervised by elected judges may also keep the same judges from correcting errors during subsequent state post-conviction proceedings, and may discourage elected high court judges from reversing verdicts on direct appeal.**
- As is revealed by main Analysis 1 and a wide array of confirming analyses (Analyses 2-5, 7-18), **state direct appeal judges and state post-conviction judges are significantly more likely to find serious error and reverse death verdicts imposed in more urbanized and populous states and counties, and less likely to reverse verdicts imposed in less urbanized and populous places.** Analysis 6 reveals the opposite pattern for **federal habeas judges, who are more likely to find serious error and reverse death verdicts from less urbanized and populous states and less likely to reverse those from relatively urbanized and populous states.**⁸³⁹ These opposing patterns are additional evidence of political pressures on state reviewing judges to affirm verdicts that, apart from such pressures, would be reversed due to serious flaws. Urban areas have more homicides and impose more death verdicts, any one of which is not very likely to make a strong and lasting impression on most local citizens. By contrast, less densely populated areas have a smaller number of homicides, each of which—and any death verdict imposed for it—is likely to be well known and important to many local citizens.⁸⁴⁰ Over the long run, therefore, reversing rural or small-town death verdicts is likely to be more controversial than reversing urban death verdicts, especially for state judges who face direct electoral discipline for locally unpopular decisions.⁸⁴¹ **At the two state stages of review, the predictable result of a desire to avoid locally controversial reversals is fewer reversals of verdicts from less populous areas than of verdicts from urban areas.** This result helps explain why the flawed verdicts found at the final federal habeas review stage—by appointed, life-tenured judges who are relatively

isolated from local political pressures—are disproportionately from more rural states. More generally, **it helps explain why the proportion of flawed verdicts found at each successive review stage does not shrink—as otherwise should occur in a properly functioning series of inspections—and instead why almost as high a proportion of flawed verdicts is found at the final capital inspection stage as at the first stage: 40%.**

- The table in the appended notes ranks states based on their population size and density and indicates the difference this factor makes, holding other factors constant, in whether states have above-average or below-average reversal rates. As the table reveals, when other factors are held constant at their average, states with low population density are prone to reversal rates as much as 30 percentage points below the 34-state norm when the reversal rates being explained are mainly those of state judges who are especially likely to suffer adverse political consequences from reversing death verdicts imposed in rural communities.⁸⁴²
- All analyses of reversals taking place at only the state direct appeal stage and at only the state post-conviction stage show that state courts with large backlogs of cases are more likely to affirm death verdicts than courts without such backlogs (Analyses 3, 4, 5 and 10).⁸⁴³ This suggests that **pile-ups of cases awaiting review, and associated delays and controversy, pressure state judges to move cases along as quickly as they can, including by affirming verdicts that in calmer times would be found to be seriously flawed.** Again, analyses of reversals taking place at only the federal habeas stage, where life-tenured judges are less susceptible to local political pressures show no similar effect.⁸⁴⁴
 - These results validate the explanation for federal court review of state court decisions famously given by Alexander Hamilton in *The Federalist Papers*. Federal court review of state decisions, Hamilton wrote, helps assure “an inflexible execution of the national laws” by national courts immune from “a local spirit” that sometimes compromises decisions of local courts. This is especially so, he wrote, when the national laws are designed to bar “arbitrary methods of prosecuting pretended offenses, and arbitrary punishment upon arbitrary convictions.”⁸⁴⁵ But the fact that federal judges are relatively immune from *local* political pressures does not make the final, federal review stage a firewall against *all* political influence on the review process. On the contrary, case-level Analysis 19 of federal habeas decisions provides evidence that **federal reviewing judges are influenced by national political pressures associated with the process by which they are appointed and promoted. Holding other factors constant at their average, Analysis 19 predicts that the probability that a capital verdict will be reversed rises or falls as much as one-third depending upon whether the review is by judges mainly appointed by Republican Presidents or by judges mainly appointed by Democratic Presidents.**⁸⁴⁶
 - Analysis 19 also provides strong evidence that reviewing federal habeas judges are forced to serve as replacement sentencers, to screen out the many death verdicts induced at trial as a result of excessively broad death-sentencing policies.⁸⁴⁷ Even so, **federal review is not a**

failsafe check on excessive, error-prone death-sentencing, given federal judges' susceptibility to political pressure, and given the proneness of the strict rules those judges apply to let some, even very serious, errors slip through.⁸⁴⁸

Reviewing judges thus are demonstrably incapable of curing all of the flawed verdicts imposed at capital trials. This is so in part because reviewing judges are susceptible to political pressures to affirm flawed death verdicts analogous to the pressures trial judges and other trial-level officials face to impose flawed verdicts in the first place—pressures that call for a forceful response to serious crime in general, but are divorced from the strength of the evidence and circumstances supporting a death verdict in particular cases.

c. The probability that innocent people have been executed is high.

As we discuss above, it is impossible to know how many innocent people have been capitally convicted, sentenced and executed—in part because officials are permitted to withhold DNA samples and other crucial information needed to determine the scope of the problem. The best researchers and policy makers can do, therefore, is to use available evidence to estimate the *risk* that innocent people have been executed.⁸⁴⁹ Our conclusion on that question is the same as the one Justice Sandra Day O'Connor reached in addressing bar groups last summer and this fall: **“If statistics are any indication, the system may well be allowing some innocent defendants to be executed.”**⁸⁵⁰

The best evidence we have been able to assemble based on counts, regression studies and case studies of the results of all three stages and each separate stage of court inspection of 4500 capital verdicts imposed in 34 states and 1000 counties across 23 years is as follows:

- **50%-plus rates of reversible error across nearly all states and years;**⁸⁵¹
- **strong indications, using multiple measures, that the errors causing these reversals are serious;**⁸⁵²

- **deep-seated and disturbing racial and political factors that are strongly associated with that error;**⁸⁵³
- **reviewing judges' inability to catch serious error even when it has caused an innocent person to be convicted and condemned;**⁸⁵⁴
- **reviewing judges' susceptibility to pressures to approve flawed capital verdicts;**⁸⁵⁵ and
- **high reversal rates persisting from the first to the last review stage, as opposed to the steadily shrinking rates of discovered error needed to instill confidence in the efficacy of inspection processes.**

Other analyses show that for every 7 or 8 death row inmates who are executed, another inmate in line to be executed is proven to be factually or legally innocent.⁸⁵⁶ Moreover, among the events helping to save innocent inmates before being executed were a documentary film maker's accidental discovery of flaws in one case while examining another; an investigation by college students as a class project in a second case; a police clerk's accidental release of a suppressed file in a third case; and a burglary at a prosecutor's office in a fourth—**fortuities that cannot be relied upon to keep miscarriages from occurring.**⁸⁵⁷

Together, these findings convince us that the probability that an innocent person has been executed during the modern death-sentencing era is high. The findings also convince us that lesser but still serious harms are rampant in the capital system, including the execution of individuals who were guilty of some offense but not one for which the law allows the death penalty.

D. Higher-Risk and Lower-Risk States, Given this Analysis

1. Connecticut and Colorado Compared to Florida, Georgia, Texas and Alabama.

As we warn above, Table 18 cannot not give a full picture of the risk of serious capital error that states face based on the factors our regression analyses identify.⁸⁵⁸ The table analyzes the effect of each factor while holding other factors at their 34-state average. It thus does not measure the

combined effect of all factors operating simultaneously. In addition, Table 18 does not account for three general factors our regression analyses consider—year, state and time trend—which gauge the influence of still other forces that are not studied directly but are associated with the location and timing of the relevant death verdicts and reversals. Subject to these limitations, however, it is possible very generally to associate a particularly high risk of error with a few states that fall fairly consistently on the high-end of the risk spectrum—and to compare those states to ones that more consistently fall towards the low end of the risk spectrum. In doing so, we consider the six important factors in Table 18 and the four additional factors addressed in the tables in notes 774, 788, 797 and 842.⁸⁵⁹

As a review of Table 18 and the accompanying tables makes clear, most states' 10 risk rankings are widely distributed across the spectrum from first (most risk of high capital reversal rates) to 34th (least risk of high reversal rates). In most cases, therefore, the information in Table 18 and the accompanying tables suggests particular areas where each state might focus policy attention without providing a strong basis for distinguishing the state from any other. In a small number of cases, however, states' risk rankings fall fairly uniformly towards one end of the risk spectrum or the other. On the low side, for example, are Connecticut and Colorado. Based on average conditions across the 23-year period,⁸⁶⁰ and on analyses of each of the 10 risk factors, holding other factors constant at their averages:

- **Seven of Connecticut's 10 risk rankings place it in the bottom half of the 34 states in terms of the probability of serious capital error, including four rankings in the bottom five of 34.** Most importantly, given our principal finding above, **Connecticut is ranked last in terms of the risk of error posed by its (low) capital-sentencing rate.** Thus, although Connecticut was one of four states with 100% reversal rates during the study period, that rate is based on a total of only two decisions and does not provide a fair estimate of the state's risk of serious capital error over the long haul. Our analyses suggest that Connecticut capital

verdicts pose less of a risk of serious error than verdicts in most other states.

- **Six of Colorado’s risk rankings are in the bottom half of all states, with an additional ranking on the border between the top and bottom half (17 out of 34).**⁸⁶¹ Colorado is ranked third-to-last in terms of the risk of error posed by its capital-sentencing rates. Colorado’s reversal rate during the study period was 75%—based on only four decisions, three ending in reversals.

Connecticut and Colorado may be contrasted to Florida, Georgia, Texas and Alabama. Based on average conditions across the 23-year period,⁸⁶² and on analyses of each of the 10 risk factors, holding other factors constant at their averages:

- **Eight of Florida’s 10 risk rankings place it in the top half of states based on its predicted risk of serious capital error, including two placing it in the top five among the 34 states.** A ninth ranking is on the border between the top and bottom halves of the 34 states (18 out of 34). The only ranking out of 10 on which Florida has a substantially *below* average risk of capital reversals is the result of its large backlog of capital appeals awaiting review—the third highest backlog in the country. As we note above, delay in the review process has the perverse effect of lowering reversal rates.⁸⁶³ Adding to concerns about the risk of serious capital error in Florida:
 - The state’s death-sentencing rate is 12th highest out of 34.
 - Three of the top ten counties in the nation with the highest death-sentencing numbers and rates are Florida counties.⁸⁶⁴
 - Florida has had more people removed from its death row following findings that they were not guilty than any other state.⁸⁶⁵

Florida’s overall capital reversal rate during the study period was 75%.

- **Seven of Georgia’s 10 risk rankings put it in the top half of all states in terms of the predicted risk of serious error. Four rankings put it the *top five* of all states.** Georgia is the only state among the 34 that is not in the bottom 10 states on at least one risk factor. And its lowest ranking (21 out of 34⁸⁶⁶) is due to its above-average number of death verdicts that are stuck in the appeals process awaiting final review. Working modestly in Georgia’s favor, its death-sentencing rate ranks only 18th out of 34. **Georgia’s overall capital reversal rate during the study period was 80%.**
- **Seven of Texas’s 10 risk rankings are in the top half of the 34 states. Two are in the top five.** As in the case of Florida and Georgia, the factor on which Texas ranks the lowest in

terms of predicted reversals is a result of its high backlog of capital cases awaiting review—the second highest in the nation. Also moderating predicted reversal rates is Texas’s relatively low death-sentencing rate—25th out of 34. **During the study period, Texas had an overall capital reversal rate of 51%.** Although high in absolute terms, this rate is towards the low end compared to other states. See Figures 1A and 1B, pp. 50-51 above. One important line of inquiry for Texas, given its high rankings on most risk factors, is whether—as some have recently claimed—its relatively low capital reversal rates are due to excessively lax state court review of capital verdicts.⁸⁶⁷ Other explanations are Texas’ high backlog of verdicts awaiting review, which tends to depress reversal rates, and the state’s relatively low death-sentencing rate.

- **Six of Alabama’s 10 risk rankings place it in the top half of the 34 states. Three risk rankings place it in the top five among the 34 states.** During the study period, Alabama’s death-sentencing rate was 11th in the nation. **Alabama’s overall reversal rate during the study period was 77%.**

2. Virginia.

As is discussed above, Virginia has extremely low capital reversal rates.⁸⁶⁸ Compared to other states with cases decided at all three review stages during the study period, Virginia’s 17% overall reversal rate—the product of the lowest state direct appeal reversal rate in the county *and* the lowest federal habeas reversal rate in the country—is more than two standard deviations below the mean. Two theories have been offered to explain Virginia’s low reversal rates—uniquely high-quality death verdicts or, on the other hand, uniquely low-quality court review.⁸⁶⁹ Our findings suggest that the truth lies in between those two poles. In fact, **Virginia’s rankings on the 10 risk factors tend to cluster around the two poles of fairly low, and fairly high, risk of serious capital error:**

- On the one hand, **Virginia falls among the *bottom* five states in terms of its risk of serious capital error in four of the ten risk categories** in Table 18 and the allied tables. Chief among these low-risk categories is **Virginia’s death-sentencing rate, the sixth lowest in the nation.** Virginia also ranks low in terms of the political pressure put on state judges through the electoral process, and given the state’s relatively strong record of apprehending and punishing serious criminal—both of which tend to relieve pressure to use the death penalty as a stop-gap response to ineffective law enforcement strategies.⁸⁷⁰
- On the other hand, **on three of the remaining six risk factors, Virginia ranks in the *top***

ten among the 34 study states—including with respect to the two racial factors that pose a high risk of capital error. The state ranks eleventh on still another factor.

Based on the factors our study identifies as important, we conclude that **the risk of serious capital error in Virginia is, on the whole, fairly moderate, but that the risk is not low enough to explain the state’s extremely low reversal rates.** Our findings tend to confirm those of the State’s Joint Legislative Audit and Review Commission, which recently concluded a year long study of the state’s death penalty ordered by the state legislature. The Review Commission concluded that federal and state judges’ adherence to strict rules limiting review for serious error in capital cases, and the state high court’s narrow review of the appropriateness of death sentences in particular cases, may have let stand the convictions and sentences of some death row inmates who did not receive proper trials.⁸⁷¹ **We, too, conclude that lax state and federal court review of Virginia death verdicts has probably depressed the state’s *reversal* rate below its actual rate of serious capital error.**

VIII. Policy Options

Our study results identify a series of conditions associated with high rates of capital error. Every such error is serious, casting substantial doubt on the reliability of a death verdict. Together, all such error—which was found in *68% of all capital verdicts imposed and fully reviewed between 1973 and 1995*—creates a high risk that innocent people have been executed, and will continue to be executed unless major policy changes are made.

A central goal of this decade-long study and the detailed description of its findings here and in *A Broken System, Part I* is to suggest policy options for responding to serious capital error and the resulting risk that innocent people will be executed. We divide our discussion of these options into three sections. Section A discusses the only two options that promise substantial declines in chronic capital error rates and the resulting risk of executing the innocent: severely curbing the scope of the death penalty to reach only the small number of offenses as to which there is a broad social consensus that only the death penalty will serve, or ending the death penalty entirely. The next section discusses options that will likely have less impact on serious capital error and the risk of executing the innocent but, especially in combination with each other, can help moderate the problem. The final section discusses options to avoid because they might magnify serious error.

We present the reforms discussed below as options among which death penalty states and counties might choose based on the particular risk of serious capital error each jurisdiction faces from the 10 or so risk factors our study identifies and from other factors. In three instances, we identify options as close to a policy imperative in the states and counties to which they apply.⁸⁷²

Information on the size or intensity of each of the 10 risk factors in each of the 34 study states during the 1973-1995 period can be found in:

- Table 18, pp. 344-45 above; and
- the accompanying tables in notes 774, 788, 797, 842 above.

Additional comparative information on the 34 study states that fills out their respective risk profiles in regard to serious capital error is in:

- Figures 1A, 1B, 9, and 11-21, pp. 50-51, 79, 121-32 above; and
- Appendices A (state report cards), C (state post-conviction cases by state) and D (examples of federal habeas cases).

Similar information on the 244 American counties that capitally sentenced five or more people during the study period, and less extensive information on all 1004 counties that imposed at least one capital sentence, is presented in:

- Figures 42A and 42B (map of all capital counties), pp. 248-49 above;
- Tables 10-16, 19 and 20, pp. 290-91, 294-95, 297-99, 301, 304, 346 and 348 above; and
- Appendix B.

These and other existing sources may not provide enough information to comprehensively assess local conditions in particular states and counties. For that reason, **it is strongly recommended that states and counties undertake their own comprehensive analyses of local death-sentencing practices, problems and conditions—as Arizona, Illinois, Indiana, Maryland, Nebraska, Nevada, North Carolina, Virginia and the U.S. Department of Justice have recently done.**⁸⁷³

A. Attacking the Problem by Severely Curtailing the Death Penalty

As we develop in Part II above, serious capital error is a chronic problem with severe effects and monumental risks, including that innocent people will be executed. What is worse, rigorous examination reveals that some of the conditions that are most strongly associated with capital error

are not easily changed.

Underlying several of the important explanatory conditions revealed by our regression analyses are fears about serious crime that generate pressure on officials to extend the death penalty to cases that are not highly aggravated—where the risk of error, reversal, retrial and a non-capital retrial verdict is high.⁸⁷⁴ Among the sources of such fears are well-founded doubts about some states' response to serious crime given their poor records of apprehending, convicting and punishing serious criminals.⁸⁷⁵ Racial conditions—how heavily the threat of homicide is concentrated in the white as opposed to the black community, and the relative size of the black community—are other apparent sources of pressure to use the death penalty in the less discriminate manner that our findings associate with a high risk of error. Even the size and density of a state or county's population are linked to the amount of serious error discovered at various stages of review.⁸⁷⁶ Obviously, none of these conditions—the problem of serious crime,⁸⁷⁷ the racial makeup of communities, the distribution of homicides between whites and blacks, and the distribution of the entire population between urban and rural areas—can easily be changed in an effort to directly alter the conditions associated with high capital error rates.

Moreover, even where changes *can* have a direct impact on potentially causal conditions that our analyses identify—*e.g.*, amending state constitutions to replace judicial elections with appointment for life,⁸⁷⁸ or increasing funding for overburdened courts⁸⁷⁹—are unlikely to occur any time soon. To make matters worse, our analyses indicate that still other factors are at work that are registered by our general measure of time trend and are associated with large increases in error rates over time.⁸⁸⁰ Because the exact contours of these latter influences are unknown—except for the fact that they are getting worse—it is possible that the ameliorative effect of reforms aimed at *known* risk

factors could be overwhelmed by the intensifying effect over time of less well-understood forces.⁸⁸¹

As the next section indicates, therefore, **the best that can be done is to try to narrow the available capital-sentencing outlets for pressures associated with race, crime and politics by more firmly restricting the penalty to highly aggravated cases.** The uncertain prospects for this approach are indicated by a separate study finding: The conditions that most strongly predict capital error operate mainly at the level of state and county death-sentencing policy, not at the level at which policy is applied to individual cases.⁸⁸² The actions most associated with capital error are those broadly defining the *classes* of cases and *threshold* amounts of evidence of guilt and aggravation that qualify for capital charging and sentencing—inclusive definitions of capital murder, long lists of aggravating factors or excessively encompassing factors, and open-ended interpretations of those definitions and factors on a statewide basis by state supreme courts and state’s attorneys and on a county-wide basis by local sentencing courts and district attorneys.

This finding suggests that it will be difficult to design effective capital reforms. Inevitably, **the officials whose policies must be relied upon to moderate the risk of dangerous error by limiting opportunities to use the death penalty in marginal cases are the same officials who have been susceptible in the past to pressures to expand the death penalty, with the devastating and chronic effects that we describe above.**⁸⁸³

Because harms from chronic capital error are great, the causes cannot be directly rooted out, and indirect remedies are unreliable, **some states or counties might conclude that the best answer to chronic capital error is to stop using the death penalty entirely, or to limit its use to the small number of offenses as to which there is something approaching a social consensus that only the death penalty will serve.** For some states, the events of September 11, 2001, in New York City,

Washington, D.C. and Pennsylvania, and events in Oklahoma City in April 1995, may provide a benchmark for defining those offenses.

B. Moderating the Problem Through Targeted Changes in Capital Policy and Practice

Some states and counties will want to explore less comprehensive reform options before concluding that the penalty's costs are not worth incurring. After discussing the overriding goal of ameliorative policies, this section lists policy options aimed at entire sets of cases, then options applicable case by case.

1. The central goal of moderating reforms: limiting the death penalty to very highly aggravated cases.

The goal of the collection of reforms discussed below is dictated by our principal finding: **The more states and counties use the death penalty, and the more often they extend it to cases that are not highly aggravated, the higher is the risk that any death verdict they impose will be reversed due to serious error.** Additional findings supplement this main conclusion by identifying conditions that pressure officials to adopt over-broad death-sentencing policies (crime fears based on inadequate law enforcement policies, relatively high concentrations of homicides in the white community relative to the black community, and large African-American populations) or the mechanisms through which these pressures are communicated to officials (direct election of judges and other officials, controversies surrounding reversals of death verdicts in non-urban areas and complaints about backlogs of capital cases awaiting review). As we discuss above, however, these contributing factors are difficult to eliminate directly,⁸⁸⁴ and probably are only a subset of the conditions prompting excessive capital sentencing.⁸⁸⁵ As a result, reform efforts can best address the problem by attacking the crucial condition that links these other conditions to high error rates and

is itself linked to a high risk of error: excessive death sentencing in cases where aggravation is not strong.

Therefore, **the central goal of these targeted reform options is to limit the death penalty to “the worst of the worst”—to defendants who can be shown without doubt to have committed a murder characterized by high concentrations of undeniably aggravating circumstances.** Accomplishing this goal requires firm policies that (1) remove the death penalty from consideration in cases where the evidence of guilt or the level of aggravation is not strong but where law enforcement or other crises create powerful pressures to apply the death penalty broadly, and that (2) insulate death-sentencing policies and decisions from direct political pressure as much as possible.

Strong support for these conclusions is found in the principal finding of the first comprehensive, state-specific study of the death penalty to be completed in the recent period of concerns about capital punishment. As the chief researcher on that study, University of Iowa Law Professor David Baldus, testified to the Nebraska Legislature, which commissioned the study, “[i]f Nebraska would limit asking for the death penalty, . . . there would be fewer questions about application of the penalty” and less reason to fear that “race or wealth of the criminal or victim was a factor in receiving the death penalty.”⁸⁶

Some of the options laid out below focus on the guilt-innocence decision in potentially capital cases. Others focus on the sentencing decision. **Both sets of options would likely decrease the risk of serious error in both types of decisions.** Our findings indicate that **it is the goal of securing death *sentences* in cases where the evidence or aggravation is not strong that invites the application of unreliable policies and practices. That invitation to error extends to both**

phases of trial, because both are crucial to obtaining a death sentence. If police, prosecutors, trial judges and jurors follow policies that discourage capital *convictions* in cases where the evidence is weak, that of course will also discourages capital sentences in the same cases. Likewise, **if trial actors follow policies they know make a death sentence unlikely because the evidence, level of aggravation vs. mitigation or law does not clearly warrant that sentence, their temptation to cut corners or use over-broad policies to secure a capital conviction as a prelude to a death sentence also declines.**⁸⁸⁷

2. Ten ways to moderate error by modifying capital policy and practice.

This section lists 10 ways to lower the risk of serious capital error, starting with six options for categorically moderating the number of capital convictions and sentences.

a. Require proof beyond any doubt that the defendant committed the capital crime.

In a speech to the National Press Club last year, Oklahoma’s Republican Governor Frank Keating, a strong death-penalty supporter, proposed **limiting the death penalty to defendants found beyond *any* doubt to have committed a capital crime.** Conservative columnist John Podhoretz made a similar proposal in the *New York Post* as a way to “save the death penalty.”⁸⁸⁸ Our study results provide strong support for these proposals. Our analyses show that:

- The crucial condition putting states and counties at risk of high capital error rates is high rates of capital convictions and sentences per 1000 homicides.
- The crucial condition putting particular capital *verdicts* at risk of error is their imposition where the case for convicting the defendant of a capital crime, or for imposing the death penalty, is not strong.
- The worst effect of serious error is convicting and potentially executing innocent persons.
- Appellate courts are not a failsafe protection against these harmful outcomes, given their

record of affirming the convictions of innocent death row inmates—including where they acknowledged that the evidence was weak but affirmed anyway because the defendant could not then prove he was probably innocent.⁸⁸⁹

Under these circumstances, **an especially effective way to discourage death verdicts where the risk of error is great is to eliminate cases where there is doubt about the defendant's guilt.**

This would replace the rule in most states that jurors who harbor doubts about a capital defendant's guilt must still convict him if they think his guilt is “beyond a *reasonable* doubt.”

In many capital cases, fingerprints, DNA, statements by the victim before dying, uncontradicted and corroborated eye-witness accounts, confessions under non-coercive conditions, and the defendant's possession of information or property that only the killer could have, leave jurors with no doubt about who killed the victim. Faced with this kind of evidence, some capital defendants *admit* guilt at trial, arguing for a lesser sentence based on extenuating circumstances. In other cases, however, the evidence is contested and unreliable—most classically, when it is based on testimony of jailhouse informants who profit by claiming the defendant confessed to the killing, questionable forensic techniques such as hair and fiber analysis and uncorroborated eye-witness identifications made under stress and describing fleeting events that were difficult to see. Faced with such evidence, but also with an egregious crime and a desire to protect the community, jurors told to decide whether the evidence is “beyond a reasonable doubt but not beyond any doubt” may convict innocent defendants because they are convinced the *crime* requires severe punishment, even if they are not sure the defendant committed it.⁸⁹⁰

The “beyond a reasonable doubt” standard also invites error because courts have been unable to narrow the kinds of doubts that jurors may harbor and still convict. Many courts have given up trying to define “reasonable doubt” and left it to jurors to decide for themselves whether doubts they

harbor are “reasonable.”⁸⁹¹ The result in capital cases is that jurors faced with evidence that *whoever* committed the crime poses an intolerable threat to the community, and who believe the defendant is the only suspect, may conclude that the “reasonable” thing to do is convict the defendant and sentence him to die despite lingering doubts about his guilt.

When lengthy or permanent imprisonment is the result—allowing mistakes to be corrected whenever they are discovered—the risks from using the ambiguous “reasonable doubt” standard are justified. **The same risks are less sensible when the penalty is death, and mistakes are not correctable. This is especially so given the connection our analyses reveal between death-sentencing policies encompassing relatively weaker cases and a higher risk of serious, reversible error. Those findings counsel against using the death penalty when jurors and reviewing courts have doubts about the defendant’s guilt, because it is in just such “close” cases where the probability of serious error is the greatest.**

b. Require that aggravating circumstances substantially outweigh mitigating ones and warrant death before a death sentence may be imposed.

Capital jurisdictions are split over how aggravated a first-degree murder must be, after accounting for mitigating circumstances, before the death penalty may be imposed. A minority of jurisdictions bar the death penalty unless aggravating circumstances *substantially* outweigh the mitigating circumstances, and unless the jury is convinced that the death penalty is demanded by the high degree of aggravation that remains after mitigation is considered. These jurisdictions thus require jurors to impose a lesser sentence unless they are convinced that the case is so aggravated, after taking mitigating factors into account, that only the death penalty will suffice to punish the offender and protect society. By limiting the death penalty to the strongest cases for that punishment,

these policies are well calculated to avoid the high rates of unreliable error that our regression analyses associate with broad death-sentencing policies.

In other states, such as California and Pennsylvania, jurors are told that they must impose death if the aggravating circumstances outweigh the mitigating circumstances by any amount, however minor or minuscule.⁸⁹² And still other states, such as Arizona, require the death penalty when aggravating and mitigating factors are *evenly balanced*—*i.e.*, unless mitigating factors outweigh aggravating ones by some or even a significant amount.⁸⁹³ Kansas also followed this practice until late last year when its supreme court concluded that “fundamental fairness” demands that ties should go to the defendant when life is at stake.⁸⁹⁴ **It is hard to imagine sentencing policies that are more likely than those of California, Pennsylvania and Arizona to inflate capital-sentencing rates—and, as a result, rates of serious capital error—through imposition of death verdicts in marginal cases. A simple way to moderate this risk is to limit death verdicts to clear cases—those where the jury finds that aggravation so far exceeds mitigation that only a death verdict can suffice—and to adopt model jury instructions that clearly inform jurors about the findings needed to permit a death verdict.**⁸⁹⁵

c. Bar the death penalty for defendants with inherently extenuating conditions.

States also may moderate death-sentencing rates and the resulting risk of serious error and of convicting and condemning the innocent by barring capital prosecutions of defendants with inherently mitigating conditions, especially conditions that keep defendants from effectively defending themselves against false charges or from showing that the evidence and law do not permit their execution.

i. Mentally retarded persons. **Because mentally retarded defendants are inherently weak candidates for the death penalty, their cases are prime candidates for serious capital error, reversal and retrial, and they are especially at risk of being convicted and condemned despite being innocent.** This explains the strong trend recently towards banning execution of the mentally retarded.⁸⁹⁶ Even strong death penalty supporters such as Criminal Justice Legal Foundation legal director Kent Scheidegger acknowledge the appropriateness “[a]s a matter of policy . . . [of] a general rule that says we’re not going to execute someone who is in fact retarded.”⁸⁹⁷

Mental retardation is inherently mitigating because the condition decreases criminal blame for reasons beyond the control of the defendant and makes it more likely that the defendant was led into criminal activity by others more responsible than he or she. Moreover, as several recent exonerations establish—including those of Earl Washington and Anthony Porter, the defendants in two of our four case studies of capitally convicted and sentenced individuals whose death verdicts were upheld at all three levels of appellate review though they in fact were innocent—mentally retarded individuals are less capable than other defendants of helping their lawyers prove their innocence.⁸⁹⁸ In some cases such as Washington’s, retarded individuals are actually led into *confessing* to crimes they could not have committed.⁸⁹⁹

ii. Juveniles. The Constitution bars executions for offenses committed by children 15 years old or younger,⁹⁰⁰ and over a dozen states and the federal government ban the death penalty for offenses committed by teenagers below the age of 18. Several other states are considering such bans.⁹⁰¹ **Barring the death penalty for crimes committed by juveniles is another logical way to lower capital error rates by removing inherently marginal cases from capital eligibility.** Youth is a strong and well-recognized basis for mitigation for many of the same reasons as retardation and

also because of the greater chance that defendants who committed serious crimes before reaching maturity can be reformed by long prison terms, until they are no longer a danger to the community.⁹⁰²

iii. Severely mentally disordered defendants. Severe mental disorder is another long-recognized basis for mitigation, and another condition that prevents defendants from helping to prove their innocence or that they are unfit candidates for execution.⁹⁰³ Capitally trying such defendants is extremely expensive, given the many points during the trial when medical and psychiatric examinations, neurological tests and battles of experts are required in order to answer a lengthy series of vexing questions that determine the appropriate legal disposition of such cases: Was the defendant competent to waive his rights and confess or submit to other investigative procedures by police officers? Was he competent to stand trial—does he understand the proceedings; can he assist his lawyer? Was he insane or was his capacity “diminished” at the time of the crime? Did his mental disorder substantially impair his capacity to understand his actions and conform them to the law? Is he competent to be executed?

Death verdicts imposed on severely mentally disordered defendants are also unusually expensive to *review*, given the need for additional costly examinations and testimony by qualified physicians and mental health professionals. And such cases present many more reasons why death verdicts may be overturned, given the wide range of legal challenges that arise solely because the defendant is severely mentally disordered—*e.g.*, that the defendant was coerced into confessing to mental health professionals who examined him for the state, was incompetent to stand trial, was not given necessary expert assistance and tests, was improperly found to be sane and to lack mental disorders that are a defense to the crime charged or provide a basis for a lesser sentence, is mentally unfit to be executed, and most importantly was incompetently represented by a lawyer with no

experience handling the special problems presented by a severely mentally disordered capital client.⁹⁰⁴

Many of these costs and bases for reversal arise in cases in which there is agreement among mental health professionals employed by the state as well as the defendant that the accused suffers from a well-recognized and medically accepted psychotic mental disorder, such as paranoid schizophrenia, with symptoms that strongly mitigate (*e.g.*, reduced culpability, and a susceptibility to treatment and rehabilitation). The reason high costs begin, rather than end, at this point is that, under current law, even uncontradicted proof of this mental condition is not a defense to a capital conviction or sentence. Instead, it triggers numerous intricate bodies of legal rules stating when, despite the defendant's severe and well-recognized condition, prosecutors may nonetheless bring capital charges against him; when trial judges may nonetheless deem the defendant competent to have submitted to certain investigatory procedures and to be brought to trial; when jurors may nonetheless convict him and sentence him to die; and when the state, thereafter, may conclude that he is sufficiently competent to be executed.

A consensus on the existence of a psychosis by panels of mental health professionals thus simultaneously (1) identifies the small minority of potentially capital cases in which there is no doubt that a true and inherently mitigating mental disorder is present; (2) distinguishes the large majority of cases in which any mental problems that may exist do not rise to the level of a clearly proven psychosis; and (3) identifies the case as one bound to be extremely expensive—requiring the employment and compensation both of medical or psychiatric specialists and a specially trained capital defense lawyer—and as a prime candidate for lengthy review, probable reversal and a costly retrial. **States or counties that bar capital prosecutions when there is clear proof of psychosis**

or other severe mental disorders thus stand to avoid many of the worst capital costs and risks of serious, reversible error.

d. Make life imprisonment without parole an alternative to the death penalty and clearly inform sentencing juries of that option.

Recently, respected criminologist and death penalty advocate James Q. Wilson argued that capital juries be given life-without-parole options to the death penalty, so that “jurors who may have some doubts about the strength of the evidence or some other plausible worry [may] hedge their bets [by sentencing the defendant to life without parole] if they are so inclined.”⁹⁰⁵ Former federal judge and FBI Director William Sessions also recently proposed that jurors be “able to impose sentences, short of death, that they believe will protect society from the criminal.”⁹⁰⁶ Providing support for these views, analyses show that (1) jurors are capable of identifying offenders for whom the death penalty is not warranted as long as there are strong assurances that the offender will remain in prison until he is no longer a threat to society, but that (2) jurors usually will *not* impose life verdicts in such cases—even though they believe the death penalty is not required—unless they are assured by the trial judge that the defendant will not be eligible for parole.⁹⁰⁷

These findings identify two steps that together can effectively discourage death verdicts in cases where our analyses show the risk of capital error is especially high because the case is not “the worst of the worst”: (1) adopt life without possibility of parole as an alternative to the death penalty, and (2) require judges to clearly inform jurors of that option. Our analyses predict that these steps will be associated with lower capital error rates for two reasons: They promote lower capital-sentencing rates by excluding marginal cases where jurors believe a lesser sentence will suffice, and they increase incarceration rates for murder which is itself a condition

associated with lower capital error rates.⁹⁰⁸

e. Abolish judge overrides of jury life verdicts.

If there is a single capital policy that most enhances the risk of serious capital error identified by our regression analyses, it is the authority four states—Alabama, Delaware, Florida and Indiana—give trial judges, after jurors vote to impose a *life* sentence, to override that decision and impose *death*.⁹⁰⁹ With the exception of Indiana, these states, not surprisingly, are in the top rank in terms of the number of death verdicts per 1000 homicides they impose.⁹¹⁰ Overrides are especially risky in Florida and Indiana because the judges who decide whether to override jury verdicts are subject to popular election. They are *most* especially risky in Alabama, which not only places particularly heavy political pressure on its elected judges, but also gives them total discretion to override jury verdicts for any reason, without explanation.⁹¹¹

Jury verdicts in capital cases allow 12 citizens to express the community's judgment on whether the death penalty is appropriate for the offense.⁹¹² When jurors hear all the evidence of aggravating and mitigating factors and vote to impose a life sentence, they identify the case as, by definition, a weak candidate for the death penalty. A jury override policy thus gives prosecutors two chances to convince a sentencer to impose a death sentence in inherently weak cases. The policy also puts political pressure on elected judges to substitute their judgment for that of jurors who represent community values at least as well as the judge but are not politically vulnerable. **Because jury overrides are an explicit policy of imposing additional death sentences in what by definition are weak cases, and because they are susceptible to political pressures on judges whose re-election prospects are tied to their override records,⁹¹³ the resulting death verdicts fall simultaneously in several categories in which the risk of serious capital error is the greatest.**

This conclusion is supported by findings of other researchers that:

- When placed in the hands of politically vulnerable judges, a mechanism that originally was expected to afford capital defendants “a second chance for *life* with the trial judge,”⁹¹⁴ is far more frequently used to impose death verdicts in cases where the conscience of the community has found the evidence too weak to justify that penalty;⁹¹⁵
- The more political pressure a state’s judicial selection method and other conditions place on judges, the more likely it is that judges will exercise their override power to impose death sentences in cases where juries believed death was not warranted.⁹¹⁶
- In some states, there is evidence that death verdicts imposed by judges in cases where the jury voted for life are especially likely to be overturned on appeal.⁹¹⁷

Whatever one might think about reforms keeping jurors from imposing death sentences they otherwise might have imposed, there is almost nothing to be said for a policy that routinely generates death sentences in cases in which a majority of jurors have voted *against* the penalty. **More so than is true of any other reform, our study findings support an end to judge overrides of jury verdicts for life.**

f. Use comparative review of murder sentences to identify what counts as “the worst of the worst” in the state, and to overturn outlying death verdicts.

Our findings indicate that over-broad capital charging, convicting and sentencing policies force capital appellate judges to function as substitute capital sentencers to winnow the many capital verdicts imposed at trial down to the few death verdicts the evidence and aggravating circumstances clearly warrant.⁹¹⁸ A problem with relying on appellate judges to perform this task is that they for the most part are limited to reversing cases in which there not only is an improper bottom-line outcome—a death verdict the evidence, circumstances or law do not permit—but also some *procedural* error that is banned by state or federal law. Absent a procedural error, appellate courts usually are not authorized to cure even very serious mistakes in the bottom-line outcome of the

trial.⁹¹⁹

Most state statutes, however, invite state direct appeal judges to conduct one type of review of the bottom-line validity of the capital outcome—by comparing cases in which death verdicts are imposed in the state to others in which they are not imposed to make sure that the death penalty is being used consistently across the state. Regrettably, almost no state appellate courts attempt to assure sentencing consistency in like cases by comparing murder cases in which the death penalty is imposed to those in which it is not imposed.⁹²⁰ Our analyses suggest that **by neglecting comparative review, state high courts surrender an important opportunity to identify what prosecutors and juries in the state consider to be *core* capital murders—cases in which the evidence is strong enough and the offense aggravated enough that death nearly always is imposed—and to distinguish “outlier” verdicts that are imposed for offenses the state’s prosecutors and juries do not consistently treat as warranting a death verdict.** Rigorously identifying core capital cases and reversing exceptional uses of the penalty narrows the risk of error identified by our analyses in three ways:

- Conscientious comparative review derives community death-sentencing standards from the strong trend over time of charging decisions by the state’s prosecutors and guilt-innocence and sentencing decisions by the state’s juries. Across cases and over time, those decisions can more narrowly identify the quality of evidence and level of aggravation that is generally thought to warrant capital treatment in the state.
- Comparative review can moderate high death-sentencing rates, and the high error rates associated with them, by quickly screening out the weak cases in which serious error is most likely and by advising prosecutors and jurors against future sentences in similar cases.
- The standards the court sets give prosecutors and trial judges a mandate and a means to resist political pressures to over-charge and to conform their decisions to popular sentiment, further lowering the risk of error that those pressures otherwise create.

* * * * *

The six options discussed above are **policies and standards designed to focus capital charges and verdicts on classes of cases in which the evidence of guilt and the amount of aggravation net of mitigation is clear and strong, and to exclude marginal categories of cases in which the risk of error is high.** These reforms would:

- moderate high capital-sentencing rates that are associated with high rates of serious error;
- help shield prosecutors and judges from political and race-related pressures that lead to overuse of the death penalty and resulting error and risk;
- increase attention paid to—and in some cases directly improve the quality of—the jurisdiction’s non-capital response to crime, the ineffectiveness of which is also related to high rates of capital error; and
- focus on capital *policy*—the guidelines defining the broad categories of cases in which the state or county invites capital charges, convictions and sentences, which are the kinds of official activities our analyses most strongly link to capital error.

As is noted above, however, the political pressures to overuse the death penalty that our analyses link to high capital error rates may keep policy makers from adopting these changes in an effective form or from sustaining them once they are adopted⁹²¹—allowing chronically high error rates to persist or to reappear, and keeping the system from achieving its goals. The four options set out below take a different approach. Instead of placing entire sets of marginal cases off limits to capital outcomes, these proposals aim to **improve the quality of decisions in each case by prosecutors, judges, jurors and defense lawyers, so that they can more reliably separate marginal from core candidates for capital verdicts.**

Each of the options described below aims to increase the capacity of particular actors in the death penalty process to serve as a check on excessive capital-charging and capital-sentencing policies and practices that our analyses so strongly associate with a high risk of serious capital error.

Some of these options might make capital trials last a few days longer and cost more. Our study findings reveal, however, that increased funding at the front-end of the capital process will more than pay for itself through reduced costs at the back-end of the process and the quelling of doubts about the integrity of the system. This point was succinctly made recently by Indiana University Law Professor Henry C. Karlson, “a staunch supporter of capital punishment” and frequent advisor to state lawyers defending capital cases: **“We spend very little money on trial, then spend a great deal on appeals,”** Karlson said. **“That’s idiocy. I say do it right the first time and you won’t need 20 years to figure out if anything went wrong.”**⁹²²

g. Base charging decisions in potentially capital cases on full and informed deliberations.

As various observers have concluded, spur-of-the-moment capital-charging decisions by single prosecutors before all the evidence is available can often commit jurisdictions to capital prosecutions in cases where the evidence of guilt is not strong, or evident aggravating circumstances are substantially offset by later-discovered extenuating factors.⁹²³ Once a case is charged capital, substituting non-capital charges, or the jury’s imposition of a non-capital conviction or sentence, may be seen as a defeat for law enforcement—even when that outcome is the appropriate one, given the evidence, circumstances and law. Over-charging of this sort in turn puts strong pressures on officials to cut corners and overstep bounds to avoid defeat, and to secure a capital conviction and sentence notwithstanding the weak evidence and aggravation or the strong mitigation.⁹²⁴

Capital statutes adopted recently by Congress and New York, and local practices in places like Austin, Texas, and Jacksonville, Florida,⁹²⁵ have identified a useful way to limit this problem. In those jurisdictions, **a decision to proceed capital in cases in which murder charges have**

been filed may not be made until three things have occurred:

- **police and prosecutors have completed their own investigation into the offense, to determine the strength of the evidence of guilt and the balance of aggravating and mitigating factors;**
- **defense lawyers, following their own investigation, have been invited to meet with prosecutors, review the state’s and their own evidence, and explain why capital charges are not warranted; and**
- **multiple individuals associated with the prosecuting office—some responsible for investigating the case, others not, some able to compare the case to similar situations where final verdicts are known, and all aiming to identify and follow local standards for limiting capital charges to “the worst of the worst”—conclude, based on all the evidence and information, that capital charges are warranted.**

These proposals track the advice of Joe Birkett, President of the Association of Government Attorneys in Capital Litigation and a Chicago-area prosecutor. Birkett reportedly recommended (1) that prosecutors “[e]liminate knee-jerk [charging] decisions” by using “written policies for deciding whether to seek the death penalty in murder cases” and “capital-case committees,” or by “consult[ing] with more experienced prosecutors elsewhere,” and (2) that “[b]efore deciding whether to seek the death penalty, prosecutors should [invite] defense attorneys to submit mitigation packets—information on a defendant’s mental state and upbringing that could evoke sympathy at trial.”⁹²⁶ In keeping with our study findings, these steps can:

- replace the indiscriminate capital-charging policies that our study strongly associates with increases in capital error;
- target capital charges on the strongest cases for a death verdict where serious error is least likely;
- help local professionals use their own standards—and any standards in state statutes or developed during proportionality review by state high courts—to resist the pressures to over-use the death penalty that also are related to high error rates; and
- foster improved law-enforcement, which is associated with lower capital error rates.

h. Make all police and prosecution evidence bearing on guilt vs. innocence, and aggravation vs. mitigation, available for presentation at trial.

The best single source of information on the strength of the evidence of guilt and the amount of aggravation net of mitigation in any given case is the police and prosecution file in the case.⁹²⁷

Often, however, potentially important evidence in the file never reaches the jury:

- **The failure of police and prosecutors to disclose evidence of innocence and mitigation is the second or third leading reason state post-conviction and federal habeas judges overturn capital verdicts.**⁹²⁸
- **The failure of police and prosecutors to disclose evidence before trial is one of the main reasons post-trial litigation over the reliability of capital verdicts takes so long—now averaging 12 years from death sentence to execution.**⁹²⁹
- **Prosecutors' charging decisions are more likely to ignore the weakness of the evidence of guilt and aggravation, and the strength of mitigation, when the rules allow them to shield evidence of problems with their case from public view.**

One reason official suppression of important evidence is common before trial, and the subject of such costly and contentious litigation on appeal, is that the legal rule stating when police and prosecutors must turn over evidence is ambiguous and difficult to apply. Under that rule, the decision whether a police officer or prosecutor must turn over evidence indicating that the defendant may be innocent turns on a police officer's or a prosecutor's guess about how the evidence might or might not change events at a trial that has not yet occurred.⁹³⁰ To avoid the problems such guesswork creates, a number of capital prosecutors around the country—including most federal capital prosecutors—follow an **“open files” policy making all the evidence in their and law enforcement files available to defense lawyers, who then decide whether there is anything in the file to present to the jury as evidence of the weakness of the state's case or in support of a defense.**

Our analyses reveal that it is in close cases—those in which a small amount of evidence

might tip the outcome in a different direction—that the risk of serious error is the greatest.⁹³¹ And yet under existing rules, it is in just those cases that officials are especially likely to conclude that disclosure of the seemingly small amounts of exculpatory evidence in their files is not required because the officials do not believe (as existing rules require before disclosure is mandatory) that the evidence would probably change the outcome of trial.⁹³² Our findings suggest that **any jurisdiction that relies on fully informed and responsible capital juries or judges to identify the “worst of the worst” cases, and to screen out the rest at the conviction and sentencing stages, and yet that does not insist that those decisions be informed by all the available evidence, takes an intolerable risk of over-conviction, over-sentencing and serious error, including execution of the innocent.** Because doing so also discourages prosecutors from making hard-headed evaluations of the true strength of the evidence of guilt and aggravation when they charge cases capitally, keeps defense lawyers from doing their jobs properly at trial, and lengthens appeals, **open-files policies in capital cases are, in our view, a second of three policy imperatives.**⁹³³

i. Insulate capital-sentencing and reviewing judges from political pressure.

Our analyses show a clear and consistent connection between political pressures elections put on state judges and higher rates of serious capital error.⁹³⁴ In making this finding, we rated states based on a variety of selection techniques, each of which places additional political pressure on state judges and all of which together are associated with higher capital error rates. Other findings also reveal political pressure on elected state *appellate* judges to affirm seriously flawed death verdicts.⁹³⁵ **Given these findings, each change listed below would decrease the risk of serious trial error and increase the likelihood that state appellate judges will correct such error when it occurs:**

- **Appoint rather than elect capital trial and appeals judges.**
- **Lengthen those judges' terms, whether they are appointed or elected.**
- **If judges are elected, use non-partisan elections or recall or retention elections, rather than contested elections.**
- **If states do not wish to give up frequent, partisan election of judges in order to improve the reliability of capital verdicts, they should use jurors to determine the sentence in capital cases—a practice the Supreme Court has recently suggested might be constitutionally required.⁹³⁶**
 - j. **Identify, appoint and compensate capital defense counsel in ways that attract an adequate number of well-qualified lawyers to do the work.**

Our adversarial system relies almost exclusively on a single, case-by-case check on false and over-broad criminal charges, convictions and sentences—**the diligent testing of the state's case by a lawyer for the accused who is as skilled, well-funded and determined as the prosecution. If this check functions properly, it can help alleviate many of the conditions our analyses show are most strongly associated with serious capital error:**

- One of the most effective checks against convicting defendants who are innocent of a capital offense is a skilled and determined criminal defense lawyer with the financial support and investigative and expert help needed to reveal defects in the state's case and demonstrate the client's innocence.
- As is documented by a careful study of improved standards for appointing, compensating and providing support services to capital defense lawyers in Indiana and by preliminary data on Oklahoma, **the routine provision of qualified and adequately funded capital trial lawyers leads to sharp decreases in death-sentencing rates. And our findings link decreases in death sentencing to substantially lower capital error rates.⁹³⁷**
- As the Indiana study also documents, an assurance of qualified counsel **dissuades prosecutors from bringing capital charges in weak cases**, given the likelihood that the lawyer will identify weaknesses in the state's case, convince jurors to forgo convicting when the state's case is doubtful, demonstrate the inappropriateness of the death penalty for defendants with inherently extenuating conditions, and identify alternatives to a death sentence that jurors find sufficient to punish the defendant and protect society.

- In these ways, **qualified and adequately compensated counsel help assure that capital convictions and death sentences are only imposed when the probability of serious error is the lowest: *i.e.*, for highly aggravated killings where the evidence in support of the capital verdict is strong.**
- Skilled lawyers are also more likely to
 - insist that the states provide the necessary resources for investigations, DNA and other forensic analyses, psychiatric evaluations and other support services that are required to avoid serious error;
 - dissuade politically vulnerable judges from making the kinds of erroneous rulings that political, racial and other pressures otherwise tend to trigger; and
 - expose the weaknesses in law enforcement strategies that are associated with high rates of capital error—and expose the insufficiency of aggressive capital charging and sentencing as a stop-gap response to such weaknesses.

This crucial adversarial check on flawed capital trials that is provided by competent and adequately funded defense lawyers has broken down in many capital jurisdictions:

- Egregiously incompetent lawyering—the only kind for which reversal is permitted⁹³⁸— is responsible for about 40% of reversals at the state post-conviction phase of capital review and between a quarter and a third of the reversals at the federal habeas stage.⁹³⁹
- An important predictor of high error rates at the state direct appeal review stage is poorly funded courts.⁹⁴⁰ Inadequately funded state courts are in turn associated with poorly funded capital defense lawyers, who typically are compensated out of court budgets. Moreover, states that resist spending money on their criminal courts are also likely to skimp on criminal defense.
- The literature abounds with horror stories about the low quality of counsel in capital cases.⁹⁴¹
- Observers also have frequently linked low-quality lawyering both to higher rates of capital-sentencing⁹⁴² and to higher rates of serious error including the conviction and capital sentencing of the innocent.⁹⁴³

The main cause of the breakdown of the adversarial check in capital cases is a dangerous combination of very heavy demands on capital defense lawyers and very low compensation. **The amounts of lawyer time and expert and investigative resources needed for an adequate defense**

in a capital cases are high—many times those needed for the typical non-capital defense.⁹⁴⁴

Most estimates of the cost of a minimally adequate capital defense, or of the market rates that prevail for such cases, run from \$50,000 at the low end to \$250,000 or more in urban areas.⁹⁴⁵ Yet **the resources states make available to compensate capital lawyers and defray their expenses are often less than 10% of the going rate for a minimal defense—and frequently are pegged to the level deemed minimally adequate for working out a plea bargain in, for example, a common larceny case.**⁹⁴⁶ Some states cap lawyers' compensation for an entire capital trial at \$5000, or even \$1000.⁹⁴⁷ Other states put ceilings of \$1000 to \$5000 on funds available for investigators and expert assistance, even though an adequate defense, particularly in factually complex cases and ones with mental health issues, requires tens of thousands of dollars in support services.⁹⁴⁸

As Fordham University Law Professor Bruce Green points out, “[t]he paltry compensation provided to lawyers who are appointed to defend capital cases . . . discourages members of the private bar from developing an expertise in death penalty litigation.”⁹⁴⁹ As University of North Carolina Professor James Coleman has further documented, the heavy time demands that conscientious lawyers feel compelled to meet if they accept appointments in capital cases in return for minuscule compensation and reimbursement of expenses “almost inevitably mean that virtually the only lawyers who are willing to handle capital cases are inexperienced, ill-prepared and underfunded.”⁹⁵⁰ The result is that capital defendants are routinely represented by lawyers:

- with only a few years or months at the bar, no qualifications besides political-patronage connections to the judge who appointed them or a willingness to speed trials along to an inevitable conclusion,⁹⁵¹ histories of bar discipline,⁹⁵² drug and alcohol problems,⁹⁵³ difficulty staying awake during trial,⁹⁵⁴ and either no capital or even criminal law experience or (at the other extreme) back-breaking criminal caseloads,⁹⁵⁵
- who fail to conduct the necessary factual investigations and legal research,⁹⁵⁶ never seek

expert assistance, forgo obviously valid motions and objections, and overlook readily available and important evidence;⁹⁵⁷ and

- who as a result are no match for the “prosecution [whose] resources in death penalty cases,” according to a Coopers & Lybrand study of the costs of capital representation, often “seem[] unlimited.”⁹⁵⁸

As Northwestern University Law Professor Larry Marshall has concluded, “If a criminal trial is to be a fair search for the truth, then each side must be given relatively equal resources with which to investigate and present its version of the truth. This is almost never the case with capital trials.”⁹⁵⁹

Scores of state and federal court reversals summarized in Appendix C and D document these and other break-downs in the adversarial check on inaccurate and unreliable capital convictions. An example is the federal habeas reversal of Frederico Martinez Macias’s capital verdict. Macias was capitally charged with killing a man during a burglary in El Paso, Texas in 1983. Macias was implicated in the murders by a co-worker in whose yard some of the victims’ property was recovered. Although the co-worker was an ex-convict who admitted taking part in the burglary-murder and disposing of the stolen property, he escaped prosecution in return for testifying against Macias. Other testimony came from jailhouse informants who said Macias confessed the crime to them, and from a nine-year-old witness who claimed to have seen Macias washing blood off his hands. A jury convicted Macias and sentenced him to die. Following direct appeal, Macias came within two days of being executed before a stay of execution was granted and lawyers at the Washington, D.C. law firm of Skadden, Arps, Slate, Meagher & Flom agreed to represent him on a volunteer basis in 1988. After two more years of intensive investigations and state and federal court review, during parts of which the Skadden lawyers were assisted by an Oklahoma Law School professor, a federal district judge found that Macias’s trial attorney had provided ineffective

assistance of counsel at trial by failing to (1) conduct even a cursory investigation, (2) identify and call two alibi witnesses, (3) call eyewitnesses who had seen the co-worker and another suspicious man—not Macias—at the victim’s home, (4) challenge the 9-year-old’s questionable memory, (5) present other witnesses who could have rebutted the prosecutor’s case, and (6) conduct the rudimentary legal research needed to avoid a damaging trial mistake on a key evidentiary point. According to the district judge, “[t]he errors that occurred in this case are inherent in a system which paid attorneys such a meager amount.”⁹⁶⁰ In affirming the reversal, a conservative United States Court of Appeals panel agreed:

We are left with the firm conviction that Macias was denied his constitutional right to adequate counsel in a capital case in which actual innocence was a close question. *The state paid defense counsel \$11.84 per hour. Unfortunately, the justice system got only what it paid for. . . .* The judgment of conviction is vacated and Federico Martinez-Macias shall be released from custody if the State of Texas has not commenced a new trial within 120 days of our mandate.⁹⁶¹

Macias thereafter was freed when a grand jury found insufficient evidence to re-indict him.

Macias’s case illustrates the high costs to the system of poorly qualified and compensated capital trial counsel. Vastly offsetting the resources saved by funding only a short trial and a single lawyer at \$12 an hour are the costs to Macias himself of spending nearly a decade behind bars despite the absence of evidence against him; to the family of the murder victim, which waited the same amount of time for the adjudicated killer to be punished, only to learn that there was no reliable evidence against him and that others who probably committed the crime had never been arrested; to 10 state and federal judges who devoted many hours and resources to the case; to the army of lawyers who eventually came to Macias’s rescue; and to the public, which paid for those appeals and lawyers and whose safety to this day is imperiled by the prosecution’s failure to discover the

weakness of its case and bring the real culprit to justice.⁹⁶²

In jurisdictions relying on a case-by-case response to the problem of serious capital error and the consequent breakdown of the capital system, it is imperative that such responses include:

- **standards assuring that only well-qualified lawyers represent capital defendants;**
- **methods of appointing capital lawyers that avoid patronage considerations and rewards to financial contributors to judicial campaigns; and**
- **sufficient compensation and reimbursement for experts, investigators and other litigation necessities to trigger the formation of a stable and qualified capital defense bar.**

C. Changes Likely to Magnify the Problem of Serious Capital Error

In addition to suggesting reforms that can help alleviate chronically high rates of serious capital error, our study results identify changes in existing practice that will not likely reduce error, and may well make things worse. Four unproductive approaches are discussed below.

1. Doing nothing is not an effective response to chronically high error rates and may well let them get worse.

The death penalty system is broken. And forces are at work that seem to be making it worse. At the state direct appeal review stage—the only stage that reviews all capital verdicts, and the stage responsible for nearly 80% of all reversals during the 23-year study period—more recent verdicts were significantly more likely to be found seriously flawed than earlier verdicts after controlling for other factors.⁹⁶³ **There is no reliable evidence that the problem of chronically high error rates will resolve itself over time, absent meaningful reforms.**

2. Cutting back on review of capital verdicts may increase the ill-effects of chronic error and lead to more error.

As we note earlier, the main changes in capital practice since the study period ended are sharp cutbacks in the breadth of appellate review in places like Texas at the state post-conviction stage, and nationwide at the federal habeas stage.⁹⁶⁴ But, as New Mexico Governor and long-time death penalty supporter Gary Johnson recently said in withdrawing a proposal to limit capital appeals, this kill-the-messenger strategy is an invitation to disaster—a change, in Johnson’s words, that could “lead to innocent people being executed.”⁹⁶⁵ Although as we have seen, the review process does not effectively feed back the information needed to improve capital trials, often misses serious errors, and performs unevenly as a substitute sentencer,⁹⁶⁶ the review process nonetheless has come to serve a crucial role in screening out large numbers of unreliable death verdicts. **Indeed, appeals screened out more than 7 seriously flawed verdicts for every execution during the study period. Especially if capital error rates continue to occur at anything like the rates during the 23-year study period, the effect of limiting inspections for error almost inevitably will be to decrease the probability that serious errors will be corrected, and to increase the risk that innocent inmates will be executed.** Limiting inspections can even cause error rates to rise, by removing the only existing, if weak, deterrent to the conditions associated with error, such as capital over-charging and over-conviction, political and race-related pressures on capital officials to expand capital punishment in lieu of effective law enforcement strategies, political pressure on trial judges to tailor rulings to popular sentiment, and under-funding of state criminal courts.

3. Piecemeal additions to the list of qualifying aggravating circumstances may increase capital error rates.

Another common modification of capital statutes is to add new aggravating factors that allow the imposition of the death penalty. The temptation to do this is great in the aftermath of a heinous

crime to which no existing aggravating factor applies. But **long lists of statutory aggravating factors—an attribute, for example, of Illinois’s capital statute that some have linked to its high rates of serious capital error—establish just the kind of broad death-sentencing policy that are associated with high error rates.**⁹⁶⁷ New aggravating circumstances should be resisted on principle.

If consideration is given to them, any change should not proceed piecemeal, but should be part of an overall revamping of the statute that removes other, less appropriate circumstances at the same time. This option again tracks the advice of Joe Birkett, President of the Association of Government Attorneys in Capital Litigation, that “[m]any states list too many aggravators” in their statute and should “streamline” those lists.⁹⁶⁸ **Prime candidates for removal, given their tendency to vastly**

expand the reach of capital statutes and to sweep in cases that are not highly aggravated are:

- **catch-all aggravating circumstances** that are vague and apply to essentially all first-degree murders—*e.g.*, that the offense was “especially heinous,” “atrocious,” “horrible” or “depraved,” or “above the norm of first-degree murder”;
- **aggravating factors that simply duplicate the definition of murder** and thus do nothing to assure that killings for which the death penalty is available are *more* aggravated than most murders—*e.g.*, that a killing occurred in the course of a felony or was premeditated, which are prerequisites to virtually all first-degree murders; and
- **repetitious aggravating factors that treat the same fact as two different reasons to impose death**, inviting prosecutors and sentencing juries to inaccurately inflate the seriousness of the offense by double-counting a single aggravating trait—*e.g.*, that the murder was both “in the course of a robbery” and “for pecuniary gain,” or “the victim was a police officer” and the killing “avoided lawful arrest.”

4. Large-scale underwriting by the state of the costs of local capital prosecutions invites higher capital error rates.

The final category of reforms against which our results caution are ones that largely replace local with state financing of capital prosecutions. These kinds of reforms may have the best of intentions—improving the quality of capital prosecutions, or defraying retrial costs so localities can

avoid errors made at the first trial. But our analyses strongly link capital error to policies that reward local officials who use the death penalty broadly, while displacing the post-trial cost of the errors these policies trigger onto taxpayers across the state and nation.⁹⁶⁹ If, in addition, local officials and their constituents can avoid most of the costs of the initial trial—and, worse, of the retrials their errors require—they will have even less reason to use the penalty judiciously. Open-ended state subsidies also make it easier for local officials to give in to political and race-related pressures to use the penalty broadly—including to mask failings of the county’s other law enforcement strategies—amplifying capital error still further.

Reforms thus should not leave the existing situation intact, while shifting larger portions of the cost of capital prosecutions from local actors who decide when to use the penalty to taxpayers statewide who pay for the post-trial review process but have little control over local decisions to seek the death penalty. Instead, reforms should couple increased state funding with policies limiting the death penalty to highly aggravated cases (see the first six options in the preceding section) or require improved case-by-case procedures (see the last four options in the prior section) while sharing their costs with local jurisdictions.

IX. Conclusion

This Report picks up where our June 2000 report left off: The death penalty in this country is a broken system that is of rising concern to many Americans. The public places great demands on the death penalty and yet has become increasingly aware that, as currently imposed, the penalty is a costly failure that does not serve the purposes for which it was established and risks taking the lives of innocent people. (See Part I above.)

Our earlier report documented these costs and risks. It showed that serious error is widespread and chronic. This is true no matter how conservatively one counts the number of judicially reversible mistakes the death penalty system makes. A review of our methods in this Report shows that we defined serious mistakes cautiously and counted them so conservatively that we excluded a number of death verdicts imposed on people who were innocent.⁹⁷⁰ Even defined this narrowly, capital error rates were 50% or more in nearly all death-sentencing states and years. Because such error keeps death verdicts from being carried out,⁹⁷¹ this finding means that most states have *failure* rates above 50%. Nationally, the average failure rate is a nearly 70%; and capital verdicts in many states and counties fail at rates of 80%, 90% and even 100%. Over the 23-year course of our study from 1973 to 1995, barely 5% of the 5800 death verdicts that were imposed were carried out. During that period, the average time from death sentence to execution was 9 years. Today, given the exacting review needed to catch so much error, that delay averages 12 years. (See Parts III.A and B.)

Each one of the thousands of capital errors identified by state courts (which found 90% of the errors) and federal courts (which found the rest) is serious. This is true because each error stymies the execution of sentence at a cost of years of delay and hundreds of thousands or even

millions of dollars in litigation costs. But it is more fundamentally true because reversible error is, by its very nature, serious error. Especially given the strong pressures on reviewing judges to approve even admittedly flawed verdicts, and given the strong bias of the rules governing court review towards approving verdicts, reversible error:

- nearly always undermines the reliability of the verdict that the defendant committed a crime that was aggravated enough to warrant death as a punishment;
- often risks the execution of people who are innocent of the crime or at least of the death penalty; and
- always frustrates the demands and expectations of the public who adopted the death penalty, the taxpayers who pay for it and the victims who directly rely on it.

We have taken it as a research imperative, therefore, to identify the conditions and practices that are significantly linked to, and predict the occurrence of, serious capital error. (See Parts IIIC-E.)

The central object of this study is to discover information of use in answering two questions. Why is there so much error in capital cases? Can anything be done to solve the problem or at least to moderate the amount of serious error?

We use a two-part method for conducting this research. First, we design and carry out a single multiple regression analysis that makes the best use of our detailed data about factors that may predict where and when capital error rates are likely to be high. Then, we verify the reliability and results of this “best” analysis using a wide variety of alternative regression techniques, diagnostic tests for evaluating methods and results, categories of reversal rates being explained, and potentially explanatory factors operating at the state, county and case levels. (See Part IV.)

The bulk of this Report is a detailed presentation of the results of:

- our main multiple regression analysis of explanations for the higher and lower rates of serious capital error in each of the 34 death-sentencing states that were active during the 23-

year study period in each of the years in which they were active (see Parts VA, B, E);

- seven follow-up regression analyses of those same state reversal rates—including ones examining only reversals at the state direct appeal, state post-conviction and federal habeas stages of review—to make sure our results reflect actual relationships in the data and are not products of particular research methods (see Parts VA-E);
- 10 additional follow-up regression analyses of state-level and county-level explanations for different error rates in the 1002 active death-sentencing *counties* during each of the 23 years when they were active—including a study of counties in the three most active death-sentencing states during the study period (Florida, Georgia and Texas) (see Parts VIA-F);
- case studies comparing rates of serious error, and rates of sentencing innocent defendants to die, in high and low death-sentencing counties (see Part VI.G.);
- detailed case studies of four innocent individuals who were sentenced to die and whose capital verdicts were *approved* at all three review stages (see Part III.B.7.c); and
- a comprehensive case-level study of factors that predict reversals as opposed to affirmances of the 600 federal habeas verdicts that were fully reviewed during the period (see Part VII).

Based on these results we reach several overarching conclusions about conditions that predict the existence and high rates of serious capital error (see Part VIII):

- Studying the problem of serious capital error using statistical and other techniques identifies a number of factors that predict high numbers or rates of capital reversals and are:
 - *statistically significant*, meaning there is only a small probability that they are the result of chance, as opposed to actual relationships between capital error and the identified explanations for it;
 - *reliable* in that they satisfy a number of diagnostic tests in most cases;
 - *linked to sizeable differences in predicted rates of serious capital error*, because, holding other factors constant at their averages, a fairly small change in a explanatory condition is associated with a fairly large increase or decrease in the amount or rates of serious error;
 - each part of a *strong and coherent overarching explanation* for serious capital error.
- For the most part, the conditions our analyses link to sizeable differences in rates and amounts of serious capital error are *capital-sentencing policies*—how often, in response to

what pressures, and in what broad classes or categories of cases, are death sentences sought and imposed—not traits of particular officials, jurors, lawyers, defendants or victims.

- **The principal conclusion of all of our analyses is that heavy use of the death penalty, especially when it sweeps in cases where the evidence supporting a capital verdict is not substantial, is a leading predictor of serious capital error.** States and counties that use the death penalty more often per 1000 homicides are significantly more likely to have substantially higher rates of serious capital error than other jurisdictions. In particular, cases with low levels of aggravation that are swept into the capital category by jurisdictions' broad capital-sentencing policies and low capital-sentencing thresholds are prime candidates for serious, reversible error. Heavy use of the death penalty also leads to court congestion and delay in processing capital appeals.
- Four other conditions strongly predict high rates of serious capital error. Each is either a measure of fears about serious crime, or a mechanism through which those fears can generate political pressure on officials to respond forcefully to crime, including through increased use of the death penalty. Some of those fears are based on actual crime and punishment rates. Others, more disturbingly, are sensitive to politics and race. We conclude that the tendency of all four conditions to heighten pressure to use the death penalty helps explain their link to high rates and amounts of serious capital error. The four conditions are:
 - the homicide threat to politically influential communities—measured by comparing the rates at which whites and blacks are victimized by homicides;
 - well-founded doubts about the ability of state law enforcement policies and officials to respond effectively to the problem of serious crime—measured by the rate at which serious criminals are apprehended, convicted and incarcerated;
 - state judges' susceptibility to negative political consequences if they do not conform their rulings in capital cases to popular sentiments—measured by the extent to which judicial selection techniques place state judges at risk of political discipline for unpopular rulings; and
 - the size of African-American and poor communities, which some influential citizens and officials evidently associate with higher rates of serious crime.
- Underfunded and overburdened court systems—another consequence in part of high death-sentencing rates—also increase the risk of serious capital error.
- Reviewing courts do not effectively keep serious errors from occurring or keep all unreliable death verdicts from being carried out.
 - The review process fails totally to prevent serious error from recurring.

- It does not catch all, including some of the most serious, mistakes.
- As a result, the probability that innocent people have been executed is high.
- There is no reliable evidence that the conditions causing serious, reversible error have improved over time, and strong evidence that some of those conditions have gotten worse.

Having identified these death-sentencing policies that predict serious, reversible error in capital cases—and the political, economic and racial pressures that generate those policies—we next consider the reform options they suggest for addressing the chronically exorbitant amounts and rates of that error that have characterized the capital system for decades (Part VIII).

It is unlikely that policy changes can do more than moderate the problem of chronically high rates and amounts of serious capital error, the ill effects of error on the effective functioning of the death penalty system and the risk error creates of executing the innocent. This is because the same state and local policy makers who developed the aggressive death-sentencing thresholds and practices that so strongly predict serious error would have to be relied upon to adopt and maintain effectively ameliorative policies. And it is also because those policy makers will continue to face the same or growing fears about serious criminal behavior, and the same financial constraints and racially sensitive political pressures, that led them to adopt the risky policies in the first place.

In some states and counties, the costs and frustration levels associated with the death penalty may be so high that only a comprehensive solution to the problem of chronic capital error and its attendant costs and risks will suffice. In those places, the available options are to stop using the death penalty altogether, or to limit its use to a small number of offenses that are so highly aggravated that there is close to a social consensus that only the death penalty will serve.

For jurisdictions that prefer to explore more incremental solutions, at least in the short run,

our study findings suggest **10 policy options for moderating serious capital error:**

- requiring proof beyond any doubt that the defendant committed the capital crime;
- requiring that aggravating factors substantially outweigh mitigating ones before a death sentence may be imposed;
- barring the death penalty for defendants with inherently extenuating conditions—mentally retarded persons, juveniles, severely mentally ill defendants;
- making life imprisonment without parole an alternative to the death penalty and clearly informing juries of the option;
- abolishing judge overrides of jury verdicts imposing life sentences;
- using comparative review of murder sentences to identify what counts as “the worst of the worst” in the state, and overturning outlying death verdicts;
- basing charging decisions in potentially capital cases on full and informed deliberations;
- making all police and prosecution evidence bearing on guilt vs. innocence, and on aggravation vs. mitigation available to the jury at trial;
- insulating capital-sentencing and appellate judges from political pressure; and
- identifying, appointing and compensating capital defense counsel in ways that attract an adequate number of well-qualified lawyers to do the work.

Approaches that would likely magnify the amount of serious error are:

- cutting back further on the scope of review of capital verdicts, which would likely increase the ill-effects of chronic error and invite more error;
- making piecemeal additions to the list of qualifying aggravating circumstances;
- shifting to the state the full costs of local capital prosecutions; and, most importantly
- doing nothing.

The very last point is the most important one. **Over decades and across dozens of states, large numbers and proportions of capital verdicts have been reversed because of serious error. The capital system is collapsing under the weight of that error, and the risk of executing the innocent is high. Now that explanations for the problem have been identified, and a range of options for responding to it are available, the time has come to fix the death penalty, or end it.**

STRICTLY EMBARGOED TO MONDAY FEBRUARY 11, 2002, 12:01 AM (ET)

A Broken System, Part II:

**Why There Is So Much Error in
Capital Cases, and
What Can Be Done About It**

END NOTES

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February 11, 2002

END NOTES

1. See, e.g., John Harwood, *Despite the McVeigh Case, Curbs on Executions Are Gaining Support*, Wall St. J., May 22, 2001, at 1 (lead story):

[P]aradoxically, the dawn of George W. Bush's presidency is bringing a swing in the pendulum away from executions in America. Though most Americans continue to back capital punishment, support has been dropping in recent years in tandem with declining rates of violent crime. Advances in DNA testing and scandals involving the prosecution of major offenses have underscored the fallibility of evidence in capital cases.

* * * * *

Recent allegations of misleading testimony by an Oklahoma police chemist who served as a frequent prosecution witness, as well as the FBI's mishandling of records in the McVeigh case, are only adding to pressure for better safeguards.

* * * * *

"The politics of the death penalty are clearly changing . . . because of the blunders of the system," says Oklahoma Gov. Frank Keating. Though he staunchly supports capital punishment, the conservative Republican says he favors establishing a higher standard of proof in capital cases, even if that makes death sentences more difficult to obtain. Just five years ago, such a change was unthinkable. But it reflects a broader reconsideration taking place across the spectrum of . . . issues.

Accord Tom Brune, *Nation Examines Death Penalty: Amid Capital Punishment Concerns, Federal, State Executions Declining*, *Newsday*, June 10, 2001, at A5; Laurie Goodstein, *Death Penalty Falls From Favor as Some Lose Confidence in its Fairness*, *N.Y. Times*, June 17, 2001; Thomas Healy, *Death Penalty Support Drops as Debate Shifts: Foes Turning Focus from Moral Issues to Flaws in the System*, *Balt. Sun*, July 25, 2001; Kenneth Jost, *Rethinking the Death Penalty*, *Cong. Q. Res.*, Nov. 16, 2001; Eric Lichtblau, *Death Penalty Reforms Gather New Momentum: With Dozens of Death Row Inmates Freed, a Cry Rises for Precautions, Such as DNA Tests*, *L.A. Times*, June 26, 2001; Toni Loci, *Push to Reform Death Penalty Growing; Mistakes Could Shake Confidence in System*, *USA Today*, Feb. 20, 2001; Emilie Lounsberry, *Death Penalty's Fairness Debated Nationwide*, *Phil. Inq.*, June 8, 2001; Richard Willing, *Support for the Death Penalty Might Be Declining*, *USA Today*, Dec. 14-16, 2001.

Reexamination of the death penalty has continued since the events of September 11, 2001. In addition to Kenneth Jost's November 2001 in-depth study of the topic in the *Congressional Quarterly Researcher* (Jost, supra), Richard Willing's article in *USA Today*, cited above, and Taylor Bright's five-part series in the *Birmingham Post-Herald*, *Is Fairness Missing from the State's Use of Capital Punishment?* (Dec. 2-7, 2001), see Patrik Jonsson, *Governors Soften on Death Penalty*, *The Christian Science Monitor*, Oct. 23, 2001; *Indiana's Other Lottery: Death Penalty*, Multi-part series published in five Indiana newspapers during the week of October 22, 2001; other recent articles cited infra notes 2 & 84 (continuing death penalty debate in Oklahoma prompted in part by a pattern of faulty forensic tests by state experts), 7 (legislative testimony and debate in Nebraska following issuance of a comprehensive study there calling for limits on use of death penalty in the state), 14 (ongoing study of death penalty in Nevada), 23 & 37 (increasing legislative support for moratorium on executions while system's fairness is studied; Kentucky governor's recent endorsement of ban on juvenile executions; surge in court reversals of Kentucky capital verdicts), 27 (progress of legislative proposal to abolish death penalty in Oregon), 30 (discussing study issued by Virginia's Joint Legislative Audit and Review Commission in December 2001 finding "significant

inconsistencies' in Virginia's use of the death penalty" and that overly strict review procedures "may have let stand the convictions of some death row inmates who did not receive fair trials"; reform proposal following study in Indiana), 34 (discussing late Fall 2001 ruling banning execution of mentally retarded defendants in Tennessee), 35 (*Chicago Tribune's* December 16-19 series on false confessions in capital and other cases), 43 (discussing strong bipartisan support among gubernatorial candidates in Illinois for maintaining the state's moratorium on executions), 44 (Fall 2001 statements by New Mexico Governor Gary Johnson, a long-time death penalty supporter, that "eliminating the death penalty in the future may prove to be better public policy given the reality of the sentence today," followed by his withdrawal of proposed legislation to shorten the review process in capital cases, which he now opposes because it might permit innocent prisoners to be executed), 69 (speech by Supreme Court Justice Sandra Day O'Connor to Nebraska Bar Association reiterating her view that the death penalty as currently administered risks executing the innocent), 73 (recent successes in California and Nebraska of ongoing campaigns to secure moratorium resolutions from local governments calling on state officials to halt executions while studies occur), 75 (decline in number of executions, continuing through end of 2001), 894 (December 29, 2001 decision of the Kansas Supreme Court limiting the application of the state's death penalty to highly aggravated cases and requiring new sentencing hearings for all of the state's death row inmates), 968 (decision of the Illinois House of Representatives Speaker to forgo attempt to override the governor's veto of bill expanding number of aggravating circumstances in Illinois's capital statute).

See also Dion Nissenbaum, Riordan Outlines Education Plan, Touches on Death Penalty, *Jose Mercury News*, Jan. 24, 2002 ("Republican Richard Riordan [who is running for governor in California] waded into two touchy issues Wednesday, telling a Silicon Valley crowd that as governor he might try to eliminate the state's elected schools chief and would be open to halting executions if evidence emerged that they were being unfairly imposed."); Michael Saul, Mayor-Elect Mike Wants To Be a Major GOP Player, *N.Y. Daily News*, Nov. 12, 2001 (Mayor-elect Michael Bloomberg has a message for his fellow Republicans: When it comes to certain hot-button issues . . . you're dead wrong. . . . 'I am . . . against the death penalty, as are most people in New York City,' he said. . . . Bloomberg . . . said he 'absolutely' plans to use his new position to help shape the Republican Party agenda."); Peter Whoriskey, Kaine Edges Out Katzen For State's No. 2 Office, *Wash. Post*, Nov. 7, 2001 (discussing election of Democrat Timothy M. Kaine as lieutenant governor of Virginia despite his opposition to the death penalty and support for a moratorium on executions, and despite the longstanding political wisdom that staunch support for the penalty is a prerequisite for statewide office in Virginia).

Note: This Report's discussion of recent events is current to November 30, 2001. Occasional references are made to matters that came to our attention after that date, but the demands of the production process have prevented us from comprehensively canvassing events after November 30.

2. See Editorial, *Assuring Justice*, *Columbus Dispatch*, July 29, 2001:

Americans disagree passionately about the death penalty, but those on both sides of the issue should be able to find common ground on this one concept:

Innocent people ought not be executed.

This is the premise behind the Innocence Protection Act, a proposed federal law that would require action to help ensure that defendants convicted of capital murder are, in fact, guilty. The bill would make DNA testing more available to condemned inmates and would set minimum standards for defense lawyers representing indigents.

See also Goodstein, *supra* note 1 ("After a decade in which there appeared to be an unshakable near

consensus in favor of the death penalty, Americans say they are now rethinking and debating capital punishment Interviews in six states this week reflect the poll numbers, which show that while there is still a majority in favor of the death penalty, the size of the majority is shrinking. While many people cited the biblical command to take ‘an eye for an eye’ . . . others said they had recently changed their minds after concluding that the death penalty was administered unfairly. Some said that what persuaded them was the news that 13 prisoners on death row in Illinois were discovered to be innocent—a revelation that led Gov. George Ryan, a Republican, to declare a statewide moratorium on the death penalty last year. Others said they were troubled by reports that the death penalty may be disproportionately imposed on blacks and Hispanics.”).

A major focus of concern is Oklahoma, where faulty forensics work may have compromised a number of death row cases, including those of some inmates who have been executed. As the *Dallas Morning News* recently reported:

In a 21-year career, Oklahoma City police chemist Joyce Gilchrist was a prosecutor's dream: She delivered supportive lab analysis and convincing testimony that helped send hundreds to prison—at least 23 people to death row.

Ms. Gilchrist may turn out to be a prosecutor’s worst nightmare: So much of her work was questioned by appeals courts and forensics experts that she was suspended and fired. Investigators are digging through 1,197 of her cases to see whether anyone is behind bars because of false or misleading testimony.

And now—in a year when Oklahoma leads the nation in carrying out the death penalty, and with suspect convictions being reviewed even beyond the Gilchrist cases—some are pondering the unthinkable: Has Oklahoma executed the innocent?

* * * * *

Separately, the Oklahoma City Police Department has asked the task force to review as many as 10 cases handled by a second former police chemist, the late Janice Davis. In one such case, Ms. Davis used hair and fiber analysis to link Dewey George Moore to a 1984 murder. The task force ordered DNA tests to determine whether Mr. Moore has wrongly spent 16 years on death row.

* * * * *

Meanwhile, the state task force reviewing Ms. Gilchrist’s files has ordered DNA testing on two death row cases: John Michael Hooker, who was convicted in the 1987 stabbing deaths of his girlfriend and her mother; and Michael Edward Hooper, convicted in the 1993 shooting deaths of his former girlfriend and her two young children.

Chemist’s Errors Stir Fear: Did Oklahoma Execute Innocent? Questions Force Experts to Dig Through Thousands of Cases, *Dallas Morning News*, Oct. 22, 2001. See also *infra* notes 50, 79, 84, 232 (discussing various aspects of the Gilchrist case).

Not everyone is equally concerned about the possibility of executing the innocent. For an argument that executing the innocent is an acceptable collateral effect of the existing death penalty, see Ernst van den Haag, *In Defense of the Death Penalty: A Practical and Moral Analysis*, in Hugo Adam Bedau, *The Death Penalty in America* 325 (3d ed. 1982). See also ABC, *Innocence and the Death Penalty*, Nightline, July 14, 1997 (transcript), quoted in part in Jackie Cooperman, ABCNEWS.com, July 15, 1997,

<http://more.absnews.go.com/sections/us/death715/index.html> (statement of Rep. Bill McCollum, R-Fla.):

I don't think there's any question that someday somebody who is innocent will be executed in this country. It's happened before. . . . [O]n the whole, considering the total number of murders there are in this country, the total number of people who are on death row . . . , the interest of the public and the victims in seeing justice done, and what I believe is a deterrent effect for the death penalty with respect to those who commit premeditated murder, I think it is a relatively low risk and one that we have to accept. We try not to let it happen . . . but realize it probably will happen.

3. Brian Bakst, O'Connor Questions Death Penalty, Associated Press, July 2, 2001.

4. See, e.g., William Clairborne, Ill. Governor, Citing Errors, Will Block Executions: Wash. Post, Jan. 31, 2000, at A1; Steve Mills & Ken Armstrong, Gov. George Ryan Plans to Block the Execution of Any Death Row Inmates, Chi. Trib., Jan. 30, 2000.

5. See, e.g., Jo Thomas, New Death Penalty Rules Are Issued in Illinois, N.Y. Times, Jan. 23, 2001, at A17 (“The Supreme Court of Illinois has adopted new rules governing the way death penalty cases are handled. The rules . . . set requirements for training and experience for all defense lawyers and assistant prosecutors handling the cases[,] . . . require regular training for [capital] judges . . . , remind prosecutors that their duty is ‘to seek justice, not merely to convict,’ . . . require prosecutors not only to give defense lawyers any evidence that may tend to exonerate their client—like a statement that someone else committed the crime, or a scientific test result that is not incriminating—but also to identify clearly which information may be mitigating . . . [, and] specify that prosecutors must hand over any relevant information relating to DNA evidence, including reports explaining any discrepancies in the testing, observed defects or laboratory errors, the reasons for these errors and the effect of these mistakes.” (discussing Ill. R. Prof. Conduct 3.8; Sup. Ct. R. 43, 411, 412, 416, 417, 701, 714)).

6. See Ray Long & Maurice Possley, House Acts to Restrict Jailhouse Informants; Bills Also Target Prosecutor Abuses, Chi. Trib., Apr. 24, 2001 (“The Illinois House overwhelmingly approved a sweeping legislative package,” sponsored by former prosecutor and Republican state representative Jim Durkin and supported by the Illinois Association of Police Chiefs, which is “designed to crack down on wrongful convictions and misconduct by prosecutors. . . . The legislation would put strict limits on the testimony of jailhouse informants, whose goal of shortening their sentences by providing testimony for prosecutors often has prompted them to twist the truth at the expense of justice . . . [and] also would place a new burden on prosecutors who fail to turn over [exculpatory] evidence to the defense, . . . gives a person facing charges that carry a potential death sentence . . . the chance to challenge the veracity of witnesses by taking depositions before trial, . . . [and] would require pretrial reliability hearings before prosecutors would be allowed to put jailhouse informants on the witness stand.”).

7. See, e.g., See Ken Hambleton, Researcher: Limit Call for Executions, Lincoln J. Star, Oct. 18, 2001 (discussed *infra* p. 396 & n.886); Robynn Tysyer, Execution Suspension Approved: Senators Hand Johanns Life-and-Death-Decision, Omaha World-Herald, May 20, 1999, at 1; Robynn Tysyer, Death Penalty Study OK'd, Omaha World-Herald, May 28, 1999, at 1.

8. See The Justice Project, 2001 State Legislation on Death Penalty Reform at a Glimpse, July 19, 2001.

9. See Howard Libit, Data Hint at Race Bias: Researchers Advise Md. To Halt Executions Until Studies End, Baltimore Sun, Feb. 23, 2001.

10. See, e.g., Daniel LeDuc, Md. House Backs 2-Year Moratorium on Death Penalty, Wash. Post, Mar. 25, 2001, at C1; Daniel LeDuc & Lori Montgomery, Md. Bill to Suspend Executions Dies, Wash. Post, Apr. 10, 2001, at A1; Jost, supra note 1 (“In Maryland, supporters believed they had enough votes to win passage of a moratorium bill in the state Senate, but opponents blocked a vote in April with a filibuster as the legislature was about to adjourn.”).

11. See, e.g., Editorial, Maryland’s Execution Pause, Wash. Post, Apr. 15, 2001, at B6 (“The Maryland Court of Appeals on Thursday accomplished what the state’s legislature failed to do a few days earlier—put a temporary halt on executions in the state.”).

12. See Ed Vogel, Nevada Assembly Rejects Death Penalty Halt, Las Vegas Review-Journal, May 17, 2000.

13. See Ed Vogel, Bill to Protect Mentally Retarded Passes, Las Vegas Rev.-J., Apr. 25, 2001 (discussing Nevada Senate’s passage of bill banning execution of mentally retarded persons by a vote of 28-11).

14. See Eric Neff, Lawmakers Tackle Tough Issue of Death Penalty, Las Vegas Sun, Oct. 30, 2001 (“No issue dogged this year’s Legislature the way Nevada’s death penalty did. . . . On Monday an interim legislative committee began in the first of six meetings to tackle the task of recommending bills to the 2003 Legislature that will improve what many believe are inherent flaws in the way capital punishment is carried out in Nevada, which has the largest per capita death row population in the nation. . . . ‘I think it’s one of the most important topics nationally, as well as in the state, of our day,’ said Assemblywoman Sheila Leslie, D-Reno, who chairs the interim study committee.”); Cy Ryan, Death Penalty Foe to Head Panel: Legislators to Consider Future of Capital Punishment in Nevada, Sept. 7, 2001; Sean Whaley, Interim Study Sessions: Lawmakers Enter Research Season; Eight Topics Funded for In-Depth Study Evaluation in Preparation for 2003 Legislature, Las Vegas Rev.-J., July 23, 2001 (discussing death penalty study ordered by state legislature, which “will look at a variety of issues, including the use of DNA testing, the cost of implementing the death penalty as opposed to life imprisonment and whether people under age 18 or who are mentally retarded should be sentenced to death”).

15. See, e.g., Lounsberry, supra note 1.

16. See Christy Hoppe, Governor Enacts Criminal Justice Laws, Dallas Morning News, June 15, 2001.

17. See Raymond Bonner, Ban on Execution of the Retarded Is Vetoed in Texas: Exception to U.S. Trend, N.Y. Times, June 18, 2001, at A1 (lead story).

18. See Christy Hoppe, Texas Senate Approves ‘No Parole’ Option, Dallas Morning News, May 4, 2001.

19. Unless otherwise noted, these actions are documented in Death Penalty Information Center, Changes in the Death Penalty Around the U.S., <http://www.deathpenaltyinfo.org/Changes.html> (visited Oct. 27, 2001). See infra note 75 (regarding number of executions in 2000 and 2001). For a useful summary, see Christy Hoppe, A Shift in Scales of Justice: Besieged System Gets an Overhaul, Dallas Morning News, May 29, 2001, at A1:

The swaggering and cocky Texas justice system sat down for a reflective session and came away with something akin to a death-row conversion.

By Monday, gone from law-and-order legislators were their defense of sleeping lawyers and executing the mentally retarded. Silenced was the refrain that innocents never get the death penalty.

Instead, after the national spotlight of a presidential election, lawmakers accepted responsibility for their criminal justice policies and voted overwhelmingly to change them—although many of the same proposals had faced defeat time and again in earlier years.

“Sometimes a picture can say 1,000 words, and the picture of a sleeping lawyer defending someone who is facing the death penalty was so vivid in the minds of people in this country that it didn’t take a rocket scientist to figure out that that is not competent representation,” said Rep. Juan Hinojosa, D-McAllen.

* * * * *

[M]ost lawmakers hailed the bills as a major victory—perhaps the most far-reaching efforts of the legislative session—ending the status quo and starting toward a fairer system. “You know, this is a law-and-order state, and I think we made the point that you can be tough on crime and still be compassionate,” said Sen. Rodney Ellis, D-Houston, who has championed indigent defense reform and prohibiting the execution of the mentally retarded.

See also Steve Mills, *Texas Revisits Death Penalty: Legislators Weigh Reform in Nation’s Execution Leader*, Chi. Trib., March 25, 2001; John Moritz, *Texas May Be Yielding to Execution Criticisms*, Fort Worth Star-Telegram, Aug. 17, 2001 (“Texas hasn’t gone soft when it comes to the death penalty, but its leaders seem to be increasingly sensitive to criticism of the state’s use of capital punishment, several observers said a day after a court halted the execution of a man who killed at age 17.”); Jim Yardley, *Texas Retooling Criminal Justice in Wake of Furor on Death Penalty*, N.Y. Times, June 1, 2001, at A1 (lead story) (“Texas, which leads the nation in executions and endured withering criticism of its death penalty system during the presidential campaign last year, is poised to make significant changes in its criminal justice laws and so, supporters of the overhaul say, create a fairer system of capital punishment.”); Jim Yardley, *Of All Places: Texas Wavering on Death Penalty*, N.Y. Times, Aug. 19, 2001.

For an article discussing other legislation given serious consideration by Texas lawmakers, see Paul Duggan, *Texas Legislators Review Use of Death Penalty; National Criticism During Presidential Campaign Reverberates in a Spate of Bills*, Wash. Post, May 14, 2001, at A3: “Texas, which has executed more convicted murderers in the last two decades than most nations of the Western world, is considering a surprising array of capital punishment reforms that could reduce the number of death sentences imposed here, lawmakers said. After meeting resistance in the Texas House and Senate in past years, several bills aimed at tempering the capital punishment system have advanced this spring, [including]”:

- “a measure that would bar executions of convicted killers who are mentally retarded,” which died in a committee last year after then-Governor George W. Bush opposed it;
- a bill “that would prohibit capital punishment for defendants accused of committing murders before age 18,” which cleared a key House committee, and stands in stark contrast to the corresponding bill in the previous session, which would have *lowered* the minimum age to 16, “the youngest allowed by the U.S. Supreme Court”;
- a bill “designed to improve the quality of court-appointed defense lawyers in capital murder trials” by “set[ting] minimum professional standards for the lawyers and offer[ing] \$22 million in indigent-defense grants to counties that limit the role of judges in the attorney-selection process,” which contrasts with a “much weaker” bill that passed both houses two years ago but was vetoed by then-Governor Bush after local judges lobbied against its removal of their patronage power over

appointments;

- a “bill that would allow jurors in capital cases to impose sentences of life without parole . . . [which] could make a jury less inclined to vote for execution,” which “died in House and Senate committees in 1999 [but t]his year . . . has passed the Senate and won committee approval in the House” and received cautious support from the current governor; and
- even a bill calling for a referendum on a proposed execution moratorium, which cleared key committees in both houses of the legislature

For an influential view that the reforms Texas has adopted are not yet enough, see Editorial, *Death Row Decisions—Once Again, the System Is in Question*, *Dallas Morning News*, Aug. 15, 2001 (“[T]he glare of publicity [two recent cases in which Texas pursued plans to execute a juvenile offender and defended in court a capital sentence imposed on a defendant whose lawyer slept through much of the trial] they have attracted once again draws attention to the need to improve the justice system in Texas.”).

20. See Eric Frazier, *New Death Penalty Law Put in Action: Suspect in Murder Won’t Face Execution*, *Charlotte Observer*, May 25, 2001, at A1:

Mecklenburg prosecutors brought an important new capital punishment law into dramatic focus Thursday when they announced they won’t seek the death penalty against a man they nearly put on trial for his life days earlier.

The law, signed by Gov. Mike Easley last week, gives prosecutors more discretion in whether to seek the death penalty. Mecklenburg Assistant District Attorney Marsha Goodenow said her office plans to review its pending murder cases in light of the change.

* * * * *

The old law forced prosecutors to seek death when they found one or more of 11 aggravating circumstances in a murder case. . . .

Critics of the N.C. capital punishment system have pointed to the old law mandating death penalty trials as a reason for the state’s higher-than-average death sentencing rates.

21. Stephen Dinan, *Time Limit Lifted on DNA Testing for Felons*, *The Wash. Times*, May 3, 2001; see, e.g., Craig Timberg, *Time Limit Lifted for DNA Appeals*, *Wash. Post*, May 3, 2001 (“Gov. James S. Gilmore III approved a new right today for death row inmates, adding Virginia to the growing list of states that give the condemned access to DNA testing long after their convictions.”).

22. See Bill Blakemore, *Support for Death Penalty Drops*, *ABC World News Tonight*, May 2, 2001.

23. See Mark R. Chellgren, *Momentum May Be Growing Toward Moratorium on Capital Punishment*, *AP State & Local Wire*, Nov. 21, 2001 (discussing legislative support for 2-year moratorium on executions in Kentucky while state “studies whether capital punishment is fairly administered.”).

24. See *supra* p. 1.

25. See *supra* pp. 1-2.

26. See *supra* p. 2.

27. See *Changes in the Death Penalty Around the U.S.*, *supra* note 19; Jost, *supra* note 1 (“Opponents of the death penalty . . . came very close to abolishing capital punishment in two states [in 2001]—New Hampshire and New Mexico.”). On Oregon, see Mark L. Thompson, *Repeal of Death Penalty Sought*, *Salem Statesman J.*, Nov. 9, 2001 (“Capital punishment opponents will begin their campaign to repeal Oregon's death penalty next week”).

28. See *Editorial, Waning Penalty*, *Boston Globe*, March 14, 2001 (“The 32-vote margin in the Massachusetts House Monday against reinstating the death penalty—compared to the nine-vote difference in 1999— . . . signals a sea change in death-penalty politics”); Emmet Meara, *Maine Death Penalty Bill Denied*, *Bangor Daily News*, May 17, 2001. See also Jost, *supra* note 1 (“Death penalty bills failed to advance [in 2001] in two . . . other . . . states, Michigan and Minnesota.”).

29. See *Death Penalty Report Released*, *Associated Press*, Aug. 4, 2001 (“A state panel reviewing Arizona's use of the death penalty released an interim report that recommends additional resources for defense attorneys [*i.e.*, “a statewide public defender's office to represent defendants in death penalty cases”], commutation of sentences against the mentally ill, and a ban on executing [offenders for crimes committed as] minors.”).

30. See, e.g., *Associated Press State & Local Wire, Panel Recommends Change in State's Death Penalty Law*, Dec. 14, 2001 (recommendation by Indiana's Criminal Law Study Commission, which reviewed the state's death penalty at Governor Frank O'Bannon's request, that “Indiana's death penalty law should make it clear that only people who intended to kill someone should be executed”); *Associated Press, Revised Death Penalty Wins Final Legislative Approval*, June 7, 2001 (Connecticut “state lawmakers approved a bill that would . . . require a comprehensive study of how the state imposes the death penalty.”); Suzan Bibisi, *Death Penalty Bill Becomes Law*, *Waterbury Republican-American*, July 7, 2001 (noting that Governor Rowland of Connecticut signed legislation establishing a comprehensive study of Connecticut's death penalty system); Raymond Bonner, *Justice Dept. Set to Study Death Penalty in More Depth*, *N.Y. Times*, June 14, 2001 (“The Department said today that it would undertake a comprehensive study of the federal death penalty to determine whether the system is racially or ethnically biased.”); *Changes in the Death Penalty Around the U.S.*, *supra* note 19 (discussing studies in Arizona, Illinois, Indiana, Maryland, Nebraska, North Carolina and Virginia); Frank Green, *Study Critical of Death Penalty: Statute Audit Panel Cites ‘Significant Inconsistencies’*, *Richmond Times-Dispatch*, Dec. 11, 2001 (“A state study found ‘significant inconsistencies’ in Virginia's use of the death penalty and concluded that strict adherence to rules by federal and state appeals courts may have let stand the convictions of some death row inmates who did not receive fair trials. The study released yesterday by the Joint Legislative Audit and Review Commission also suggested the Virginia Supreme Court was overly strict in its use of state law in determining whether a death sentence was excessive or disproportionate. In all 119 death cases that have come before the state justices since the death penalty resumed in 1977, none was found excessive.”); Robert Schwaneberg, *AG Studies Disparities in Capital Punishment*, *Newark Star-Ledger*, Aug. 21, 2001 (“[New Jersey] Attorney General John Farmer Jr. has launched a review of standards that prosecutors use for seeking the death penalty, following a landmark study [issued by the New Jersey state supreme court] that found wide differences in how aggressively capital punishment is sought in the 21 counties. Farmer said he will head a panel of four former attorneys general and a former director of criminal justice to study why some counties are far more likely than others to seek the death penalty and whether the state should impose a more uniform approach to the way it is applied.”); *supra* notes 7, 12-14 & accompanying text (discussing Nebraska and Nevada studies).

31. Robynn Tysver, *Inmate-DNA Bill Gets 2nd-Round Approval*, Omaha World-Herald, May 10, 2001.

32. See, e.g., Akron Beacon J., Nov. 12, 2000 (Ohio); Diane Plumberg, *Gov Signs Tuition Hike, DNA Testing Funds Into Law*, The Oklahoman, http://www.oklahoman.com/cgi-bin/show_article?ID=689458&-pic=none&TP=getarticle (Oklahoma); *Changes in the Death Penalty Around the U.S.*, supra note 19. See also *Uniform Statute for Obtaining Post-Conviction DNA Testing, Unanimously Approved at the Ninth Meeting of the National Commission on the Future of DNA Evidence*, Apr. 9-10, 2000.

On some limitations of these statutes (particularly Florida's) that keep them from providing full protection to capital prisoners, see, e.g., Jagger, *Death Row Case Demands DNA Testing*, Miami Herald, Aug. 28, 2001.

33. See, e.g., *In re Adoption of a new subdivision 29.17, entitled, "Post-conviction Motion for Forensic Testing not Available at Trial," of Supreme Court Rule 29* (Mo. S. Ct. Feb. 20, 2001); Francis X. Clines, *Inmate Access to DNA Tests Gains Ground Across Nation*, N.Y. Times, Dec. 19, 2000 (discussing administrative order of Ohio Attorney General Betty D. Montgomery, requiring DNA testing of death row inmates who show that "a favorable [DNA test] result for the inmate [would] 'by itself' . . . produce exoneration"); *DNA Tests Okd for Texas Cons*, N.Y. Daily News, Apr. 6, 2001; *Legislators Reevaluate Death Penalty with New Initiatives; The Aims: Proper Representation for Those on Death Row and New Trials If DNA Evidence Shows a Need*, Phil. Inq., July 10, 2001; *Changes in the Death Penalty Around the U.S.*, supra note 19; supra pp. 2 & n.15 (discussing recent DNA legislation in Texas and Virginia).

34. See, e.g., Associated Press, *Revised Death Penalty Wins Final Legislative Approval*, June 7, 2001 ("After years of making changes to strengthen the death penalty, [Connecticut] lawmakers approved a bill that would exempt mentally retarded people from execution, remove one category of crime from qualifying for capital punishment and require a comprehensive study of how the state imposes the death penalty."); Bibisi, supra note 30 (noting that Connecticut Governor John Rowland signed legislation banning execution of retarded individuals); Bill Bell, Jr., *Legislation Sent to Holden Would Ban Death Penalty for Mentally Retarded*, St. Louis Post-Dispatch, May 11, 2001 ("Capital punishment in Missouri for the mentally retarded would become a thing of the past under legislation on its way to the governor. House members gave final approval to the bill Friday, voting 107-19; the Senate approved the same measure Thursday night. The sponsor is Sen. David Klarich, R-Ballwin. Gov. Bob Holden indicated Friday he probably would sign the bill. 'I've always said that someone who meets the definition of mentally retarded, I would have serious concerns about ever putting an individual like that to death,' he said."); Amber McDowell, *Court Denounces Execution of Retarded*, Associated Press, Dec. 4, 2001 ("Executing the mentally retarded is cruel and unusual punishment prohibited by both the federal and state constitutions, the Tennessee Supreme Court ruled Tuesday. Such executions violate 'the evolving standards of decency that mark the progress of a maturing society both nationally and in the state,' Justice Riley Anderson wrote in the majority opinion."); Holden Signs Ban on Death Penalty for Mentally Retarded, St. Louis Post-Dispatch, July 2, 2001; Lounsberry, supra note 1 (discussing Arizona's adoption of ban on executing mentally retarded persons in April 2001); Jeb Bush Signs Bill Barring Executing the Retarded, N.Y. Times, June 13, 2001 ("Joining a rising number of states that prohibit the execution of individuals who are mentally retarded, Gov. Jeb Bush extended the ban to Florida today under a bill he signed into law."). See also Raymond Bonner, *Argument Escalates on Executing Retarded*, N.Y. Times, July 23, 2001; Raymond Bonner, *Drawing a Line on Death*, N.Y. Times, June 24, 2001 ("Capital punishment is a divisive topic in this county, and recently a heated debate has arisen about whether convicted murderers who are mentally retarded should be executed. President Bush jumped in the fray when he said, 'We should never execute the mentally retarded.'"); Duggan, supra note 19 (as of 1989, "only two states barred capital punishment for mentally retarded defendants"; in the ensuing 11 years (as of

2000), 11 more adopted a ban).

On North Carolina, see, e.g., Associated Press, *Easley Signs Law Banning Execution of Mentally Retarded*, *Herald Sun*, Aug. 4, 2001 (despite reservations, “Gov. Mike Easley . . . signed legislation that bans executions of the mentally retarded,” noting that support for the bill by “the state district attorney’s association and Attorney General Roy Cooper were ‘compelling factors’ in his decision”); Dana Damico, *Death-Penalty Reforms Remain on Legislative Agenda: Backers Push Proposals Ranging from Outright Ban to Additional DNA Tests*, *Winston-Salem J.*, Apr. 30, 2001 (legislation to ban the execution of retarded defendants passed the North Carolina Senate by a vote of 31-18); Stan Swofford, *Decision Could Nullify Appeal*, *Greensboro News & Record*, July 26, 2001 (noting that North Carolina ban on executing retarded individuals may spare “as many as 20 percent of the state’s 220 Death Row inmates”; “[n]ationwide, an estimated 300 mentally retarded people are on death row awaiting execution.”).

35. On Albert Burrell’s case, see Tom Guarisco, *Former Death Row Inmate Wants to Repair Cars Again*, *Baton Rouge Advocate*, January 3, 2001:

The nearly fatal turn Albert Ronnie Burrell’s life took 14 years ago finally ended Tuesday when he walked away from Angola’s death row a free man.

Burrell was living with his mother in rural north Louisiana in 1987 when a jury convicted and sentenced him to die for the double murder of an elderly Union Parish couple.

Last week, the state Attorney General’s Office dismissed all charges against Burrell and his supposed accomplice, Michael Ray Graham Jr. of Roanoke, Va. The Attorney General’s Office said the case against the men was weak, and DNA testing of blood found at the scene did not match their blood.

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Burrell’s release came late Tuesday afternoon after a court hearing in Union Parish. Burrell was once as close as 17 days away from execution. He is mentally retarded[, cannot read or write,] and did not understand how and why he ended up on death row, but he fully expected to die, according to his attorneys and family.

On the Porter and Washington cases, see *infra* pp. 27-34. In regard to the proclivity of retarded persons, children, suspects under coercion from police officers and others to confess to crimes they did not commit, particularly in capital and other murder cases, see *The Chicago Tribune’s* recent four-part series, Ken Armstrong, Steve Mills & Maurice Possley, *Cops and Confessions*, *Chi. Trib.*, Dec. 16-19, 2001; Editorial, *New Doubts About Confessions*, *Chi. Trib.*, Dec. 19, 2001.

36. In *McCarver v. North Carolina*, 121 S. Ct. 1401 (2001) (mem.), the Supreme Court agreed to reconsider its decision in *Penry v. Lynaugh*, 492 U.S. 302 (1989), that the Eighth Amendment Cruel and Unusual Punishment Clause does not bar execution of mentally retarded individuals. See Lounsberry, *supra* note 1 (“Twelve years after approving such executions, the [U.S. Supreme Court] said it would . . . determine if imposing the death penalty on the mentally retarded violates the Eighth Amendment ban on cruel and unusual punishment”). After North Carolina adopted legislation retroactively barring executions of the mentally retarded prisoners, see *supra* note 36 and accompanying text, the Court dismissed the *McCarver* case and immediately agreed to review the same claim made by a Virginia death row inmate, Daryl Atkins. See Gina Holland, *Court Reviews Executing Mentally Ill*, Associated Press, Sept. 25, 2001 (“The Supreme Court made

clear Tuesday that it will soon decide if it is constitutional to execute mentally retarded killers, substituting a moot North Carolina inmate's case with one from Virginia.”).

37. See *Changes in the Death Penalty Around the U.S.*, supra note 19; supra pp. 1-2 & nn.14, 19 (Nevada, Texas). On Kentucky, see John Cheves, *Judges Are Overturning Death Sentences*, *Lexington Herald-Leader*, Oct. 31, 2001 (Kentucky “death penalty opponents have found a new ally: Gov. Paul Patton announced this month that he supports a proposed bill to abolish the death penalty for those who committed their crimes as 16- and 17-year-olds.”).

38. Harwood (*Wall St. Journal*), supra note 1:

Other [states], including Arkansas and North Carolina, have indirectly curbed [the death penalty's] application by beefing up standards or taxpayer funds for the representation of indigent defendants. . . .

Just last week, the Texas House voted to create the state's first standards for court-appointed lawyers. The Texas Senate had already passed similar legislation. The Supreme Court this fall is scheduled to revisit whether to bar the execution of mentally retarded inmates.

The pendulum swing is occurring even in Oklahoma City, where Mr. McVeigh bombed the Alfred P. Murrah Federal Building six years ago, killing 168 people. There is early evidence that Oklahoma convicts are receiving fewer death sentences in the wake of the state's decision to improve legal counsel for poor defendants and expand access to DNA testing. . . .

[Under the Oklahoma reforms, a] state board . . . provides lawyers for poor defendants.... In the past, if a lawyer assigned to represent an indigent defendant “had vital signs, he was determined to be competent,” says [Board chairman Jim] Bednar. “In theory I'm not opposed to the death penalty. But it's the practice we need to look at. The system is flawed.”

[Bednar] began to overhaul the indigent-defense agency by winning funding increases to hire better-quality lawyers. The agency is now sending the message that attorneys for poor inmates “are really going to show up and do our job,” Mr. Bednar says.

Because of stiffer opposition, prosecutors are becoming “more hesitant to seek the death penalty,” he adds. In fiscal year 1998 . . . , prosecutors in the area served by his Norman office, which covers roughly the western half the state, sought death sentences in 36 cases. They obtained the punishment in four cases. Last year, prosecutors sought 26 death sentences and obtained only one.

See also *Changes in the Death Penalty Around the U.S.*, supra note 19; *The Justice Project*, supra note 8. For evidence of a similar decrease in the use of the death penalty as a result of better qualified and compensated counsel in Indiana, see Norman Lefstein, *Reform of Defense Representation in Capital Cases: The Indiana Experience and Its Implications for the Nation*, 29 *Ind. L. Rev.* 495 (1996) (discussed *infra* note 937).

On Virginia, see Associated Press, *Court to Help Set Standards—Death-Penalty Lawyers Affected*, Jan. 4, 2002 (“Beginning Jan. 1, the state Supreme Court becomes involved in setting standards for lawyers qualified to represent people facing the ultimate legal trouble: the death penalty. Lawyers appointed to represent indigent Virginia clients receive the lowest compensation in the nation. The change in the law represents a step by the state to compile a better list of attorneys with the skills and training necessary to defend people accused of capital crimes.”).

39. See *supra* p. 2 & n.16.

40. See *Changes in the Death Penalty Around the U.S.*, *supra* note 19.

41. See, e.g., Hon. Bill Delahunt, *Protecting the Innocent*, Harvard Crimson, May 14, 2001; Harwood, *supra* note 1:

In Congress, legislation that would create financial incentives for states to expand access to DNA testing and set standards for legal representation of defendants in capital cases is gathering support in both parties. In the Senate, its 19 co-sponsors include four Republicans and last year's Democratic vice presidential candidate, Joseph Lieberman, who declined to back the bill a year earlier. Its . . . co-sponsors in the House include several members of the GOP's conservative wing.

GOP Rep. Mark Souder of Indiana, one of the co-sponsors, says, "I support the death penalty, [but] I'm a little uncomfortable. We want to be more sure."

See also Jost, *supra* note 1 (as of November 2001, bill had 21 co-sponsors in Senate); Brooke A. Masters, *Executions Decrease For the 2nd Year: Va., Texas Show Sharp Drops Amid a National Trend*, Wash. Post, Sept. 6, 2001 ("The federal Innocence Protection Act, which would provide DNA testing and set minimum standards for court-appointed defense lawyers, also continues to make progress. The House version has 210 sponsors, close to a majority. In the closely divided Senate, several moderate Republicans have recently come out for the bill."). On Senator Warner's views, see *infra* note 77.

42. See *Ryan Says He Won't Run for Re-Election*, Chi. Trib. Aug. 9, 2001 ("Above all, [Ryan] pointed with pride to his death-penalty moratorium. Ryan vaulted into the national spotlight in January 2000 by halting executions indefinitely after a string of men were released from death row either because evidence showed they were innocent or because they received unfair trials. Since Illinois reinstated capital punishment in 1977, 12 death row inmates have been executed while the sentences of 13 others have been overturned. A Ryan-appointed commission is studying the state's death penalty system. 'During the past three years, I've talked to people all over the world about the shocking condition of our capital punishment system,' Ryan said. 'I knew then as I do now that I had no choice but to call a halt to what was obviously a deeply flawed process.'"); *7 of 8 Candidates for Governor Would Prolong Execution Freeze*, St Louis Post Dispatch, Oct. 29, 2001 ("7 of the 8 candidates for Illinois governor [including the state's conservative Republican attorney general and one other leading Republican candidate] say they would keep the state's freeze on executions until it is proved that innocent people aren't being sentenced to death. It's a political oddity in these 'law-and-order' times—one that dramatizes how jittery Illinoisans are about a justice system that has had to release more death row inmates than it has executed."); *supra* p. 1 & nn.4-6.

43. See Gerald F. Seib, *Bush's Race Issue: What's the Role of Death Penalty*, Wall St. J., Feb. 28, 2001 (quoting Governor Rowland, a death penalty supporter, stating that Republicans and especially the Bush Administration need to take seriously African American citizens' doubts about the fairness of the death penalty).

44. See *Facing Death*, Santa Fe New Mexican, Oct. 28, 2001 ("Gov. Gary Johnson—for years a staunch advocate of the death penalty—now says that eliminating capital punishment might be good public policy. . . . [I]n a letter being sent this week to the hundreds of people from around the world who have written to him about [an impending] execution, Johnson, who campaigned in 1994 and 1998 as a supporter of capital punishment, said, 'My mind is not closed on the subject.' 'I am of the opinion that swift and sure punishment

deters crime,' Johnson wrote. 'Currently, I do not believe that New Mexico's death penalty serves as an effective preventative measure because it is neither swift nor sure. The time period currently allowed for appeals under the process is too long and yet I have come to believe that innocent people might be put to death if these safeguards are not in place.' . . . '[E]liminating the death penalty in the future may prove to be better public policy given the reality of the sentence today.'"). See also Gilbert Gallegos, Johnson Yanks Support from Time Cap on Sentence Appeals, *Albuq. Trib.*, Nov. 6, 2001 (discussing Governor Johnson's decision to withdraw legislation he had previously proposed that would have placed a two-year cap on the length of capital appeals; Johnson now believes that "limiting death-row appeals would probably lead to innocent people being executed," a view he bases in part on "a case in the 1970s in which four members of a motorcycle gang were wrongly accused and convicted for murdering a University of New Mexico student. The four men were later released from death row after another man admitted to the murder. 'That case proved that had I passed my law, someone like that might have been sentenced to death,' Johnson said. 'That scared me to death. I've had several things happen to me causing me to reexamine my position.'" (discussed *infra* p. 419 & n.966)). See also Barry Massey, *As the 2002 Legislature Convenes, Gov. Gary Johnson Says He Will Consider Repeal of the Death Penalty*, *Santa Fe New Mexican* Jan. 16, 2002.

45. See John Harwood, *Bush May Be Hurt by Handling of Death-Penalty Issue*, *Wall St. J.*, Mar. 21, 2000 (noting that "independent Gov. Jesse Ventura of Minnesota ha[s] abandoned his former support for capital punishment"); David Shaffer, *Though Most in State Back Death Penalty, Support is Decreasing*, *Minneapolis-St. Paul Star Trib.*, Mar. 20, 2000 ("Gov. Jesse Ventura, a one-time death penalty advocate, also has changed his mind. In February, he said on 'Meet the Press' that he no longer supports it because the risk of putting an innocent person to death bothers his conscience. The government has no right to take someone's life, he said.").

46. See Robert Reno, *Support for Death Penalty Goes Wobbly*, *Des Moines Reg.*, June 12, 2000 ("The most recent defector from . . . capital-punishment . . . is Oliver North," who recently declared, "I think capital punishment's day is done in this country. I don't think it's fairly applied."). See also Murray Campbell, *Capital Punishment: Bush Faces a Shift in Public's Mood*, *Toronto Globe & Mail*, Feb. 21, 2001 ("The [death penalty] has ceased to split Democrats and Republicans; conservatives such as Pat Robertson, Oliver North and George Will have criticized the death penalty. Even John DiIulio, director of Mr. Bush's new White House Office of Faith-Based and Community Initiatives, is reported to have abandoned his support for capital punishment."); *infra* note 57 (John DiIulio's views).

47. See George Will, *Innocent on Death Row*, *Wash. Post* Apr. 6, 2000 at A23.

48. See Bruce Fein, *Death Penalty Ignominy*, *Wash. Times*, Mar. 20, 2001 ("For a select category of barbaric crimes, the death penalty is justified . . . [b]ut it is disgraceful for the government in cases to deny an indigent accused at least mediocre defense counsel to bolster what may be the chief safeguard against executing the innocent . . .").

49. See Paul Craig Roberts, *Wash. Times*, June 19, 2001 ("An ever-growing number of books, innocence projects and overturned convictions speak to the unreliability of conviction. A surprising number of death row inmates have been discovered to be innocent of the capital offense for which they were convicted. A criminal justice system that convicts innocents on the serious charge of murder is certain to convict innocents on less serious charges as well.").

50. Dreher's views are particularly relevant to the impact of legal error on faith in the capital system:

FBI Director Louis Freeh did what Pope John Paul II has not been able to do: turn this law-

and-order Catholic conservative against the death penalty.

Timothy McVeigh's guilt is unquestionable, as is the gravity of his crime. If ever there were a case where it was important for the government to play by the rules, it was this one.

And still, they fumbled.

To be sure, the 3,000 pages of documents the FBI failed to turn over to McVeigh's lawyers will not exonerate him. McVeigh did it. He admits he did it. He deserves to die.

That's not the same as saying the state should put killers to death. After the McVeigh debacle, who can trust our government to administer capital punishment?

We know all about McVeigh's saga. But what of the anonymous cases where the defendant's guilt is less obvious, and law enforcement feels less of an obligation to do things by the book?

Consider the shocking scandal unfolding in Oklahoma City, of all places, involving the work of police chemist Joyce Gilchrest. Over 14 years, the state won hundreds of felony convictions based in part on her expert testimony. But she has long been criticized for sloppy work.

Last week, a judge released a convicted rapist who had been behind bars for 15 years after an independent lab analysis contradicted her findings.

At least that poor sod has his life. Eleven men have been executed for convictions won partly on the basis of Gilchrest's testimony. Twelve others sit on death row. The state is investigating.

* * * * *

So why is McVeigh a tipping point? I can't say for sure, but there's something about the psychology of this case that magnifies the FBI's relatively insignificant error. If they screw up even with McVeigh . . .

That sets a hairline crack in the foundation of our justice system, one that collapses the rock-solid faith in it one must have if one is to support state-sanctioned killing.

In the end, McVeigh will probably be executed, and justice will have been done. On that day, I won't feel any compassion for the guy, but I will feel terrible for those unknown innocents yet to be executed so we could be free to whack Tim McVeigh.

Think of men like Anthony Faison and Charles Shepard, who were freed from prison yesterday after someone else confessed to a 1987 murder that sent them up the river—and a key state

witness admitted to lying on the stand.

Had they been convicted in a death-penalty state, which New York was not at the time, they might be dead today.

* * * * *

We conservatives cannot afford to let our justified outrage at unrepentant killers like McVeigh make us morally indifferent to the deadly and irrevocable peril in which society places the truly guiltless on trial for their lives.

At some point in this death-penalty debate, the sanctity of innocent life demands that men and women of conservative conscience have to say: Enough.

Rob Dreher, *Feds Shake Faith in Death Penalty*, N.Y. Post, May 15, 2001.

51. See Seib, *supra* note 43 (urging President Bush to rely on his “death-penalty credentials” to “open the door to a national debate on the justice system” and especially the “death penalty [which] hits blacks disproportionately”).

52. See Death Penalty Information Center, *Conservative Leaders Join Moratorium Group in Urging President Bush to Suspend Federal Executions*, <http://www.deathpenaltyinfo.org/whatsnew.html> (visited June 27, 2001) (“Citizens for a Moratorium On Federal Executions (CMFE), a Washington-based group of prominent citizens concerned about the death penalty, sent a letter to President Bush this week calling for a halt to federal executions until lingering questions about its fairness can be resolved. The letter was signed by both well-known death penalty opponents and conservative leaders such as Emmett Tyrell Jr., editor in chief of *The American Spectator* and John Whitehead, founder of the Rutherford Institute.”); R. Emmett Tyrrell, Jr., *Capital Brutality*, *Am. Spectator*, Feb. 1, 2001, at 13.

53. See Milt Bearden, *Death Penalty Would Hinder Anti-Terrorism*, *Wall St. J.*, June 4, 2001. See also Wayne Woodlief, *Gutsy Reps Voted Bravely—Death Penalty Nays Stood Up for True Justice*, *Boston Herald*, Dec. 23, 2001 (conservative columnist praising five Massachusetts congressmen for voting against a “death penalty add-on” to an extradition bill, which “makes it harder to bring terrorist suspects arrested abroad to justice in the United States,” given the refusal of Canada, European nations, South Africa and other countries “to extradite suspects from the al-Qaeda network if they will face execution” in the U.S.).

54. James Orenstein, *In this Death Penalty Case, the Choices Were Too Few*, *N.Y. Times*, June 17, 2001 (concluding that the recent execution of federal prisoner Juan Raul Garza “forces us to confront troubling questions about not only the general fairness of a capital punishment system that has a disproportionate impact on African-Americans and Hispanics, but also the fairness of depriving Mr. Garza of a basic protection that every federal inmate on death row has received,” *i.e.*, an instruction informing the jury that it had the alternative of sentencing the defendant to life without parole”).

55. See Brooke A. Masters, *Standards for U.S. Executions Proposed: Nonpartisan Committee Seeks Broad Consensus on Reforming Death Penalty*, *Wash. Post*, June 27, 2001, at A4.

56. See Harwood (*Wall St. Journal*), *supra* note 1:

Increasing opposition to capital punishment among religious leaders helped fuel the shift in

opinion. Catholic bishops have called for the abolition of capital punishment as part of the “ethic of life” that leads to their opposition to abortion. In early 1999, then-Missouri Gov. Mel Carnahan commuted the death sentence of one inmate after receiving a personal plea from the Pope. Last year, televangelist Pat Robertson, a former Republican presidential candidate, called for a moratorium on capital punishment, after earlier unsuccessfully lobbying Mr. Bush to spare the life of convicted Texas murderer Karla Faye Tucker.

57. See John J. DiIulio, *Abolish the Death Penalty*, *Officially*, *Wall St. J.*, Dec. 15, 1997, at A23.

58. See Ken Camp, *Texas CLC Seeks Stay of Executions*, *The Baptist Standard*, Apr. 23, 2001. The Baptist Life Commission is the agency that studies ethical issues for the Texas Baptist Convention, which is affiliated with the Southern Baptist Convention, a denomination with a strong position in favor of capital punishment. See *National Catholic Reporter*, June 30, 2000 (quoting recent resolutions by Southern Baptists supporting capital punishment).

59. See *State Methodist Organization Calls for Moratorium on Death Penalty*, *The Associated Press* June 9, 2001.

60. See John W. Whitehead, *Passing a Moratorium on Federal Executions Would be a Giant Step for a Compassionate Conservatism*, *Letter to President George W. Bush*, Apr. 3, 2001.

61. See Caryle Murphy, ‘An Eye for an Eye’ Challenges Faith, *Wash. Post*, May 13, 2001 (“Most Christian evangelicals, believing that the Bible condones the death penalty, continue to back it. But some are rethinking it since Texas’s 1998 execution of Karla Faye Tucker, who became a born-again Christian in prison. ‘One should not underestimate the significance of [her] execution to the psyche of American evangelicals,’ said Richard Cizik, spokesman for the National Association of Evangelicals. ‘It’s left a long-standing impression. It certainly did with me. It was wrong.’ Her death also led the influential evangelical magazine *Christianity Today* to declare that the death penalty ‘has outlived its usefulness.’). See also Peter Steinfels, *Belief*, *N.Y. Times*, May 12, 2001:

Only a few years ago, few lost causes seemed quite as irretrievably lost as opposing the death penalty. Not only did 8 out of 10 Americans favor capital punishment in 1994, but that proportion had been growing for over a quarter of a century.

Such overwhelming support may still exist for the execution of Timothy J. McVeigh, but not for the death penalty generally. This shift in public opinion is remarkable . . . in itself. But so are two other things. First, a major factor behind the shift is religion. Second, virtually no one has thought to complain about that fact.

62. See Alan Johnson, *Justice Has Change of Heart on Ohio Death-Penalty Law*, *Columbus Dispatch*, Apr. 26, 2001:

Campaigning for attorney general in 1990, Paul E. Pfeifer tore into Democrats for not zealously enforcing the death-penalty law that he’d helped write nine years earlier as a state senator.

* * * * *

Now, as an Ohio Supreme Court justice responsible for weighing life-and-death cases, the 58-year-old Pfeifer sees the issue far differently.

* * * * *

He doesn't oppose the death penalty, but he clearly is wrestling with the ramifications of the law he co-wrote and steered as Senate Judiciary Committee chairman.

* * * * *

[Recently h]e has taken a close look at how capital-punishment cases are reviewed by the high court, a process he says is handcuffed by a lack of information and time.

Further, he has called for Gov. Bob Taft to form a blue-ribbon panel to evaluate all 201 Death Row cases to see how many could be commuted to life in prison without parole.

"We know we're not going to execute all these people on Death Row," Pfeifer said. "This state is not going to start executing people at the clip of 10 a year, which is what you would have to do to stay current at the pace we're putting them in."

63. See Rethinking Justice: a Cleburne Judge Who Helped Write the State's Laws Says He Now Has Concerns about the Death Penalty, Fort Worth Star-Telegram, July 24, 2001:

30 years after helping craft Texas' capital punishment laws, Senior State District Judge C.C. "Kit" Cooke is questioning the fairness of the death penalty.

During a statewide legal seminar last week in Corpus Christi, Cooke recalled the 11 death penalty cases he has presided over during his 23-year judicial career. He talked about how those trials have altered his feelings regarding Texas' ultimate punishment.

"I think the mood is changing in this country and people are realizing there are deficiencies in the system," said Cooke, 54, of Cleburne . . . "We always think we've got the right person, but the system is not infallible. . . ."

Although Cooke still supports the death penalty, he has concerns about possible deficiencies in the system such as inadequate legal representation, access to DNA testing and the racial disparity of those executed. "I was looking at it as a young politician, with about 90% of my district supporting the death penalty," said Cooke, referring to his service in Johnson County as a state representative when he was 24. "Now, from a judge's perspective and taking care of people's rights, I think it has a lot of flaws . . ."

64. See Lise Olsen, The Death Penalty: Uncertain Justice, Seattle Post-Intelligencer, Aug. 6, 2001 (noting that death penalty "[c]ritics have a respected ally in former [Washington] Supreme Court Chief Justice Richard Guy, who spearheaded numerous reforms before retiring this year. Guy concedes they didn't go far enough. He's convinced the state's capital punishment system is broken—appeals take too long and cost too much. 'The legislature has the obligation to look at whether the cost is worth what we pay for it in terms of money, time and facts that have torn people apart,' he said."); Lise Olsen, State's Chief Justice Calls for Reform in Death Penalty Defense System, Seattle Post-Intelligencer, Aug. 18, 2001 ("Concerned about a capital punishment system marred by bad lawyering, the chief justice of the Washington Supreme Court is recommending that the state assume more responsibility for funding death penalty defense teams and screening lawyers assigned to the high-stakes cases. Chief Justice Gerry Alexander said the state should pick

up at least part of the tab for capital defense statewide to take pressure off rural counties and help ensure local judges appoint qualified attorneys and not cut corners on legal fees.”).

65. For example:

- Legislative reform efforts in Illinois are being led by a former prosecutor and Republican state representative, Jim Durkin, and are supported by the Illinois Association of Police Chiefs. See supra note 6.
- “Ohio’s capital-punishment law . . . is under fire from an unlikely source—conservative, faith-based Republicans . . . [who are] lining up with liberal Democrats . . . to support a study of the death-penalty law.” See Alan Johnson, *Ohio Study of Execution Law Sought*, Columbus Dispatch, June 18, 2001.
- Conservative Nevada Senator Mark James (R-Las Vegas), “who has worked nearly a decade to toughen Nevada’s criminal laws,” is urging the state Assembly to adopt a moratorium on executions while Nevada’s capital punishment system is studied. “[N]ever has the death penalty been the subject of a study in this state that I know of,” said James. “And the system is broken.” James also expressed concern about who receives the death penalty in Nevada. “The Assembly passed a bill unanimously which prohibits racial profiling in traffic stops and yet 40 percent of those on death row are African Americans and nobody says a thing about it.” He added, “There is not a single person on death row that had a fully funded private defense. If you’re rich, you’re not going to get capital punishment—period.” *Senator Says Justification for Death Penalty Study Obvious*, Associated Press (AP) Newswires, April 21, 2001.
- Virginia Delegate Frank Hargrove (R-Hanover County), recently introduced a bill in the Virginia General Assembly to abolish the death penalty. Hargrove, who once proposed bringing back hanging as a method of execution in Virginia, said that he was now troubled by the possibility that mistakes could lead to the execution of an innocent person. Another conservative Virginia Republican, Jeannemarie Devolites (R-Fairfax County), is sponsoring a moratorium bill which would suspend executions until after the Joint Legislative Audit and Review Commission completes its review of the state’s death penalty system. “I think there’s a lot of concern, not just from legislators, but from the public as a whole, that we could be executing innocent people,” said Devolites. A third Virginia legislator, Delegate Robert Marshall (R-Prince William), has proposed legislation that would require that capital juries be told that Virginia nearly executed an innocent man, Earl Washington, who was eventually exonerated by DNA evidence. *Death Penalty Information Center, Changes in the Death Penalty Around the U.S.: Virginia*, <http://www.deathpenaltyinfo.org/newvoices.html#VA> (visited June 27, 2001) (quoting Associated Press, Jan. 20, 2001).
- “In eight years as St. Louis [Missouri’s] circuit attorney, Dee Joyce-Hayes stood by her assistants as they argued for the death penalty Despite that history and 12 years as a trial prosecutor herself, Joyce-Hayes, just seven months out of office, denounced the death penalty ‘I began over a period of time to have experiences that certainly caused me to question at least the efficacy of the death penalty, if not the moral implications of it,’ said Joyce-Hayes.” *Elizabeth Holland, Joyce-Hayes Speaks Against Death Penalty*, St. Louis Post-Dispatch July 30, 2001.
- The strongly pro-death penalty *Orlando Sentinel* recently published an editorial calling for a study and moratorium:

Capital punishment historically has played a small, but significant, role in Florida's criminal-justice system, a role this newspaper has supported for years. Fairness and the integrity of that system, however, demand a new look. Florida leads the nation in the number of people—21—who have been removed from death row in the past two decades because the defendants were later found innocent or because of serious flaws in the way cases were handled. Those flaws included misconduct by police and prosecutors and inept defense lawyers. Illinois is a distant second with 13 people removed from its troubled death row. In fact, during the past 25 years, Florida has executed 51 people. Is there any chance that an innocent person was among them? Considering the number of people removed from death row, that question should disturb any reasonable Floridian. . . . Gov. Jeb Bush, . . . should convene a special bipartisan commission to scrutinize capital punishment, and he should delay any further executions until that investigation is completed.

Editorial, Revisit the Death Penalty: Gov. Bush Should Create a Panel on the Death Penalty and Delay Executions, *Orlando Sentinel*, Aug. 19, 2001.

- “Last month, the name of [Texas’s] death row, the Terrell Unit, was changed after Charles Terrell, a former chairman of the Texas Board of Criminal Justice, asked that his name be removed. Mr. Terrell made the request in part because he questions the fairness of the capital punishment system in his home state.” Yardley, *Texas Wavering on Death Penalty*, supra note 19.

See also Peter Beinart, *Mercy Seat*, *New Republic*, June 11, 2001 (“Washington is several years behind public opinion and the states on the issue [of the death penalty]. And . . . it is often state and local Republicans who have taken the lead [in reform efforts]. Last year, the Republican governor of Illinois announced a moratorium on executions. Nebraska’s GOP-controlled state legislature passed one as well. In overwhelmingly Republican New Hampshire, the state legislature passed legislation outlawing the death penalty altogether. . . . [And] the Texas state legislature, including the Republican-led State Senate, [have] now pass[ed] a series of reforms . . .”).

66. See Cheyenne Hopkins, *Keating Proposes Death Penalty Standard*, *The Oklahoman*, June 23, 2001 (quoting National Press Club speech citing decline in faith in the capital system and calling for a “higher threshold” of certainty about guilt before convicting defendants subject to the death penalty that would “requir[e] a juror to go deeper in his own deliberations than the ‘reasonable doubt’ standard requires”); John Podhoretz, *Why DNA Will Save the Death Penalty*, *NY Post*, June 19, 2001 (advocating changes in “[s]tate and federal sentencing guidelines [that] make it possible to impose the death penalty only in cases where the physical evidence makes it absolutely certain that the accused is indeed the killer. When the evidence is circumstantial, the death penalty will not be sought.”); supra note 1 (further discussion of Governor Keating’s proposal). See also Death Penalty Information Center, *Changes in the Death Penalty Around the U.S.: Ohio*, <http://www.deathpenaltyinfo.org/Changes.html#OH> (visited June 27, 2001) (“On February 15, 2001, Rep. Jones proposed a bill (HB 101) to prohibit the death penalty unless the defendant is convicted beyond any doubt of aggravated murder and an aggravating circumstance and the aggravating circumstances outweigh beyond any doubt the mitigating factors”).

67. Joseph Hoffmann, *Violence and the Truth*, 76 *Ind. L.J.* 939, 940-41 (2001); see *id.* at 941 (“public support” for the death penalty is “dropping sharply”). See also conservative commentator John Podhoretz’s view that “arguments in opposition to the death penalty are receiving a receptive hearing in a way they haven’t since the early 1970s. For more than two decades, Americans approved the use of the death penalty

by a three-to-one margin. Lately, that has shrunk to two-to-one, still overwhelming, . . . but a significant drop in support nonetheless.” Podhoretz, *supra* note 66; accord Jost, *supra* note 1 (“There’s no doubt that public support for executions has declined.” (quoting Dudley Sharp, resource director of a leading pro-death penalty group, Justice for All)).

68. See, e.g., Lounsberry, *supra* note 1.

69. “Noting that Minnesota does not have the death penalty, O’Connor noted, ‘You must breathe a big sigh of relief every day.’” Bakst, *supra* note 3; see Charles Lane, Justice O’Connor Questions Death Penalty, *Wash. Post*, July 3, 2001 (“Justice Sandra Day O’Connor, whose vote often decides close cases at the Supreme Court, has added her voice to the growing chorus of skepticism about the administration of capital punishment in the United States.”); Alan Berlow, A Supreme Court Shocker: Sandra Day O’Connor’s Criticisms of the Death Penalty Couldn’t Have Come from a More Unlikely Source, *Salon*, July 3, 2001, <http://www.salon.com/-news/feature/2001/07/04/oconnor/index.html>; Healy, *supra* note 1 (“More than just a surprise, however, O’Connor’s statement was the latest and perhaps clearest sign of how sharply the debate over the death penalty has shifted in recent years.”). Justice O’Connor elaborated on these warnings at a Nebraska Bar Association meeting several months later. See John Fulwider, O’Connor Lectures Lawyers, *StatePaper.com*, Nebraska, Oct. 18, 2001 (in Justice O’Connor’s speech to the Nebraska Bar Association, she said that “innocent people may well continue to receive the death penalty if lawyers don’t . . . start doing more pro bono work for indigent defendants. . . . ‘More often than we want to recognize, some innocent defendants have been convicted and sentenced to death,’ O’Connor told members of the Bar Association . . . ‘That will continue to happen unless qualified lawyers take up their cases,’ she said.”)

For a similar view from a front-line federal judge, see Judge Michael Posner, *Life, Death and Uncertainty*, *Boston Globe*, July 8, 2001:

To the [federal district] judge in charge, the murder trial of Kristen Gilbert offered an unsettling lesson—and inescapable conclusion—about the ultimate cost of the death penalty. . . .

* * * * *

Presiding over this, the 1st death penalty case in Massachusetts in several decades, was the most complicated and stressful thing I’ve ever done (aside, perhaps, from raising teenagers).

The experience left me with one unavoidable conclusion: that a legal regime relying on the death penalty will inevitably execute innocent people Any honest proponent of capital punishment must face this fact.

See also the like-minded concerns of Ohio Supreme Court Justice Paul E. Pfeifer and Texas Senior State District Judge C.C. “Kit” Cooke, both of whom—like Justice O’Connor when she was majority leader of the Arizona state legislature—played important roles in adopting those states’ modern death penalty laws.

70. See Blakemore, *supra* note 22 (also discussed in Richard Morin & Claudia Deane, *McVeigh’s Execution Approved, While Principle Splits Public*, *Wash. Post*, May 3, 2001, at A9) (even when questioned on the eve of the McVeigh execution, only 63% of Americans—the lowest proportion measured in years—expressed support for the death penalty on a recent *ABC-Washington Post* poll); Jonsson, *supra* note 1 (“While a majority of Americans still support the death penalty, that number has slipped from 77 percent five years ago to 63 percent, according to polls); Jost, *supra* note 1 (“Support for the death penalty has dropped since

reaching its peak in 1994. During roughly the same period, the percentage of people against capital punishment doubled.”).

Other recent polls have measured support for the death penalty between 59% to 67%—in all cases the lowest each respective poll has measured in years. See Peter D. Hart Research Associates, Inc., *Poll Finds Support for Death Penalty Alternatives and for System Reforms*, Mar. 27, 2001 (“A national poll recently conducted by Peter D. Hart Research Associates found only 60% favored the death penalty for persons convicted of murder”); Jeffrey M. Jones, *Two-Thirds of Americans Support the Death Penalty*, Gallup News Service, Mar. 2, 2001 (“Since [1994], support [for the death penalty] has declined, dropping to the current level of 67%. Even news of the Oklahoma City bombing and the federal government’s seeking (and obtaining) the death penalty for McVeigh in 1995 did not reverse the downward trend.”); *The Declining Support for Execution*, N.Y. Times, May 10, 2001 (discussing various polls including one by the Pew Research Center pegging support for the death penalty at 66%); *Sparing Innocents*, Boston Globe, Apr. 3, 2001 (reporting Harris Poll showing national support for death penalty at 64%); Richard Willing, *Even for Death Penalty Foes, McVeigh Is the Exception*, USA Today, May 4, 2001, at 1A (support for death penalty drops to 59% when respondents are given choice between supporting the death penalty, opposing it except for mass murder Timothy McVeigh (22%), and opposing it in all cases (16%)). See also Lichtblau, *supra* note 1 (“Recent polls show that, while a majority of Americans still favor the death penalty, the numbers are shrinking. California saw a particularly sharp drop, with support declining from 78% in 1990 to 58% last year, according to a Los Angeles Times Poll.”). See generally Samuel R. Gross & Phoebe C. Ellsworth, *Second Thoughts: Americans’ Views on the Death Penalty at the Turn of the Century*, in *Capital Punishment at Century’s End: New Insights, Old Doubts* (Stephen P. Garvey ed. 2001).

71. See Duggan, *supra* note 19 (“Of the 38 states with capital punishment laws, 35 offer juries the life-without-parole choice”); Editorial, *McVeigh Errors Raise Doubts About Other Capital Cases*, USA Today, May 16, 2001 (reporting that Wyoming recently adopted life without parole as an alternative to the death penalty, making it, the 36th death penalty state (out of 38) to do so); The Justice Project, *supra* note 8 (reporting that, during its 2001 legislative session, Oregon adopted life without parole as the alternative to a death sentence, making it the 37th death state (out of 38) to do so). On Texas, see *supra* p. 2 & n.18.

72. See, e.g., Morin & Deane, *supra* note 70 (in a May 2001 poll, 46% favored the death penalty over life without parole, while 45% favored life without parole—up from 38% measured two years ago); Peter D. Hart Research Associates, *supra* note 70 (in national poll in mid-March 2001, 38% favor death penalty versus 48% favoring alternative of life without parole and restitution to victim); Jones, *supra* note 70 (in national poll in late February 2001, 54% favor death penalty versus 42% favoring life without parole). See also Illinois Poll Finds Drop in Death Penalty Support, Peoria J. Star, Jan. 30, 2001 (“More Illinoisans would rather see a murderer in prison for life without the possibility of parole [47% favored this option] than sentenced to death [33% favored this option], according to a survey just released”).

73. See, e.g., Morin & Deane, *supra* note 70 (in recent ABC-*Washington Post* poll, “fifty-one percent of those interviewed favored halting all executions until a commission is established to determine whether the death penalty is being administered fairly while 43 percent opposed a halt. . . . More than four in 10 [death penalty supporters] supported a moratorium . . . [, and] the proportion [of all respondents] who favored a halt in executions rose to 57 percent when respondents were reminded that the governor of Illinois recently stopped all executions in his state while a commission reviews how the death penalty has been applied.”); Peter D. Hart Research Associates, *supra* note 70 (“72% [of Americans polled in national survey] favored suspension of the death penalty until questions about its fairness can be studied, up from 64% in August 2000”); Jeffrey M. Jones, *Americans Closely Divided on Death Penalty Moratorium*, Gallup News Service, Apr. 11, 2001 (“The public’s support for a moratorium ranges between 53% and 42% depending on exactly how the

concept is presented to them.”); Governor Asked to Halt Executions, *Las Vegas Sun*, Oct. 31, 2001 (“A recent Field Poll found that as many as 73 percent of Californians support a moratorium.”). See also Healy, *supra* note 1:

In the early to mid-1990s, . . . polls showed that 75 percent to 80 percent of Americans supported the death penalty. It was the highest level of support since polling began in the 1930s, and few experts predicted that it would drop any time soon.

But that is precisely what has happened over the past two or three years [with] polls show[ing] that the level of support has fallen to between 60 percent and 65 percent, the lowest point since the mid-1970s.

* * * * *

Though public support for capital punishment has not fallen below 60 percent in nearly three decades, researchers says a further drop in support is not unthinkable. Of the 60 percent to 65 percent of Americans who say they still favor the death penalty, roughly half express some doubts, which means they could eventually switch sides altogether.

For polling information indicating the source of public concern about the death penalty, see Morin & Dean, *supra* note 70 (reporting that in recent ABC-*Washington Post* poll, “68 percent said the death penalty is unfair because ‘sometimes an innocent person is executed,’” while “63 percent agreed that capital punishment is unfair because ‘it’s applied differently from county to county and state to state”); for the first time since the question has been asked, starting in 1985, a majority of Americans believe that the death penalty is not an effective deterrent, a view shared by 40% of death penalty supporters).

In response to a nationwide death penalty moratorium campaign, nearly 60 cities and counties around the nation have adopted resolutions calling on state governors and legislatures to halt executions while the issue is studied. See, e.g., Council on Record Favoring Death Penalty Moratorium, *Lincoln J. Star*, Oct. 30, 2001 (“The Lincoln [Nebraska] City Council tilted 4 to 3 Monday in favor of a death penalty moratorium . . . follow[ing] 3 hours of thoughtful debate.”); Martin Dyckman, *Death Penalty Moratorium Has Victory*, *St. Petersburg Times*, Jan. 13, 2002 (discussing vote by Tallahassee City Commission calling for a moratorium on executions in Florida); Maria Alicia Gaura, *Santa Clara County Wants Execution Suspended*; Davis Asked to Review Process, *S.F. Chron.*, Oct. 31, 2001 (discussing Santa Clara County Board of Supervisors’s adoption of a resolution urging the governor to impose a death penalty moratorium in California; six other California municipalities, including Oakland and San Francisco, have previously passed such resolutions). At the forefront of this movement are 14 city counsels in North Carolina—those of Asheville, Carrboro, Cary, Chapel Hill, Charlotte, Cofield, Davidson, Durham, Fayetteville, Greensboro, Hillsborough, Orange, Thomasville, and Winston-Salem. Other cities that have adopted such resolutions are Atlanta, Baltimore, Buffalo, Detroit, Harrisburg, Hartford, New Haven, Philadelphia, Pittsburgh, Rochester, San Francisco, Tucson and Wilmington (Delaware).

74. See, e.g., Bureau of Justice Statistics Bulletin, *Capital Punishment 1999* (NCJ 184795) and *Capital Punishment 2000* (NCJ 190598) tbl.5 & App. 1 (Dec. 2000, 2001) (only 214 death sentences were imposed in 2000, compared to 272 in 1999 and 300 in 1998; the number of death sentences imposed in 2000 was the lowest since 1980; the drop from 1999 to 2000 was the largest in any year since 1976-1977); Bill Sloat, *Fewer Killers Going to Ohio Death Row*, *Cleveland Plain Dealer*, Aug. 21, 2001 (“Only three convicts were sentenced to death row [in Ohio] last year, an all-time low. And so far this year, there have been five. Criminal justice statistics show a steep decline in the overall percentage of aggravated-murder convictions

carrying the death sentence starting in 1999. Juries began turning to a punishment relatively new in Ohio: Life without parole. . . . Last year, 27 percent of Ohio's aggravated murder convictions carried life without parole, the highest number ever. Four percent were for death, the lowest percentage since records have been kept. . . . It's a dramatic shift from 1998, when sentences for death and life without parole were nearly even. 'Usually we've been up in the double digits with 15, sometimes 17 a year,' Reginald A. Wilkinson, director of the state prison system, said of the inmates entering death row. 'Now the numbers are way down.' . . . [C]riminologists, corrections officials, academics and prosecutors say there could be other reasons for the decline. Some speculate that it may be related to falling crime rates. . . . Other experts suggest the number dropped because juries and judges might be reluctant to sentence people to death because they now know executions actually take place in Ohio. . . . Others say it could be that there are concerns there might be innocent people on death row.''); Willing, *Support for the Death Penalty Might Be Declining*, supra note 1 ("U.S. juries last year sentenced fewer convicts to death than in any year since 1980, according to new Justice Department research that, along with a drop in executions, reflects what some analysts say appears to be waning enthusiasm for capital punishment. . . . The numbers suggest that questions about whether the death penalty is being applied fairly might be resonating across the USA."); supra note 30 (discussing downturn in death sentences in Oklahoma); infra note 233 (reporting decline in death sentences being imposed by California jurors). See also Harwood, supra note 1 ("The number of people annually sentenced to death in the U.S. has fallen in three of the last four years for which statistics are available, to 272, in 1999, since peaking at 319 in 1994 and 1995."); Death Penalty Information Center, *Death Row Declines*, <http://www.deathpenaltyinfo.org/whatsnew.html> (noting that "the number of prisoners on death row as of April 1, 2001 is 3,711, down from earlier this year. This decrease reverses a long trend since the death penalty was reinstated [30 years ago] of consistently increasing death row size." (emphasis added)).

75. See Death Penalty Information Center, *The Death Penalty in 2001: Executions Decline; Death Row Numbers Also Drop* (Dec. 2001) (noting that Texas and Virginia, which have accounted for around half of all executions for about a decade, accounted for less than 30% of the 2001 executions); Jim Yardley, *Number of Executions Falls For Second Straight Year*, N.Y. Times, Dec. 14, 2001 (66 executions in 2001, "down from 85 in 2000 and 98 in 1999 . . . is the first time since executions resumed in 1977 that the number of executions has fallen in consecutive years Equally notably, Texas, the perennial leader in executions saw a decline to 17 executions in 2001, compared with a record 40 last year, and trailed Oklahoma, which led the nation by executing 18 convicted murderers. The drop in executions concludes a year in which capital punishment was debated in statehouses across the country as polls showed growing public concern that an innocent person could be put to death."). See also Brune, supra note 1 ("[T]he fast pace of state executions appears to have slowed as the nation takes stock of how capital punishment works."); Brooke A. Masters, *Executions Decrease For the 2nd Year: Va., Texas Show Sharp Drops Amid a National Trend*, Wash. Post, Sept. 6, 2001 ("[F]ive of the 10 states that have executed the most people—Louisiana, South Carolina, Alabama, Arizona and Georgia—have not executed anyone in 2001 [as of September 6]. In Alabama, all four scheduled executions were stopped by the state or federal courts. Maryland has not carried out an execution since 1998.").

76. See Patrik Jonsson, *Governors Soften on Death Penalty: Numbers of Commuted Sentences, While Sill Small, Rise as Fairness Questions Grow*, The Christian Science Monitor, Oct. 23, 2001:

[North Carolina Governor Mike] Easley's uncertainty about the fairness of the conviction [of Robert Bacon, Jr., leading to Easley's recent commutation of Bacon's death sentence] is in fact part and parcel of an uptick in the number of death- row commutations granted by U.S. governors and parole boards.

Indeed, the trend may be emblematic of a stirring debate from the Carolinas to California—not about the morality of the death penalty, but how fairly and accurately it’s applied.

As it is, the number of commuted sentences has gone from an average of one per year since the Supreme Court reinstated the death penalty in 1976 to an average of three per year since 1999. (The one-a-year average in the 1980s is high, skewed by anti-death penalty governors like New Mexico's Tony Anaya, who once commuted five people at once.) By contrast, the recent decisions come from a broad swath of social conservatives such as Oklahoma Gov. George Keating and pro-death penalty Democrats like Easley.

See also Estes Thompson, *Easley Commutes Sentence of Man who Denies Killing Girlfriend*, Associated Press, Jan. 10, 2002 (“A condemned man who steadfastly denied beating his girlfriend to death in 1990 escaped execution Thursday as [North Carolina] Gov. Mike Easley commuted his death sentence to life in prison. . . . No one saw the killing and no blood or fingerprint evidence connected the attack to Alston, who had been convicted about six weeks earlier of assaulting Perry. Alston, a brick mason, contended his innocence would be proved by DNA tests on evidence that has disappeared.”).

77. See Death Penalty Information Center, *Innocence and the Death Penalty*, <http://www.death-penaltyinfo.org/innoc.html> (visited Jan. 7, 2002) (“Since 1973, 99 people in 22 states have been released from death row with evidence of their innocence.”). See also John Aloysius Farrell, *DNA Scrutiny Tests Judicial System*, Boston Globe, June 26, 2001 (“public opinion polls suggest that the ‘innocence’ argument has done more to undermine support for capital punishment than any other”); Masters, *Executions Decrease*, supra note 75 (explaining the decline in support for the death penalty to its “lowest [point] in two decades” and a “sharp” decline in executions recently to the fact that “[t]wenty-one people have been released from death row in the past three years after DNA tests or other new evidence cast doubt on their convictions,” and quoting statement of Republican Senator John Warner of Virginia that “[t]he number of cases of inmates being taken off death row says to the public that this system has faults and we’ve got to take greater steps to ensure guilt beyond a reasonable doubt”).

78. See supra note 77; NAACP Legal Defense and Educational Fund, Inc., *Death Row U.S.A.* Fall 2001; infra p. 24 & nn.127, 128.

79. Recent events in Oklahoma have aggravated concerns that innocent people have been executed:

When Jeffrey Pierce was convicted of rape in 1986, he lost his freedom and his family. He and his wife decided to divorce and she left Oklahoma to raise their twin infant sons as if he did not exist. To survive in prison, he learned to do two things: mind his own business and lift weights.

But today, after maintaining his innocence throughout the 15 years he spent behind bars, Mr. Pierce, 39, was freed because DNA testing refuted the crucial testimony against him from an Oklahoma City police chemist long accused of shoddy work and now the focus of one of the most wide-ranging investigations into a police laboratory.

* * * * *

Mr. Pierce had been a landscaper who happened to be working near the scene of the rape. The DNA test results that cleared Mr. Pierce, as well as a separate review of his case by the Federal Bureau of Investigation, set in motion the larger inquiry into the chemist, Joyce Gilchrist. Last week,

the federal Justice Department began an investigation while Gov. Frank Keating of Oklahoma ordered a review into every felony conviction linked to Ms. Gilchrist to make certain that no one else has been wrongly convicted.

Among those hundreds of cases are 11 in which the defendant was executed and 12 in which the defendant is on death row. Mr. Keating has expressed confidence that no innocent person has been executed.

The investigations into Ms. Gilchrist, who analyzed forensic evidence like blood, hair, semen and fibers from 1980 until she was promoted in 1994 to a supervisory position, come as other police laboratory scientists are under scrutiny in Illinois, West Virginia and Florida. . . .

Jim Yardely, Forensic Expert Under Scrutiny as DNA Test Frees ‘Rapist’, N.Y. Times, May 8, 2001. See supra notes 2, 50; infra notes 84, 232.

80. See, e.g., Farrell, supra note 77 (noting that “the families of three executed men in Texas and Virginia have gone to court to try to use DNA to prove their relatives were not guilty” after officials refused to make the relevant evidence available for testing); Frank Green, DNA Tests Not Likely After an Execution: Virginia Opposing Third Request of its Kind, Richmond Times-Dispatch, Mar. 26, 2001 (discussing refusal of Virginia to release biological material for testing that would reveal whether three inmates executed for separate offenses in that state were accurately convicted and executed for rape murders); Brooke A. Masters, New DNA Testing Urged in Case of Executed Man: Post, Others Ask Va. Court to Release Evidence, Wash. Post, Mar. 28, 2001, at B1 (discussing thus far unsuccessful efforts of press, charities and members of the family of executed individuals to secure access to DNA-testable biological samples in cases of individuals executed by Virginia, despite consistent claims of innocence); Mr. Washington’s Release, Wash. Post, Feb. 12, 2001 (discussing efforts by “Virginia officials . . . to block posthumous DNA testing that could resolve . . . remaining questions” about guilt or innocence of inmates executed by that state).

So far as we are aware, posthumous DNA testing in cases of executed individuals has occurred only twice in this country:

Frank Lee Smith spent 14 years awaiting execution in Florida. Then, in December [2000], a DNA test cleared him of the 1985 murder of an 8-year-old girl. Unfortunately for Smith, the state opposed DNA testing until six months after he died of cancer on death row at age 52.

* * * * *

Apparently the only time a court anywhere in the country has ordered DNA testing in a case where an execution has already been carried out was last year in Houston County, Ga. That testimony was requested by the [Boston] Globe, which argued that there was an “overriding right to know the result of modern DNA testing on evidence maintained in a death penalty case.” Authorities . . . did not oppose the Georgia request. Testing results have thus far been inconclusive.

Green, supra. On the Frank Lee Smith case, see infra pp. 34-35.

81. See Green, supra note 80 (“The Catholic Diocese of Richmond sought the DNA in the O’Dell and Barnabei cases [both O’Dell and Barnabei consistently proclaimed their innocence before and after trial and in their last words before being executed] using a state law that permits evidence to be donated to a charity

once it is no longer needed. The requests were turned down and, in the O'Dell case, the evidence was destroyed by Virginia Beach Circuit Court order [on request of the local prosecutor.]; Associated Press State & Local Wire, DNA Evidence in Coleman Case to Stay in California, Aug. 25, 2001 (discussing ruling of Virginia circuit judge that the *Boston Globe*, *Washington Post*, *Richmond Times-Dispatch*, *Virginian-Pilot* and Princeton, N.J.-based Centurion Ministry would not be permitted to test DNA of Roger Keith Coleman who was executed in 1992 for a rape murder he consistently contended he did not commit; noting that the “Virginia attorney general’s office [has] opposed the new testing,” and that a “Virginia court has never allowed DNA testing on evidence in a case where the convicted person has been executed [despite a string of] post-execution requests [including] in the cases of Joseph O'Dell III, who was executed in 1997, and Derek R. Barnabei, who was executed last year”). Cf. *id.* (quoting the clerk of the Norfolk Circuit Court, who has custody of the files in the Barnabei case: “As long as I am the clerk, we will not destroy the evidence without a court order.”). See also Brooke A. Masters, Va. Evidence Destroyed Despite Warnings to Clerk, *Wash. Post*, Oct. 18, 2001 (“An Arlington courthouse clerk threw away all the evidence from a 1999 death penalty case in violation of Virginia law, despite warnings from two colleagues that the material contained DNA and that the inmate’s appeals were pending, according to court documents filed yesterday. . . . At least 50 exhibits, including the murder weapon, were thrown out May 23.”).

States—including Louisiana and Virginia in 2001—and localities are even adopting laws and policies permitting the systematic destruction of evidence that could confirm or disconfirm the accuracy of executions. See *id.* (“A new law passed in the recent [Virginia] General Assembly says evidence in a capital case need only be kept by the state’s Division of Forensic Science until the execution has been carried out.”); Barbara Bradley, DNA Testing in Crime Cases Causing Distrust in the Criminal Justice System, *NPR Morning Edition*, Aug. 29, 2000, transcript available at 2000 WL 21481402 (“DNA has become one of the most powerful tools to prove the guilt or innocence of a criminal defendant. And in recent years, dozens of convicted prisoners have been released after the original biological evidence was tested again. But now prisoners wanting to take advantage of this new science are hitting a roadblock. Police and courts across the country are destroying the biological evidence that could determine whether a person has been wrongly convicted.”); also discussing decision of officials in Houston, Texas to destroy biological evidence contained in court files of scores of rape and rape-murder cases).

82. See Paul F. Enzinna, Afraid of a Shadow of a Doubt (Op-ed), *Wash. Post*, May 7, 2000, at B8 (quoting a Virginia prosecutor’s successful argument in court that Virginia should be allowed to destroy DNA samples that could have proven that Joseph O’Dell did not commit the rape murder for which he was executed in 1997).

83. Cf. Editorial, Review the Errors, *Ariz. Daily Star*, June 29, 2001 (editorial endorsing proposal of Arizona Supreme Court Chief Justice Thomas Zlaket to establish a special commission to determine the circumstances under which wrongful convictions occur in Arizona); Carol Sowers, Arizona Lawyers Seek ‘Innocence’ Panel, *Ariz. Repub.*, Nov. 12, 2001 (“Two Valley Lawyers want Arizona to be the first state with an Innocence Commission, a public panel that would investigate when the wrong people are sent to prison.”).

In every case in which an innocent person is shown to have been wrongfully convicted of murder in Canada (which does not have the death penalty), an ad hoc commission is appointed under the direction of a former Supreme Court Justice or other respected jurist or lawyer, to inquire into what went wrong and to propose reforms. See James Lockyer, *Guilt Revisited: A Comparative Perspective on Canada, the United Kingdom and the United States*, talk delivered at DNA and Human Rights: An International Conference, University of California, Berkeley, CA. Apr. 27, 2001. For the report of the first such commission—the Kaufman Commission, which examined the wrongful conviction of Guy Paul Morin, discovering serious abuses in the use of jail house informants and proposing ameliorative legislation that a number of Canadian provinces have adopted—see www.attorneygeneral.jus.gov.on.ca:80/morin/morin.htm. The United Kingdom

has established an ongoing Criminal Cases Review Commission with authority to examine the accuracy of convictions that have become final in the relevant nations and to propose that the Court of Appeal overturn ones shown to have reached a false conclusion. The Commission thus far has referred to the Court of Appeal about 70 wrongful conviction recommendations, over 25 in homicide cases; the court has accepted nearly all of them, including three in cases of individuals wrongfully executed in the 1950s and 1960s before the UK abolished the death penalty. See James Lockyer, *Associates in the Defense of the Wrongfully Convicted*, Canada, <http://globetrotter.berkeley.edu/humanrights/DNA/>.

84. Arnold Hamilton, *Chemist's Errors Stir Fear: Did Oklahoma Execute Innocent?*, *Dallas Morning News*, Oct. 22, 2001. The Oklahoma Attorney General is currently conducting a study of hundreds of convictions based on testimony by a compromised laboratory employee, Judith Gilchrist. See *supra* notes 1, 50, 70; *infra* note 232. Despite strong indications that Gilchrist fabricated evidence in at least one case that led to an execution, the state has given the lowest priority to cases in which the defendant was executed after being convicted in part based on Gilchrist's testimony, and has refused in the mean time to permit others to examine the forensic evidence. See, e.g., Hamilton, *supra* ("In Oklahoma, the 12 pending death row cases that Ms. Gilchrist worked on are being reviewed first by the state task force, according to OSBI spokeswoman Kym Koch. Those with sentences of life without parole will be reviewed next, followed by those with lesser sentences. The 11 Gilchrist cases in which inmates have already been executed will be considered last, if at all."); Lois Romano, *Police Chemist's Missteps Cause Okla. Scandal*, *Wash. Post*, Nov. 26, 2001 ("the most sensitive aspect of the [Gilchrist] investigation is whether the state of Oklahoma, relying on Gilchrist, may have executed an innocent man—or someone who would have been sentenced to life without her critical testimony. Twenty-three capital cases have been identified in which Gilchrist provided testimony. Of those, 11 convicted murderers have already been put to death, and 12 sit on death row. . . . As the Gilchrist investigation lumbers past its seventh month, the state multi-agency task force has agreed to test only three of the 12 current death row cases connected to Gilchrist—and none of the 11 in which the perpetrators have already been executed. . . . The Oklahoma Indigent Defense System, though, wants to conduct forensic testing on several additional Gilchrist death row cases, and seven of the 11 individuals already executed—but has met resistance from state Attorney General Drew Edmondson."). For further background, see Henry Weinstein, *Evidence Questioned in Execution: Police Memos Contradict Chemist's Forensic Testimony at a 1982 Oklahoma Murder Trial*, *L.A. Times*, Aug. 30, 2001:

The investigation of a controversial Oklahoma City police chemist has produced evidence raising questions about whether she testified falsely in the 1982 rape and murder trial of a man who was executed last year, protesting his innocence to the end.

On Wednesday, Oklahoma Atty. Gen. Drew Edmondson released two memos written last month by four Oklahoma City Police Department forensic scientists saying tests they had conducted of material found at the murder scene contradicted testimony given by Joyce Gilchrist [that semen matching Johnson's was found at the crime scene]. . . .

Edmondson, who released the memos after they were described in [newspaper] stories . . . , said it was possible that "we might reexamine the evidence down the road." Now, however, Edmondson said his office was focused on reviewing numerous cases in which Gilchrist testified and the defendant received a long prison term or a death sentence not yet carried out. . . .

[A lawyers working on Johnson's behalf] . . . filed a lawsuit seeking access to all records in the case. The city has opposed this request and has acknowledged that it had lost some of the records. . . .

[DNA technology only became available after Johnson's trial, at which point he] attempted to secure DNA testing of the biological evidence. The state attorney general's office resisted, saying that under federal laws governing death penalty appeals, Johnson was ineligible for testing. Federal judges agreed, so no testing was done.

85. This is not to say that officials never hide actions that threaten or take innocent life. But when they do, their suppression of the facts is typically thought to be improper. This is not so in the death penalty context, which makes it unique.

86. Many private enterprises no longer distinguish between quality control, safety and other kinds of inspections for faults, recognizing that high rates of any kind of error signal a high risk of all kinds of error and thus are likely to indicate that the production process itself is inefficient. See, e.g., Frederick H. Abernathy, et al., *A Stitch in Time: Lean Retailing and the Transformation of Manufacturing—Lessons from the Apparel and Textile Industries* (1999).

87. Of the 4546 death sentences imposed during the 1973-1995 study period that were finally reviewed on direct appeal, state supreme courts found serious error and reversed 1852 (41%) of them. The raw numbers reported here, but not the percentages, are slightly different from those reported in *A Broken System, Part I*. Since that Report was published, we have continued to check the numbers, including based on new information received from readers of the first report, to assure that our figures are as accurate as possible. As here, this has led to modest adjustments, none of which affect our conclusions. See *infra* note 90, 174.

88. See *infra* pp. 17-18 (explaining why the 10% figure substantially understates how frequently state post-conviction courts discovered reversible capital error).

89. See *supra* pp. 6-7.

90. See *supra* note 87 (explaining why some of the numbers in the National Report Card are slightly different from those in the National Report Card in *A Broken System, Part I*). Although the raw numbers changed slightly, the percentages are the same, with three exceptions: The overall reversal rate for only direct appeal and state post-conviction, excluding errors discovered on federal habeas dropped from 47% to 46%; the reversal rate for only direct appeal and federal habeas reversals, but not those occurring on state post-conviction, rose from 64% to 65%; and the percentage of cases awaiting direct review rose from 21% to 22%.

91. See *infra* pp. 17-18 (explaining why the reversal rate at the state post-conviction stage is probably substantially higher than 10%).

92. At the state post-conviction stage, the phase of the trial affected by the reversal is known in 336 cases. Of those reversals, 126 (37.5%) overturned phase-one verdicts, and 210 (62.5%) overturned phase-two verdicts.

93. At the federal habeas stage, the phase of the trial affected by the reversal is known in 239 cases. Of those reversals, 102 (43%) overturned phase-one verdicts, and 139 (58%) overturned phase-two verdicts. (These figures sum to 101% because there were a small number of cases in which relief was granted on claims undermining both phase-1 and phase-2 verdicts.)

Latzer and Cauthen reviewed a sample of undifferentiated state direct appeal and state post-conviction reversals of capital verdicts occurring between 1990 and 1999, concluding that 39% affected the first-phase determination, while 61% affected the second-phase determination. See Barry Latzer & James

N.G. Cauthen, *Capital Appeals Revisited*, *Judicature*, Sept.-Oct. 2000, at 64, 66. As we develop elsewhere, Latzer & Cauthen's sampling technique was skewed against finding phase-one error, see James S. Liebman, Jeffrey Fagan & Valerie West, *Death Matters: A Reply to Latzer and Cauthen*, *Judicature*, Sept.-Oct. 2000, at 76 (finding that 83% of cases Latzer and Cauthen's sampling technique failed to count were phase-one reversals), and they failed to distinguish between direct appeal and state post-conviction reversals. Together, our more complete state post-conviction data, see *supra* note 92 and accompanying text, and Latzer and Cauthen's estimates after accounting for the effect of their sampling biases, suggest that well more than 40% of direct appeal reversals during the 1990-1999 period were phase-one reversals. (Together, the fact that their bottom-line number for an undifferentiated set of reversals from both the direct appeal and state post-conviction stages is based on a systematic undercount of first-phase reversals as a result of biased search criteria, and the fact that the number they come up with is close to the accurate number for the state post-conviction phase *taken by itself*, suggest that an accurate count of all direct appeal reversals *taken by themselves* must be well above that number.)

94. See, e.g., *Lowenfield v. Phelps*, 484 U.S. 231 (1988) (states may impose death sentences only for murders that are capitally aggravated, but may make that determination at either the first or second phase of capital trials).

95. See Jeffrey Fagan, James S. Liebman & Valerie West, *Death Is the Whole Ball Game*, *Judicature*, Nov.-Dec. 2000, at 144-45; *Death Matters*, *supra* note 93, at 72-77, 99.

96. At the state post-conviction stage, the part of the trial tainted by reversal is known in 336 cases. Of those reversals, at least 166 (49%) undermined the decision whether the defendant committed aggravated murder, because they tainted (1) the finding of guilt of some level of murder, as determined at the first-phase of trial, and/or (2) the finding that the murder was capitally aggravated, as determined at the first phase of trial in a minority of states and at the second phase of trial in most states. At the federal habeas corpus stage, the basis for reversal is known in 239 cases. Of those reversals, at least 122 (51%) undermined the decision whether the defendant committed aggravated murder, for one or both of the same two reasons.

These figures substantially undercount reversals of decisions determining the level of offense committed. The data we collected reveal the number of death verdicts reversed based on prosecutorial suppression of evidence and incompetent lawyering at the second and third phases of trial but do not reveal the precise determinations made at those stages that were compromised by incompetent defense counsel or prosecutorial suppression of evidence. To assure a conservative estimate of the proportion of guilt-related reversals, we accordingly count all second-phase reversals on these two grounds as "sentence-related" error, even though some of these reversible errors in fact tainted the finding of aggravated murder and thus should be counted as guilt-related error.

Collecting information on the basis for reversal, and the aspect of the capital trial that it compromised, is costly and time-consuming. The limited resources available for our research permitted us to collect that information for the second stage of review (state post-conviction) and the third stage of review (federal habeas) but not for the first stage of review (direct appeal), where the number of reversals is larger. For reasons discussed at pp. 45-47 below, it is unlikely that the proportion of reversals affecting the determination of guilt of aggravated murder is greatly different from the nearly identical rates for the two later review stages.

97. These points are developed at greater length in *Death Matters*, *supra* note 93; *Death Is the Whole Ball Game*, *supra* note 95.

98. *Woodson v. North Carolina*, 428 U.S. 280, 305 (1976).

99. See *supra* note 86 and accompanying text.

100. See *infra* note 680 (noting statistically significant tendency of federal judges to use “per curiam” opinions, which often are unpublished decisions, more often when reversing than when affirming death verdicts). For evidence of efforts on the part of state reviewing courts to avoid controversy generated by reversals of capital verdicts, see *infra* pp. 194, 218-19, 236, 257.

101. See James S. Liebman, Jeffrey Fagan & Valerie West, *A Broken System (Part I): Error Rates in Capital Cases, 1973-1995*, at 19-20, 26-27 & nn.39, 132; App. C, pp. C-1 to C-2 (2000) [hereinafter, *A Broken System, Part I*].

102. Mr. Shiffman graciously provided his data to the lead author of this Report. His report on his study appeared in the *Nashville Tennessean* on July 23, 2001.

103. Nationally, the proportion of death verdicts passing inspection on direct review was 59%: 100% - 41% reversed on direct appeal = 59%. If the 300% Tennessee adjustment holds nationally, the proportion passing both direct appeal and state post-conviction inspection is 41%: 59% passing direct review inspection - 30% of those 59% that were reversed on state post-conviction = $59\% - 18\% = 41\%$. And in that event, the proportion passing all three inspection stages is 25%: 41% passing inspection at the first two stages - 40% of those 41% that were reversed on federal habeas = $41\% - (.40 \times 41\%) = 41\% - 16\% = 25\%$. The overall error rate, in that case, is 75%.

To see how large a difference it makes to account for the many verdicts that got stuck in the review process and were not finally reviewed at the state post-conviction stage, consider a state that imposed 170 death verdicts during the study period, 41% (or 70) of which were reversed on direct appeal, leaving 100 verdicts *available* for state post-conviction review. If there were 10 state post-conviction reversals in the state during the period, we conservatively estimated the error rate as 10% (10/100). But if at least 20% of those 100 cases were not in fact finally reviewed at the state post-conviction stage during the study period (as is almost certainly true of all states), the *actual* rate of reversible error at the state post-conviction stage was at least 30% higher than our estimate. ($10/75 = 13\%$. $13\% - 10\% = 3\%$. $3/10 = 30\%$.) If, instead, 40% of the available cases were unreviewed (as was certainly true in a number of states), then *A Broken System* underestimated the error rate by 70%. ($10/60 = 17\%$. $17\% - 10\% = 7\%$. $7/10 = 70\%$.)

Our best estimate is that around 45% of all verdicts available for state post-conviction review during the study period were not finally reviewed during the period. We know that a total of 2694 verdicts cleared direct review and thus were available for state post-conviction and federal habeas review during the study period. We also know that 257 of those verdicts were reversed on state post-conviction review, leaving 2437 verdicts (2694-257) to be accounted for. Of those, only 598 (25%) were finally reviewed on federal habeas. The remaining 1839 verdicts (2437-598), comprising fully 68% of the verdicts that were available for some type of post-direct appeal review during the study period (1839/2694), got stuck in the system either at the state post-conviction phase or the federal habeas phase. Given the steadily and substantially shrinking pools of cases clearing each of the three stages, it is reasonable to think that most of the unaccounted for verdicts were stuck in state post-conviction process, while a much smaller proportion of the original verdicts were stuck in federal habeas proceedings. It thus is reasonable (if probably conservative) to allocate two-thirds of the unaccounted for verdicts ($.67 \times 1839 = 1232$) to the state post-conviction phase, and one-third (607) to the habeas stage. On that assumption:

- The proportion of verdicts available for review on state post-conviction that were not reviewed is 46% (1232/2694).
- The number of verdicts finally reviewed on state post-conviction is 1462 (2694-1232).
- The state post-conviction reversal rate is 18% (257/1462)—which is 80% higher than we estimated in *A Broken System*.
- And the overall reversal rate, combining all three review stages, is 71%, not the 68% we conservatively report. (100% - 41% reversed on direct appeal = 59% available for state post-conviction review. 59% - 18% (59%) reversed on state post-conviction = 48% available for federal habeas review. 48% - 40% (48%) reversed on federal habeas = 29% available for execution, and 71% reversed.)

See also *infra* note 315.

104. 408 U.S. 238 (1972).

105. See *Roberts v. Louisiana*, 428 U.S. 325 (1976); *Roberts v. Louisiana*, 431 U.S. 633 (1977).

106. See *Woodson v. North Carolina*, 428 U.S. 280 (1976).

107. See *Lockett v. Ohio*, 438 U.S. 586 (1976).

108. Those states are Alabama, California, Indiana, Kentucky, Mississippi, Montana, New Mexico, South Carolina, Tennessee, Virginia, Washington and Wyoming.

109. See *State v. Voclain*, 369 So.2d 190 (La. 1978); *State v. Adams*, 367 So.2d 8 (La. 1978); *State v. McGraw*, 366 So.2d 1278 (La. 1978); *State v. Jones*, 363 So.2d 714 (La. 1978); *State v. Willie*, 360 So.2d 813 (La. 1978); *State v. Yates*, 357 So.2d 541 (La. 1978); *State v. Forrest*, 356 So.2d 945 (La. 1978); *State v. Hamilton*, 356 So.2d 422 (La. 1978); *State v. Brooks*, 351 So.2d 1197 (La. 1977); *State v. Smith*, 351 So.2d 1191 (La. 1977); *State v. Buggage*, 351 So.2d 97 (La. 1977); *State v. Sheppard*, 350 So.2d 615 (La. 1977); *State v. Preston*, 349 So.2d 1252 (La. 1977); *State v. Roberts*, 350 So.2d 130 (La. 1977); *State v. Gleason*, 346 So.2d 206 (La. 1977); *State v. Williams*, 346 So.2d 204 (La. 1977); *State v. Williams*, 343 So.2d 1026 (La. 1977); *State v. Perkins*, 343 So.2d 730 (La. 1977); *State v. MacHeard*, 343 So.2d 730 (La. 1977); *State v. Tyler*, 342 So.2d 574 (La. 1977).

110. The prisoners whose death verdicts were overturned on successive petitions during the study period, but whom we counted as having had their death verdicts affirmed are: Levis Aldridge (Florida: 925 F.2d 1320), Stephen Booker (Florida: 922 F.2d 633), Frank Smith (Florida: 61 F.3d 815), Carl Songer (Florida: 769 F.2d 1488), and Willie Watson (Louisiana: unpublished).

We also counted at least eight death verdicts as having been affirmed at all three stages, though the defendants were later found to be innocent and released. Among those were some whose exoneration occurred as a result of a post-study-period successive federal habeas petition. See *infra* note 128 (listing the eight cases); *infra* pp. 25-27 (discussing the exoneration of Lloyd Schlup, whose death verdict is counted in our study as having been affirmed at all three review stages, but who later was exonerated in a successive federal habeas proceeding).

111. See *supra* p. 8 n.* for a discussion of the slightly different rounding procedures used in this example for ease of exposition, and in the National Report Card at p. 9 above.

112. See Valerie West, James S. Liebman & Jeffrey Fagan, *Look Whose Extrapolating?: A Reply to Hoffmann*, 76 *Ind. L.J.* 951 (2001).

113. See *infra* pp. 91-93 & Figure 10.

114. Between 1973 and 1995, federal courts finally reviewed about 600 death verdicts. As of 1995, however, hundreds of other verdicts imposed in the same years were still undergoing federal habeas review, and were not counted in our reversal rates. Figure 10, p. 93 below, shows (1) that flawed verdicts are over-represented in the part of each sentence-year cohort of cases that we did not count because they still were awaiting review as of the cut-off date, and (2) that federal habeas cases without reversible error are over-represented in the verdicts we counted. It thus is likely that the reversal rates we report for each sentence-year cohort of cases are lower than they eventually turn out to be, once all verdicts in the cohort are finally reviewed.

115. Although for the reasons discussed at pp. 17-18 above, we substantially underestimated state post-conviction reversal rates, we have no reason to believe that our cut-off date, and our decision not to extrapolate outcomes of decisions delayed beyond that cut-off date, either exacerbated or moderated this undercount.

116. See Hoffmann, *supra* note 67, at 957.

117. See, e.g., *id.*

118. See *id.* at 958.

119. See *infra* p. 36.

120. See, e.g., *Williams v. Taylor*, 529 U.S. 362 (2000).

121. See, e.g., *Mitchell v. Kemp*, 762 F.2d 886 (11th Cir. 1985) (denying habeas relief to capital prisoner whose lawyer made only a minimal pretrial inquiry into whether the death penalty was appropriate in his case, on the ground that the incomplete investigation did not prejudice the defendant (discussed in *Mitchell v. Kemp*, 483 U.S. 1026 (1987) (Marshall, J., dissenting from denial of certiorari)); *Glen v. Tate* 71 F.3d 1204 (6th Cir. 1995) (despite conclusion that defense counsel were incompetent—they “never took the time to develop” evidence about the mental impairments of the 19-year-old brain damaged and “mentally deficient” defendant who acted under the influence of his older brother; “never spoke to any of [the defendant’s] numerous brothers and sisters,” “never examined his school records,” and “never examined his medical records (including an emergency room record prepared after he collapsed in court one day) or records of mental health counseling they knew he had received”—court upholds capital conviction citing a lack of prejudice); *Agan v. Singletary*, 12 F.3d 1012 (11th Cir. 1994) (although defense counsel provided incompetent representation—pleading defendant guilty to a capital offense without any agreement on sentence, spending only seven hours total investigating the case, never contacting the attorney who had previously handled the case, never investigating the defendant’s mental disabilities which were well documented, and never examining state investigation file indicating the investigator’s belief that someone else committed the killing and containing correspondence from the victim documenting death threats from other suspects—court upholds the capital conviction, citing a lack of prejudice); *Brimmer v. State*, 1998 WL 612888 (Tenn. Cr. App. Sept. 15, 1998) (denying state post-conviction relief from capital conviction imposed at trial where the defendant was represented by a lawyer who admittedly was intoxicated and abusing drugs throughout the trial period on the ground that lawyer’s incompetence did not prejudice the defendant at the guilt-innocence stage of trial); *infra* note 160 (discussing several similar cases in Texas).

122. See, e.g., *Strickler v. Green*, 524 U.S. 978 (1998) (despite the fact that police and prosecutors failed to disclose (1) that the central identification witness against the defendant had given conflicting statements to police, including that she could not identify the defendant, and (2) that the witness had steadily embroidered her story during a series of police interviews, conforming it to the police officers’ theory of the case, Court upholds capital verdict, citing a lack of prejudice); *Townes v. Murray* 68 F.3d 840 (4th Cir. 1995) (citing waiver, court refuses to consider claim that the trial court improperly failed to determine if the mentally impaired defendant was competent to represent himself at the sentencing phase of his capital trial after his trial attorney withdrew, because his appellate lawyer’s brief on the issue in the Virginia Supreme Court—which clearly challenged the trial court’s failure to determine if the defendant could competently represent himself at the trial for his life—failed to specify the particular time during the trial when the trial court’s inquiry should have been made); *Smith v. Zant*, 887 F.2d 1407 (11th Cir. 1989) (although the confession the police took from the mentally retarded defendant violated the Constitution because the defendant did not

understand that he had a right to consult a lawyer before talking to the police, and although the confession was the prosecution's central evidence of capital murder and was inconsistent with defense evidence that the defendant acted in response to a physical provocation (in which case the offense was manslaughter, not murder, much less *capital* murder), the court upholds the capital conviction, ruling that the admission of the unconstitutional confession was harmless); *Reams v. State*, 909 S.W.2d 324 (Ark. 1995) (reviewing 18-year-old African American defendant's capital sentence for a murder committed by a co-defendant during a robbery; although the verdict was imposed by an all-white jury after the prosecutor exercised peremptory challenges to strike all the potential black jurors, and although faulty instructions may have prevented the jury from considering defendant's youth and other extenuating circumstances as bases for a sentence less than death, the court upholds the verdict, noting that "even in death penalty cases, a defendant must have raised the allegations of error at the trial court level by having made a specific, timely objection" and ruling that because of counsel's failure to object at trial, errors could not be corrected on appeal); *Cannaday v. State* 455 So.2d 713 (Miss. 1984) (although jailer's questioning of 16-year-old retarded defendant outside the presence of her attorney clearly violated the Constitution, and although the statement was extremely inflammatory as reported out of context to mean something different from what the young woman evidently meant (according to a third-party witness), court upholds capital conviction, citing lack of prejudice); *Commonwealth v. LaCava*, 666 A.2d 221 (Pa. 1995) (although the prosecutor repeatedly made irrelevant and highly prejudicial and inflammatory statements to the jury at both the guilt-innocence and sentencing phase of trial—including that because the defendant was known to sell drugs, he should be assumed to have sold them to children (there was no evidence that the defendant sold drugs to children, and the offense charged had nothing to do with drugs or drug dealing)—the court upholds the capital conviction, finding the errors harmless and ruling that, although the defendant's trial was flawed, it was fair enough).

123. See, e.g., Ken Armstrong & Maurice Possley, *Break Rules, Be Promoted*, Chi. Trib., Jan. 14, 1999, at N1 ("Between 1993 and 1997, there were 167 published opinions in which the Illinois Appellate Court or Illinois Supreme Court found that prosecutors committed some form of misconduct that could be considered harmless. In 122 of the cases—or nearly three out of four times—the reviewing court affirmed the conviction, holding that the misconduct was 'harmless'"; also quoting retired Illinois appellate judge Dom Rizzi, criticizing courts' and chief prosecutors' tendency to ignore "troublesome pattern[s]" of misconduct); Ken Armstrong & Steve Mills, *Justice Derailed*, Chi. Trib., Nov. 14, 1999, at N1; Spencer Hunt, *Clouded Cases: Prosecutors' Conduct Risks Reversals*, Cincinnati Enquirer, Sept. 10, 2000, at A1 (reporting, based on study of court records, that, although the Ohio Supreme Court "repeatedly has criticized [Cincinnati] prosecutors for making improper courtroom statements to win 14 death penalty cases over the past 12 years," and "has written at least four lengthy opinions since 1988 telling prosecutors to stop the misconduct, the record shows justices are more than willing to forgive these mistakes and uphold death sentences" on grounds of a lack of prejudice—as the court has done in 13 or 14 cited cases, with the 14th still under review; also quoting the Chief Justice of the Ohio Supreme Court, stating: "We better stop complaining about it if we're not going to do something about it."); *Prosecutorial Restraint: Death Penalty Allows No Margin for Error*, Columbus Dispatch, July 15, 2000, at 6A (citing a number of recent capital cases in which the Ohio Supreme Court had "express[ed] frustration" and "mounting alarm" over the increasing incidence of misconduct by prosecutors" and trial judges; in nearly all cases, however, the error was found harmless and the death verdict was upheld).

124. We also know (1) that over four-fifths of the death verdicts approved on federal habeas raised claims of ineffective assistance of counsel or prosecutorial suppression of evidence, and (2) that one of the two main reasons these claims are denied is a lack of "prejudice"—*i.e.*, that the court would not reverse due to malfeasance or nonfeasance by defense counsel or the prosecutor because the defendant could not prove that the misconduct probably changed the outcome of the trial. Regrettably, however, collecting information on

whether claims were denied on “no prejudice” grounds was beyond our capacity, given finite time and resources, and we thus cannot say how many verdicts were approved on “no prejudice” grounds, in addition to those approved on “harmlessness” grounds.

125. See, e.g., *Gray v. Netherland*, 518 U.S. 152 (1996); *Sawyer v. Whitley*, 501 U.S. 333 (1992); *Coleman v. Thompson*, 501 U.S. 722 (1991); *Smith v. Murray*, 477 U.S. 527 (1986); *Dugger v. Adams*, 489 U.S. 214 (1989); Steve Mills & Ken Armstrong, *Lawyers Err, and Appeals Door Shuts*, Chi. Trib., July 10, 2001, at N1 (“After Jeffrey Rissley landed on Illinois’ Death Row, he seemed to have solid grounds for an appeal: His trial lawyer, a probate and real estate attorney, had advised him to plead guilty without bargaining for any leniency—a move practically unheard of in capital cases. But the attorney advising Rissley about his appeal misinterpreted a new state law, leading Rissley to file the appeal six days after a crucial deadline. That tardy filing prompted the Illinois Supreme Court to rule that Rissley . . . had forfeited the right to raise his trial attorney’s incompetence—virtually his only issue—in further appeals.”); *infra* note 216 (discussing *Giarratano* case).

126. Number and Percent of Cases Where Default or Harmless Error Was Basis for Denying Federal Habeas Relief

<i>Reason for Denial</i>	<i>Number</i>	<i>%</i>
Waiver	113	32
Harmless Error	63	18
One or Both of Above	156*	44
Total Denials of Relief	358	

* The above two figures do not sum to 156 because procedural default and harmless error were sometimes found in the same case. Source: HCDB.

127. On the 95th exoneration, see *Man on Death Row for Five Years Found Innocent*, Associated Press, May 26, 2001:

A north Alabama man who spent five years on prison’s death row has been found innocent at retrial. Gary Wayne Drinkard, 455, of Falkville was released Friday after a Morgan County jury found him innocent of capital murder charges in the robbery-slaying of Decatur businessman Dalton Pace.

Drinkard’s first conviction had been overturned by the Alabama Supreme Court because prosecutors were allowed to talk about Drinkard’s prior criminal history. . . .

On the 96th exoneration, see Pat Leisner, *Jurors Acquit Man in Slaying Retrial*, AP Online, June 6, 2001:

A 30-year-old man who had faced the death penalty was acquitted [in Florida] of two counts of first-degree murder Wednesday in a retrial.

Joaquin Martinez, a Spanish citizen living in the United States, fought back tears at the jury’s verdict. His mother, Sara Martinez, wept openly.

* * * * *

Martinez spent nearly three years on death row until the Florida Supreme Court overturned his conviction and death sentence and ordered a new trial.

There was no physical evidence linking Martinez to the slayings in a case that attracted the attention of the Pope and the King of Spain

On the 97th exoneration, see Associated Press, *Sheets Released from Death Row, Lincoln (Nebraska) J. Star*, June 13, 2001 (“Former death-row inmate Jeremy Sheets . . . was released from prison Tuesday, three years after he was ordered to die for the kidnaping, rape and killing of an Omaha girl. . . . His release came after the Nebraska Supreme Court threw out a taped statement used to convict Sheets Without the taped statement, prosecutors said, they did not have enough other evidence to proceed with another trial.”).

On the 98th exoneration, see Raymond Bonner, *Death Row Inmate is Freed After DNA Test Clears Him*, N.Y. Times, Aug. 24, 2001:

Charles Fain has been on death row for almost 18 years for the rape and murder of a 9-year-old girl [Daralyn Johnson] who was snatched off the street in Nampa [Idaho], a small town west of here.

But this afternoon, Mr. Fain, 11 days shy of his 53rd birthday, walked out of the maximum security prison here into the blazing sun, a free man. Two hours earlier, a state judge ordered the charges against him dismissed on the basis of DNA tests indicating that hairs found on the girl’s body, which had been used to convict Mr. Fain, were not his.

* * * * *

“Justice requires the action we have taken today,” David L. Young, the Canyon County prosecutor, said today at a news conference “It also requires that we do everything we can to solve this case.”

Mr. Young added, “The killer has not yet been apprehended.”

Today the Johnson family seemed to accept Mr. Fain's release.

“We would like to say we are in complete support of the judicial system and all those involved in the reinvestigation of this case,” the family said in a statement. “We are confident that we will have closure and that all those involved will be brought to justice.”

See also Henry Weinstein, *DNA Tests Give Idahoan Freedom After 17 Years on Death Row*, L.A. Times, Aug. 24, 2001 (quoting Fain’s lawyer saying, “This case demonstrates that those who presuppose that only the guilty get the death penalty are wrong.”).

On the 99th exoneration, see Phil Long & Amy Driscoll, *Inmate on Death Row Goes Free After 17 Years*, Miami Herald, Jan. 4, 2002:

A chance discovery two years ago of an old legal transcript in a lawyer's files led to freedom

Thursday for Juan Melendez—17 years after he was sent to Florida’s Death Row for a murder another man claimed to have committed.

Polk County prosecutors effectively set Melendez free when they announced Thursday that they do not have sufficient evidence to re-try him for the 1983 slaying of a Central Florida beauty school owner.

* * * * *

Melendez, 50, was sentenced to death in 1984 for the murder of Delbert “Mr. Del” Baker in his Auburndale beauty salon. Melendez lost several rounds of appeals and his death sentence was upheld. He was nearing the end of his appeals when his former defense lawyer, Roger Alcott, discovered a key transcript as he moved old boxes of files following his appointment as a Polk County circuit judge in early 2000.

The transcript details a conversation taped about a month before Melendez’s trial. On the tape, Vernon James, a now-deceased witness in the case, admitted being involved in the murder and said that Melendez was not at the scene. . . .

Melendez’s appellate lawyers . . . [found] that James, once a suspect in the murder, had told up to 20 other people—including a former law enforcement officer—of his involvement in the murder. Some said he had confessed to the killing.

Armed with new evidence, the lawyers returned to state court to appeal Melendez’s conviction. . . . Circuit Judge Barbara Fleischer . . . found that the trial prosecutor, Hardy Pickard, had failed to disclosed potentially damaging information to the defense, including serious inconsistencies in statements by John Berrien, one of two major state witnesses. [Berrien subsequently recanted his testimony.]

Additionally, the judge said Pickard misled the jury about testimony from the other main state witness, David Luna Falcon, by telling jurors that Falcon had “nothing to gain” from testifying. But Falcon, who testified at trial that Melendez had confessed, had struck a deal with prosecutors to reduce his own prison time in exchange for testimony.

* * * * *

The new evidence, along with Pickard’s withholding of information, “combine to undermine the confidence in the outcome of the defendant’s original trial,” [Judge Fleischer] concluded.

128. Among the factually or legally innocent inmates whose capital verdicts were affirmed at *all three* stages, who thus were approved for execution and in whose cases *A Broken System* assumes there were *no* errors at all, are: Herbert Bassette (VA), Paris Carriger (AZ), Ronald Monroe (LA), Donald Paradis (ID), Anthony Porter (IL), Lloyd Schlup (MO), Frank Lee Smith (FL), Joseph Spaziano (FL), and Earl Washington (VA). Among the factually or legally innocent inmates whose capital verdicts were affirmed at *two* stages, and in whose cases *A Broken System* assumes there were no errors at both those stages, are: Blazak (AZ), Bowen (OK), Brown (FL), Clemmons (MO), Creamer (GA), Drake (GA), Fain (ID), Guerra (TX), Harris (WA), Jacobs (FL), Jent (FL), Knapp (AZ), Kyles (LA), Macias (TX), Melendez (FL), Miller (FL), Wallace (GA), and Williamson (OK). Among the factually or legally innocent inmates whose capital verdicts were affirmed

at *one* stage, and in whose cases *A Broken System* assumes there were no guilt-innocence errors at that stage, are: Adams (TX), Bloodsworth (MD), Burrell (LA), Burrows (IL), Brandley (TX), Cobb (IL), Cook (TX), Cruz (AZ), Cruz (IL), Deeb (TX), Dexter (MO), Farmer (CA), Gauger (IL), Graham (LA), Hernandez (IL), Jimenez (NV), Jimerson (IL), Jones (CA), Jones (IL), Lucas (TX), McManus (TX), Miller (OK), Miranda (NV), Mitchell (TX), Munson (OK), Nelson (GA), McMillian (AL), Nieves (PA), Peek (FL), Robison (AZ), Smith (IL), Smith (LA), Smith (IN), Dennis Williams (IL), and Darby Williams (IL).

129. *State v. Schlup*, 724 S.W.2d 236, 241-42 (Mo. 1987).

130. *Schlup v. State*, 758 S.W.2d 715, 717 (Mo. 1988).

131. *Schlup v. Armontrout*, 941 F.2d 631, 639 (8th Cir. 1991).

132. Schlup used a procedure allowing a small class of prisoners with newly discovered evidence to file a “successive” petition raising claims of error that courts at the three regularly available review stages had previously denied. (Because successive petitions are not available as of right, our study did not count reversals occurring through this procedure. See *supra* pp. 19.) *In 1996, Congress forbade all such successive petitions.* See 28 U.S.C. § 2244(b).

It is in the nature of valid innocence claims like Schlup’s that they can be proved only through successive litigation in which testimony at each prior stage is revealed to be false by newly discovered evidence that became relevant for the first time when the false testimony was given. Since 1996, therefore, it has been in the nature of valid innocence claims like Schlup’s that existing court review mechanisms cannot be relied upon to reveal innocence.

133. Brooke A. Masters, *Missteps On Road To Injustice; In Va., Innocent Man Was Nearly Executed*, Wash. Post, Dec. 1, 2000, at A1.

134. Brooke A. Masters, *DNA Clears Inmate in 1982 Slaying*, Wash. Post, Oct. 3, 2000, at A1.

135. *Washington v. Commonwealth*, 323 S.E.2d 577, 585-86 (Va. 1984).

136. See Masters, *Missteps on Road to Injustice*, *supra* note 133.

137. *Washington v. Murray*, 952 F.2d 1472, 1477-78 (4th Cir. 1991).

138. *Washington v. Murray*, 4 F.3d 1285, 1290 (4th Cir. 1993).

139. *State v. Porter*, 489 N.E.2d 1329, 1335-36 (Ill. 1986).

140. *State v. Porter*, 647 N.E.2d 972, 975-76 (Ill. 1995).

141. *Id.* at 974-75.

142. *Id.*

143. *United States ex rel. Porter v. Warden*, 1996 WL 167340 (N.D. Ill. Apr 4, 1996) (quoting *Strickland v. Washington*, 466 U.S. 668, 691, 694 (1984)).

144. *Porter v. Gramley*, 112 F.3d 1308, 1313 (7th Cir. 1997).

145. Tom Ragan, *Years After Death Row Travesty, Killer Gets Due*, Chi. Trib. Sept. 8, 1999, at M1.

146. See Steve Mills, *Simon Also Suspected in Milwaukee Slaying*, Chi. Trib., Mar. 10, 1999, at 1.

147. Steve Mills, *Porter Case Had Wrongs at Each Turn*, Chi. Trib., Feb. 12, 1999, at 1.

148. Shannon O'Boyle, Paula McMahon & Ardy Friedberg, *Death Row Prisoner Dies; Now DNA Test Clears Him*, South Florida Sun-Sentinel (Ft. Lauderdale Fla.), Dec. 15, 2000, at 1A.

149. *Smith v. State*, 515 So.2d 182, 183-84 (Fla. 1987).

150. See, e.g., Dave Maass, *Some Counties Simply Can't Afford To Pursue Capital Offense Cases*, Arizona Capitol Times, May 25, 2001, at A1:

A May 4 decision by the Arizona Supreme Court to send a death row inmate back for re-sentencing has left some county officials wondering whether they can afford to pursue the death penalty in capital offense cases. . . .

The court held in its opinion, written by Chief Justice Thomas Zlaket, that "so long as the law permits capital sentencing, Arizona's justice system must provide the adequate resources to enable indigents to defend themselves in a reasonable way."

The problem is many smaller counties don't have the resources to pay for a pre-sentencing investigation. It's a choice between the death penalty and fiscal death.

See also Russell Gold, *Regional Report: Counties Struggle With High Cost of Prosecuting: Death-Penalty Cases; Result Is Often Higher Taxes, Less Spending on Services; 'Like Lightning Striking'*, Wall St. J., Jan. 9, 2002 ("Just prosecuting a capital crime can cost an average of \$200,000 to \$300,000, according to a conservative estimate by the Texas Office of Court Administration. Add indigent-defense lawyers, an almost-automatic appeal and a trial transcript, and death-penalty cases can easily cost many times that amount.... The cost, county officials say, can be an unexpected and severe budgetary shock—much like a natural disaster, but without any federal relief to ease the strain. To pay up, counties must raise taxes, cut services, or both.").

151. See also Rhonda Cook, *System Prolongs Death Row Wait*, Atlanta Const., Oct. 27, 2001 ("The average time a Georgia death row inmate waits for his or her sentence to be carried out is 11 years, but 64 of the 128 currently awaiting execution have been under death sentences even longer. Waiting the longest—28 years—has been Carl Isaacs, and his execution date remains undetermined.").

152. The discussion in this section is partially reprinted, and expanded, in James S. Liebman, *An "Effective Death Penalty?": AEDPA and Error Detection in Capital Cases*, forthcoming in the *Brooklyn Law Review*, 2002.

153. See, e.g., sources collected in James S. Liebman, *The Overproduction of Death*, 100 Colum. L. Rev. 2030, 2112-14 n.197 (2000).

154. See *id.*

155. See, e.g., *Candidates for Lieutenant Governor Hold Opposing Views on the Death Penalty*, Roanoke Times, Oct. 30, 2001 ("Support for the death penalty has long been considered almost a prerequisite for

becoming a major party candidate for statewide office in Virginia. ‘If you ran for statewide office, it was assumed you had to be for the death penalty,’ said Robert Holsworth, director of the Center for Public Policy at Virginia Commonwealth University. ‘Being opposed to it was viewed almost as a disqualifier for office.’”).

156. See *supra* pp. 5 & n.77, 24 & n.127.

157. For a more extensive discussion of this issue, with numerous examples and citation of a variety of sources, see 100 Colum. L. Rev., *supra* note 153, at 2111-14 & nn.194-99. See also *infra* notes 161, 209 (discussing voters’ removal of judges from California Supreme Court because of their votes to reverse capital verdicts).

158. See *A Broken System, Part I*, *supra* note 101, at 9.

159. See *infra* pp. 333-34.

160. On the tendency of the Illinois appellate system to allow serious error in capital cases to go unremedied on harmless error and waiver grounds, see the investigative reports cited *supra* notes 123, 125. On the tendency of Ohio appellate judges to deem disturbing capital error to be “harmless,” see the articles cited *supra* note 123. On the same tendency in Texas capital cases, see, e.g., Janet E. Elliott, Prisoner Loses Attorney Ruling; Court Says ‘Competent Counsel’ Right Does Not Apply in Appeals, *Houston Chron.*, Jan. 2, 2002 (“Death row inmates don’t have a right to capable counsel in their appeals., the state Court of Criminal Appeals ruled Wednesday” in a 6-3 decision; as a result, appellate lawyer’s incompetent failure to challenge errors on these appeals will bar all subsequent consideration of the error); James Kimberly, Guilty . . . Or Merely Proven Guilty?: Once on Death Row, it Might Not Matter, *Houston Chron.*, Feb. 6, 2001, at 1:

Colella . . . is by no means the only death row defendant to have his claims of innocence ignored by the [Texas] Court of Criminal Appeals. As former Presiding Judge Michael McCormick pointed out, the court is not particularly concerned with claims of innocence.

“Actual innocence claims have to go through the clemency process. That’s what it is there for,” said McCormick . . .

The chairman of the Texas Board of Pardons and Paroles, however, says he leaves innocence questions to the courts.

* * * * *

The Court of Criminal Appeals has never been particularly kind to defendants. . . . But never has the court been as reluctant to reverse a conviction or a death sentence as it is today.

* * * * *

Three times the Court of Criminal Appeals has upheld convictions in cases in which there were allegations the defense attorney slept during portions of the trial. The court also once ruled against a capital defendant who presented evidence that her attorney was a close friend of the key prosecution witness. Another time, the court ruled against a death row inmate whose attorney was accused of drinking and snorting cocaine outside the courtroom during the capital murder trial.

The Texas Defender Service . . . reviewed 103 of the 187 habeas corpus appeals filed with the Court of Criminal Appeals over five years.

The service said almost half the petitions—typically a defendant's final appeal in state court—were less than 30 pages, indicating the court-appointed attorney either did not know how to properly write a habeas writ or made little attempt to investigate the case.

Eighteen of the habeas petitions for death penalty defendants were less than 15 pages long. Two were only three pages.

The Court of Criminal Appeals never raised questions about the competency of the attorney in any of the cases. Instead, the court just ruled against the defendants, Marcus said.

See also Ken Armstrong & Steve Mills, *Gatekeeper Court Keeps Gates Shut: Texas Judges Prove Reluctant to Nullify Cases*, Chi. Trib., June 12, 2000, at N1:

By the time the Court of Criminal Appeals of Texas ruled on Cesar Fierro's request for a new trial in 1996, it was clear his confession had been obtained under disturbing circumstances.

Nearly everyone involved with the case had come to agree that Fierro confessed to the murder of an El Paso taxi driver only after police across the border in Juarez, Mexico, raided his parents' house, held them captive and threatened to torture his stepfather with electrical current from a generator attached to his genitals.

The prosecutor who put Fierro on Death Row now believes that. So does the local district court judge who reviewed how the confession was obtained. He recommended that Fierro get a new trial.

Even the Court of Criminal Appeals, Texas' highest criminal court, agreed that Fierro's rights were violated and police committed perjury.

But the court . . . stopped there. It refused to grant Fierro a new trial.

In Texas, the Court of Criminal Appeals is the state's court of last resort, a gatekeeper that is supposed to remedy injustice, correct fundamental errors that occur at trial, and ensure that convicted defendants receive a fair hearing on appeal.

But a Tribune investigation found that it has not always done that. To handle Death Row appeals, the court has appointed attorneys with previous disciplinary records or little experience. In its rulings, the court has frequently proved tolerant of flawed convictions and reluctant to acknowledge holes in the prosecution's case.

In Fierro's case, the majority on the nine-member court ruled that the violation of his rights was "harmless." The court kept Fierro on track for execution, and he has now been on Death Row for two decades.

* * * * *

The Court of Criminal Appeals also has refused relief to Death Row inmates represented by an attorney who slept at trial. It refused relief to a defendant who, a psychologist testified, was more likely to commit future acts of violence because he is Hispanic—testimony that even the state attorney general’s office found objectionable. The U.S. Supreme Court last week ordered a new sentencing hearing for the defendant, and the Texas attorney general has said six other inmates may receive new sentencing hearings on the same grounds.

The court has even refused relief to a convicted rapist, Roy Criner, even though DNA testing conducted after trial showed the semen found in the victim wasn’t his. . . .

The Tribune’s investigation of the death penalty in Texas found deep-seated problems that call into question the system’s integrity.

161. Rather than picking and choosing among judges based on our subjective judgments about how trustworthy they are when it comes to identifying serious error, we counted all reversals by all judges during the study period. We did this for three reasons. First, to resort to our own conclusions that Judge A’s rulings count, but Judge B’s don’t—notwithstanding that both were elected or appointed to review death verdicts and make the final rulings *A Broken System* counted—would be inconsistent with acceptable social scientific practice and susceptible to bias and inaccuracy. Second, we counted only *final* reversals, meaning ones subject to scrutiny and validation by at least two and often three or four levels of courts, with the ruling at each level besides the lowest one being made by panels of at least three and often as many as seven or nine (or even more) judges. When added to the screening of judges that takes place through elections and appointments, the multi-tiered review process at each stage and the multi-member review panels at all but the first levels of review provide strong assurances that findings of reversible error in capital verdicts are not the result of a single judge’s idiosyncracies but the mutually verified conclusions of multiple judges, the vast majority of whom in the aggregate are, by political and professional disposition, reluctant to reverse capital verdicts. Third, we used this method because it is conservative: It *moderates* reversal rates. Given the political and professional realities already discussed, it is probably the case that for every Rose Bird, the California Supreme Court Chief Justice who some claim was too quick to overturn death verdicts—but then was voted out of office for that reason, see *infra* note 209—there are several judges like Malcolm Lucas, who replaced Rose Bird and, without fear of political discipline, voted to *affirm* virtually every capital verdict he reviewed. If we chose to eliminate decisions in which any particular “outlying” judge concurred (assuming there were objective criteria for identifying such judges), the result would be the elimination of many more affirmances joined by Chief Justice Lucas and similar judges (see *infra* note 209) than reversals joined by Chief Justice Bird and similar jurists—with the effect that reversal rates would be even higher than *A Broken System* finds.

162. See case studies cited *supra* notes 121-23, 125, 160.

163. See *supra* p. 24 & n.128.

164. About 15% of the death row exonerations (releases from death row due to findings of factual or legal innocence) occurred as a result of direct appeal findings that there was not enough evidence to allow a reasonable person to conclude that the defendant was guilty. See *Innocence and the Death Penalty*, *supra* note 77. Exonerations following state post-conviction and federal habeas review almost never occurred for this most glaring of reasons, and instead were generally prompted by the kind of violations that are the most hidden from view: prosecutorial suppression of evidence of innocence and other exculpatory evidence, and an incompetent lawyer’s failure to find similar evidence. Our federal habeas data reveal no reversals because the evidence of guilt was insufficient to convince a reasonable jury, and only two reversals based on a

conclusion that there was insufficient evidence to justify a death sentence.

165. See, e.g., the debates in both Houses of Congress that preceded the adoption of the Antiterrorism and Effective Death Penalty Act of 1996. Statements by many supporters of the Act juxtaposed favorable views of the properly discerning capacities of state direct appeal judges with claims that federal habeas judges tend to have excessively low standards for reversible error. See, e.g., 142 Cong. Rec. S3362 (Apr. 16, 1996) (Sen. Hatch) (“I have to say, one of the biggest problems [is] loony judges in the federal courts who will basically grant a habeas corpus petition for any reason at all.”). Pennsylvania Senator Arlen Specter, for example, extended similar criticisms to, and proposed similar limits on, the state post-conviction stage. For an overview, see Larry W. Yackle, *A Primer on the New Habeas Corpus Statute*, 44 *Buff. L. Rev.* 381 (1996). These views explain the various rules adopted by the U.S. Supreme Court during the late 1980s and early 1990s and by Congress in 1996 that curbed federal habeas review, all of which proceeded on the assumption that direct appeal courts catch the most serious errors at the first stage of review, and that life-tenured federal habeas judges at the last stage of review are more likely than elected state judges to reverse based on non-serious error. Similar rules adopted in Texas and elsewhere imposed similar constraints on state post-conviction review based on like comparisons of the appropriately tough-minded decisions of state direct appeal judges and laxer review thought to occur at the state post-conviction stage. See *supra* p. 27 & n.132; *infra* pp. 63-64 & nn.202-06 (discussing recently expanded constraints on state post-conviction and federal habeas review).

166. See *infra* pp. 40-41 (further discussion of the seriousness of error found at the direct appeal stage).

167. Of the 351 state post-conviction reversals, the reason for reversal is known in 299 cases. Of those 299:

- 116 (39%) were reversed because of egregiously incompetent defense lawyers;
- 57 (19%) were reversed because of police or prosecutorial suppression of evidence of innocence or that a death sentence was not appropriate (47 cases) or because of other prosecutorial misconduct (10 cases);
- 58 (19%) were reversed due to misleading instructions to jurors on how they were required to decide the case; and
- 13 (4%) were due to judge or jury bias.
- 239 (80%) were reversed due to one of these four serious flaws.

See Appendix C for all the relevant information and summary tables. The first four figures above do not sum to the last figure (239) because five verdicts were reversed due to *both* incompetent lawyering and prosecutorial suppression of evidence. For the reasons discussed *supra* notes 87, 90, these figures vary slightly from those reported in *A Broken System, Part I*.

168. See the Wallace case discussed in Appendix D, p. D-6.

169. Reasons for federal habeas reversals are known for 220 cases, as set out below. Because there often were multiple bases for reversal, we counted each basis as its proportion of the total number of bases for relief. Where there was one basis for reversal, that claim is counted as 1; where there were two bases for reversal, each basis is counted as .5; each is counted as .33 (one-third) where there are three bases for relief; and so forth.

Reasons for Reversals on Federal Habeas

Reason for Relief	No. of Verdicts Overtured for this Reason	% of Reversals Where Reason Is Known
Incompetent defense lawyer	50.17	23%
Lawyer denied altogether	.25	.1%
<i>(All denials of competent lawyer)</i>	<i>(50.42)</i>	<i>(23%)</i>
Prosecutorial suppression of evidence	12.33	6%
Prosecutors' intentional exclusion of black jurors	3.33	1.5%
Other police, prosecutor misconduct	11.28	5%
<i>(All police, prosecutor misconduct)</i>	<i>(26.94)</i>	<i>(12%)</i>
Invalid instructions on burden of proof	38.00	17%
Invalid instructions on aggravating circumstances	13.80	6%
Invalid instructions on mitigating circumstances	18.03	8%
Other invalid capital sentencing instructions	2.20	1%
<i>(All invalid instructions)</i>	<i>(72.03)</i>	<i>(33%)</i>
Juror bias caused by third parties during trial	1.50	.7%
Juror bias due to pretrial influences	5.00	2%
Illegal exclusion of blacks from jury pool	8.50	4%
<i>(All biased decision makers)</i>	<i>(15.00)</i>	<i>(7%)</i>
Exclusion of exculpatory evidence	2.00	.9%
Defendants denied funds for adequate defense	2.08	.9%
Defendant was mentally incompetent to be tried	5.33	2%
Involuntary confession	5.00	2%
TOTAL	178.80	81%

Source: HCDB.

170. A small number of reversals (2% of federal habeas reversals; 0 state post-conviction reversals) occurred because capital defendants asked for, but were not allowed to see, lawyers during in-custody interrogations. As the United States Supreme recently held, such reversals are not technicalities. Rather, as the Court noted, the purpose of requiring defendants to be informed of their right to an attorney and to be provided with a lawyer (if they ask for one) before additional interrogation takes place is to assure the reliability of statements made to police. See *Dickerson v. United States*, 530 U.S. 428 (2000). For an example of the miscarriages that can result when suspects make unreliable statements without understanding their rights and without having a lawyer present, see the case study of Earl Washington's case, *supra* pp. 27-30. See also the recent *Chicago Tribune* study of false confessions, *supra* note 35.

171. The reasons for reversal counted here include incompetent defense lawyering, prosecutorial suppression of exculpatory evidence, rulings excluding such evidence and instructions to ignore such evidence. Because our state post-conviction data do not distinguish among improper instructions on these and other bases, we conservatively assumed that only 40% of the misinstructions were on this ground. We also conservatively omitted reversals for other forms of prosecutorial misconduct and improper instructions on burden of proof, even though some of these violations involve prosecutorial mishandling of exculpatory evidence, prosecutorial statements calling on jurors to ignore such evidence and judicial instructions keeping jurors from giving weight to such evidence. Even when counted in this conservative manner, error fitting in this category accounts for 60% of all state post-conviction reversals and 50% of all federal habeas reversals.

172. See *infra* pp. 319-24.

173. See *supra* pp. 17-18; See *A Broken System, Part I*, *supra* note 101, at 19-20, 26-27 & nn.39, 132; App. C, pp. C-1 to C-2.

174. For full information and summary tables, see *infra* Appendix C. New information obtained since the publication of *A Broken System* decreased the proportion of retrial verdicts imposing sentences less than death from 75% to 73%, but increased the proportion of acquittals from 7% to 9%, leaving the overall proportion of outcomes less than death the same, 82%. See *supra* notes 87, 90.

175. See Fed. R. Evid. 612 (applicable in about 40 states, as are comparable rules elsewhere).

176. See, e.g., Fed. R. Evid. 804(b)(1), 803(5) (applicable in about 40 states, as are comparable rules elsewhere); see also Fed. R. Evid. 801(d)(1). In point of fact, the need to use prior testimony instead of current testimony is infrequent because most witnesses in capital cases—law enforcement officers, co-offenders, and surviving victims (usually of robberies of commercial establishments)—were working age or younger when the first trial occurred.

177. See *supra* pp. 40-41.

178. See *supra* pp. 22-23, 26, 30-32, 41.

179. See *infra* pp. 319-24.

180. See sources and examples cited in 100 Colum. L. Rev., *supra* note 153, at 2078-84 & nn.142, 145; *id.* at 2144-45 & n.271.

181. Insufficient evidence almost always is challenged on direct appeal, and state legal rules almost always bar claims raised on direct appeal from being raised again in state post-conviction proceedings. See also *supra* note 164.

182. See *supra* pp. 40-41.

183. For additional discussion of this study for the *Nashville Tennessean*, see *supra* pp. 17-18 & n.102.

184. See Shiffman, *supra* note 102.

185. For examples of how overall reversal rates are calculated, see *supra* pp. 8 n.*, 19; *supra* note 103.

186. The six states that had death verdicts finally reviewed at only the direct appeal stage are Colorado, Connecticut, New Jersey, New Mexico, Ohio and Oregon.

187. The two states for which we only have data on outcomes at the direct appeal and federal habeas stages review are Delaware and Washington.

188. See Figure 1A, p. 50 below, for a list of these 26 states.

189. Conversely, the more random the pattern of bars is, the less evidence the chart provides of a relationship between reversal rates and the factor being measured. For discussion in addition to that in the text just below about why our subsequent multiple regression analyses are a more comprehensive and reliable method of testing relationships between two variables—such as reversal rates and homicide rates—than are the bivariate comparisons permitted by graphs such as those discussed here in text, see *infra* pp. 82-83.

190. See *infra* pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 258-59, 372-75.

191. For a discussion of how combined, or overall, reversal rates are calculated, see *supra* pp. 8 n.*, 19; *supra* note 103.

192. Judges reviewing death verdicts in later years are different from judges reviewing verdicts in earlier years, and the same judges face political and other pressures in some years (say, election years) that are different from pressures they face in other years. Analyzing differences in reversal rates based on the year of the final decision directly assesses the effect of having different decision makers in different years and of different pressures on the same decision makers.

This same measure of change may also be indirectly affected by changes in the quality of capital verdicts over time: Death verdicts reviewed later tend to be ones that were imposed later, so analyses of differences in reversal rates for verdicts reviewed in later, versus earlier, years may detect differences in the quality of verdicts imposed in later, versus earlier, years. As a result, analyses of changes in reversal rates across all study years in which death verdicts were reviewed will be partly sensitive to changes over time in how reliably trial actors reached a verdict. Nonetheless, because the effect of sentence-year conditions on review-year outcomes is indirect, sentence-year conditions probably have less of an effect on differences in reversal rates measured by review year than do the conditions described in the first paragraph of this footnote.

193. Trials held in later years may be funded differently from, or staffed by better trained or less well-trained professionals than, trials held earlier. Analyzing differences in reversal rates based on the year death verdicts were imposed directly assesses the effect of these trial-specific conditions on reversal rates. This same

measure of change may also be indirectly (and, so, we think, less substantially) affected by changes in the attitudes and behaviors of *reviewing* judges: Verdicts imposed in a given year can only be reviewed by judges on the bench in that or (more probably) a later year, not by the different set of judges who were on the bench earlier. Consequently, analyses of changes in reversal rates based on the year death verdicts were imposed will be indirectly sensitive to changes in how carefully judges scrutinized capital verdicts for error in later, as opposed to earlier, periods.

194. Of the 2349 reversals occurring during the study period, 1852 (79%) occurred at the direct appeal stage, and 240 (10%) occurred at the federal habeas stage. The remaining 257 (11%) occurred at the state post-conviction review stage. For discussion of our regression findings on changes in direct appeal reversal rates over time after controlling for other factors, see *infra* pp. 194-95, 258-59.

195. See *supra* pp. 11-18.

196. See *supra* pp. 17-18.

197. See decisions cited *supra* notes 105-09 and accompanying text.

198. See *supra* p. 43 & n.173.

199. Information on California, Florida, Illinois, Nevada, and Washington are collected in *A Broken System, Part I*, *supra* note 101, at 146-47 n.241.

On Alabama, see Letter from James S. Liebman to Senator Patrick Leahy, Chair, Senate Judiciary Committee, July 2, 2001 (analyzing capital reversal data supplied to the Judiciary Committee by the Alabama Attorney General, which reveals a 74% reversal rate from 1977 to 2000, compared to the 77% reversal rate we discovered for the 1977-1995 period).

On Kansas, see *infra* p. 400 & n.894.

On Kentucky, see John Cheves, *Judges Are Overturning Death Sentences, Citing Mistakes at Trial*, *Lexington Herald-Leader*, Oct. 31, 2001 (“[S]uddenly . . . judges are overturning death sentences, citing mistakes at trial, twice as often as juries recommend them. Six men were taken off Kentucky’s Death Row in the past year, while just three more men arrived. . . . ‘I don’t ever remember a series of reversals coming down like this before,’ said Joe Bouvier, a Fayette County prosecutor who studies capital punishment issues for the Kentucky Commonwealth’s Attorneys Association.”).

Information about Louisiana and North Carolina comes from conversations with capital punishment lawyers in those states. But cf. Ames Alexander & Liz Chandler, *Errors, Inequities Often Cloud Capital Cases in the Carolinas*, *Charlotte Observer*, Sept. 10, 2000, at 1A (from 1977 to 2000, “more than half of all death sentences imposed [whether or not they were finally reviewed] have been thrown out because of flawed trials”).

On Ohio, see sources cited *supra* note 123 (noting reluctance of Ohio Supreme Court to reverse capital verdicts despite errors identified by that court).

On Pennsylvania, see Debbie Garlicki, *Death Row Defenders Rroil Legal Waters*, *Allentown Morning Call*, Jan. 13, 2002 (“Over the last six years, judges have . . . ordered new trials in at least 14 [Pennsylvania capital] cases and new sentencing hearings in at least 23 cases involving the defenders.”); Debbie Garlicki, *Yet Another Death Penalty Is Set Aside*, *[Allentown] Morning Call*, Aug. 28, 2001.

On Tennessee, see Shiffman, *supra* note 102 (study showing state direct appeal reversal rate of about 50% for the 1980-2001 period, compared to the 29% reversal rate we discovered for our study period).

On Virginia, see, e.g., Alan Cooper, *Death Sentence Ruled Subject to New Hearing*, *Richmond Times-Dispatch*, Sept. 28, 2001; Frank Green, *Death Penalty Cases Scrutinized More: Hearings Are Being Ordered in Virginia*, *Richmond Times-Dispatch*, Apr. 9, 2001 (“Bumps and potholes are slowing the pace

of capital cases on Virginia's once smooth road from death sentence to execution."); Brook A. Masters, *Legal Scrutiny Slows Pace of Executions in Virginia*, Wash. Post, Dec. 26, 2000 ("The number of executions in Virginia fell by nearly half this year, as state and federal appeals courts intervened more often in the commonwealth's fastest-in-the-nation system of capital punishment."); *Death Sentence Reversed in Va.*, Wash. Post, June 8, 2001 ("Kevin Green . . . is the second death row inmate in seven weeks—and the sixth since 1999—to win a reversal from the Virginia Supreme Court"; case is thought by some "legal analysts" to "reflect the Virginia Supreme Court's growing concern" with the reliability of Virginia capital verdicts); *With Plea, Virginia Convict Moves Off Death Row*, Wash. Post, Apr. 29, 2000 ("For the first time in a decade, an inmate learned yesterday that he would walk away from Virginia's death row alive without the governor's help.").

On Washington, see also Lise Olsen, *The Death Penalty: Uncertain Justice*, Seattle Post-Intelligencer, Aug. 6, 2001 ("30 men have been sent to death row since Washington's death-penalty law was reinstated in 1981—but 15 have had their death sentences overturned. Only three have been executed, and 2 of them 'volunteered.' The leading causes of the reversals are ineffective assistance of counsel and judicial error—both of which figured in 6 rulings. . . . Statewide, 22% of the condemned have gotten off death row due to ineffective assistance of counsel."); Olsen, *supra* note 64 (discussing reversal of James Leroy Brett's death sentence because of incompetent lawyering, noting that "[i]t was one of three death sentences overturned so far this year by the state Supreme Court—the most in a single year since capital punishment was reinstated in 1981.").

200. See *supra* pp. 22-47.

201. See *supra* pp. 1-5.

202. See, e.g., Alan Berlow, *The Wrong Man*, *The Atlantic Monthly*, Nov. 1999, at 84 ("In 1995 Congress weighed in on the need for speedier executions when it eliminated the \$20 million annual budget for Post-Conviction Defender Organizations which had provided some of the most sophisticated and effective counsel for death-row inmates in twenty death-penalty states); Roscoe C. Howard, Jr., *The Defunding of the Post-Conviction Defense Organizations as a Denial of the Right to Counsel*, 98 W. Va. L. Rev. 863 (1996). See also, e.g., Editorial, *Death Penalty Blunder*, Wash. Post, July 12, 2001 ("The Virginia Supreme Court has adopted guidelines that effectively slash the fees of court-appointed attorneys in death penalty cases. The move comes at a time when national attention has been focused on the low quality of counsel in capital cases."); *Death Row Takes a Toll on Lawyers; Money, Resources Are in Short Supply*, *New Orleans Times-Picayune*, Aug. 20, 2001 ("After Congress pulled federal funding for post-conviction appeals in 1996, the number of capital cases in Louisiana kept climbing and defense lawyers working pro bono for death row inmates already were scarce. With urging by the Louisiana Supreme Court, which has put specific death penalty appeals on hold until the defendants had lawyers, the state responded in 1999 and enacted a law that gives those on death row the right to representation. But legislators didn't provide the \$1.3 million they themselves said was needed for the program.").

203. At the national level, see the Anti-Terrorism and Effective Death Penalty Act of 1996 ("AEDPA"), Pub. L. 104-132, 110 Stat. 1214 (1996), which made a number of changes in preexisting federal habeas review, all designed to cut back on the amount of review and relief. See Randy Hertz & James S. Liebman, *Federal Habeas Corpus Practice and Procedure* §§ 3.2-3.5 (4d ed. 2001); *supra* p. 27 & n.132. U.S. Department of Justice data show that since AEDPA was adopted, federal habeas courts at the appellate level have used "procedural terminations" to decline to decide the legality of underlying convictions and sentences about 15-20% more often than had been the case before the Act was adopted—with no appreciable improvement in the amount of court time spent on the cases. See <http://www.ojp.usdoj.gov/bjs/abstract/ppfc96.htm> (data do not distinguish capital and non-capital cases; most cases are non-capital cases).

At the state level, see, e.g., Florida Court Issues New Rules to Speed Up Appeals Process for Death Row Inmates, Associated Press Newswires, July 12, 2001; Duggan, *supra* note 19 (“The most significant death penalty change enacted during [George W.] Bush’s six years as governor [of Texas] was a [1995] law that toughened and accelerated the appeals process for condemned inmates. During Bush’s tenure, the state carried out 152 executions, an average of one every other week.”). For more on these and other state-level changes, see 100 Colum. L. Rev., *supra* note 153, at 2136-38 & nn.252-53.

204. For evidence that it has become more difficult to provide lawyers for defendants challenging death verdicts in state post-conviction and federal habeas proceedings, see Elizabeth Amon, The State of Pro Bono 2001, Nat’l L.J., Jan. 7, 2002 (“Lawyers in death penalty work say they’ve seen a drop in volunteerism. Capital cases are expensive and demanding, and those involved in placing attorneys on them say large firms in particular have shied away from them. ‘It’s a real crisis,’ says Robin Maher, director of the American Bar Association pro bono death penalty representation project. ‘In Alabama alone, there are 30 people on death row who don’t have lawyers.’”).

205. Consider, e.g., Lloyd Schlup’s exoneration, which occurred as a result of a type of federal habeas procedure that Congress withdrew in 1996. If a similar case arose today, there would be no judicial mechanism available to the innocent prisoner to avoid execution. See *supra* p. 27 & n.132.

206. The discussion in this section is reprinted and expanded in “Effective Death Penalty?,” *supra* note 152.

207. See *A Broken System, Part I*, *supra* note 101, at 61, Figure 8; 102-07 & Figure 33.

208. The combined state direct appeal and state post-conviction reversal rate and the federal habeas reversal rate have a .341 correlation (significance = .044, 1-tail; .088, 2-tail).

209. See Maura Dolan, Execution Issue Clouds Davis’ Judge Selection, L.A. Times, Nov. 13, 1999, at A1 (reporting complaints that California Governor Gray Davis, “haunted by the memory of former Chief justice Rose Bird . . . whom voters ousted because she never voted to uphold a death sentence,” is demanding, in the opinion of Peter Keane, Dean of Golden Gate University Law School, that his appointees to the state’s trial and appellate courts profess support for the death penalty as “the greatest thing since sliced bread”). See also Stephen B. Bright & Patrick J. Keenan, Judges and the Politics of Death: Deciding Between the Bill of Rights and the Next Election in Capital Cases, 75 B.U. L. Rev. 759, 760-65 (1995) (describing California voters’ removal of three state supreme court justices due to their votes to reverse capital verdicts).

210. See Paul Elias & Rinat Fried, A Failure to Execute, The Recorder, Dec. 15, 1999, at 1 (quoting federal judges blaming lax review by California state court judges for the time federal courts must devote to federal review of California death verdicts and for high federal court reversal rates; also quoting the chief justice of the California Supreme Court acknowledging that lax state court review in the past, during the tenure of Chief Justice Malcolm Lucas, may have placed a burden on federal courts).

211. See Green, Study Critical of Death Penalty, *supra* note 30 (discussing December 2001 study by Virginia’s Joint Legislative Audit and Review Commission concluding that “the strict adherence to rules by federal and state appeals courts may have let stand the conviction of some death row inmates who did not receive fair trials” and “suggest[ing that] the Virginia Supreme Court was overly strict in its use of state law in determining whether a death sentence was excessive or disproportionate”).

212. See *A Broken System, Part I*, *supra* note 101, at 106-07.

213. See *infra* pp. 389-90.

214. For press treatments of this issue, see, e.g., Linda Greenhouse, *Death Penalty Gets Attention of High Court*, N.Y. Times, Oct. 30, 1999, at A1 (noting the Supreme Court's "unusual flurry" of grants of review in Virginia/Fourth Circuit capital cases); Brooke A. Masters, *4th Circuit Is Steering Hard to the Right*, Wash. Post, July 5, 2000, at B1 (finding in recent Supreme Court decisions a message "that the 4th Circuit had gone too far in death-penalty cases"); Tim McGlone, *State's Death-Row Cases Draw High-Court Scrutiny*, The Virginian-Pilot, Dec. 9, 1999, at A1 (describing recent Supreme Court decisions as "applying the highest level of scrutiny in modern times to [Virginia's] handling of death penalty cases"). For citation of the relevant cases, see Hertz & Liebman, *supra* note 203, § 3.2; 100 Colum. L. Rev., *supra* note 153, at 2060 n.105. The progression of Virginia cases in which the Court has granted certiorari continued recently. See *Mickens v. Taylor*, 121 S. Ct. 1651 (Apr. 21, 2001) (mem.) (granting review of decision adverse to Virginia capital prisoner in *Mickens v. Taylor*, 240 F.3d 348 (4th Cir. 2001)); *supra* note 36 (discussing Court's October 2001 decision to review the constitutionality of the death sentence Virginia has imposed on retarded prisoner Daryl Atkins).

215. See *A Broken System, Part I*, *supra* note 101, at App. C-49 (discussing (1) Virginia Governor's execution-eve grant of clemency to Earl Washington, due to doubts about his guilt, which placed him under a life sentence without parole until a subsequent Governor fully exonerated him based upon proof of his innocence, see *supra* pp. 27-30; and (2) conditional pardon of Joseph Giarratano, permitting him to secure a new trial, after the state and federal courts had declined to provide that same relief, notwithstanding the acknowledged illegality of Giarratano's initial conviction, because of their strict application of the technical waiver rules barring claims a prior lawyer had failed to present in the proper way at the proper time). See generally, Brooke A. Masters, *New Evidence Overturns Murder Conviction in Va.*, Wash. Post, Nov. 14, 2001 ("Since 1989, Virginia has freed five inmates after DNA testing cast doubt on their guilt, but all five were released through gubernatorial clemency rather than the courts.").

216. See *infra* pp. 218-19, 236.

217. See *infra* pp. 323-24, 333-34.

218. See *supra* p. 43.

219. See *supra* pp. 22-35.

220. See *supra* pp. 8-9, 11.

221. See *supra* p. 11.

222. See *supra* p. 69.

223. See *supra* pp. 24 & n.128, 25-35.

224. See *supra* p. 36 & n.150.

225. See *supra* pp. 24 & n.128, 25-35.

225A. Sources and Periods Covered in Figures 8A and 8B:

homicides, 1973-1995:*	Uniform Crime Reports
homicide arrests, 1973-1995:†	The Uniform Crime Reporting Program Data: County Level Offense Arrest Data, available from the Inter-University Consortium for Political and Social Research (ICPSR)*
murder convictions:‡	Source Book of Criminal Justice Statistics, 1986-1996
number of death sentences, 1973-1995:	DRCen
outcomes of court review, 1973-1995:^	DADB (direct appeal); Appendix C, infra (state post-conviction); HCDB (federal habeas)
dispositions, 1973-2002:^	
– released, not guilty: after direct appeal, federal habeas, 1973-Jan. 2002:	Death Penalty Information Center, Innocence and the Death Penalty, http://www.deathpenaltyinfo.org/innoc.-html
after state post-conviction, 1973- April 2000:	Appendix C, infra
– clemency, 1973-Jan. 2002:	Death Penalty Information Center, http://www.deathpenaltyinfo.org/clemency.html
– reversed on successive petition:	HCDB
– executions, 1973-Sept. 2001:	NAACP Legal Defense and Educational Fund, Inc., Death Row U.S.A. Fall 2001

* Homicide and arrest data are for each of the 34 study states during the portion of the 1973-1995 study period when the state had a valid post-*Furman* death-sentencing statute. The Uniform Crime Reporting Program defines homicide as “the willful (non-negligent) killing of one human being by another. The classification of this offense is based solely on police investigation as opposed to the determination of a court, medical examiner, coroner, jury, or other judicial body. Not included in the count for this offense classification are deaths caused by negligence, suicide, or accident; justifiable homicide; and attempts to murder or assaults with intent to murder, which are scored as aggravated assaults.”

† Homicide arrest data are only available from 1977 to 1995. Arrests from 1973 to 1976 are calculated by determining the average number of arrests per homicide for each state from 1977 to 1995, then multiplying that number times the number of homicide for each study state during the relevant years from 1973 to 1976. Outliers were removed and replaced with an estimate based on the number of homicides. Missing values for particular states and years are estimated in the same manner.

‡ Information on murder convictions following homicide arrests from 1973 to 1985 is not available. The Source Book of Criminal Justice Statistics provides information on the number of murder convictions for all states combined for each even-numbered year between 1986 and the present. For even numbered years from 1986 to 1994, we began with the 50-state murder conviction statistic reported in the Source Book. For odd numbered years between 1987 and 1995, we estimated murder convictions as the average of the prior and following years. We then used that actual or estimated number of murder convictions for all 50 states to derive a corresponding number for each of the 34 study states. To do this we (1) derived a national rate of murder convictions per homicide arrest for each year from 1986 to 1995 by dividing the number of murder convictions in each year by the number of homicide arrests for that year, then (2) multiplied the homicide arrests in each state and year by the national rate of convictions per arrest. To estimate murder convictions for the 1973-1985 period, we used the same procedure—multiplying the national rate of murder convictions per homicide arrest for the post-1985 period that we derived from the Source Book times the number of homicide arrests in each of the study states in each of the relevant years between 1973 and 1985. *The estimates achieved in this manner are extremely rough* because they assume that there is little variation among states in the rate of murder convictions per homicide, and that murder convictions prior to 1986 have the same relationship to homicide arrests as was true in the period after 1985. We base no conclusions on the number of murder convictions reported in text and present the number only to provide as much context as possible, given the limited data that are available, for the death-sentencing events in the table about which more is known. The authors of the Source Book define “murder” for this purpose as homicides (using the definition in note * above) minus manslaughter offenses

^ Some dispositions occurred in the absence of final review by the courts. Those dispositions are indicated on Figure 8A but not on Figure 8B, which only covers dispositions following final review by the courts. Dispositions in advance of final court review include the execution of individuals who chose to give up their appeals and be executed consensually, as well as clemencies and exonerations that occurred prior to the completion of final court review. All dispositions are for death sentences and, where relevant, for court review occurring between 1973 and 1995.

226. $R = -.71$; significance = .001. Figure 9 uses non-consensual executions for the reasons set out infra note 292, and to be consistent with Figures 6, 20, and 21. If, instead, all executions are considered, the “X” pattern of the two lines in the graph is nearly the same, and the negative correlation between those lines remains high and significant: $R = -.70$; significance = .001.

227. For examples, see supra notes 134, 146, 148 and accompanying text; 100 Colum. L. Rev., supra note 153, at 2130 n.242. See also Alexander & Chandler, supra note 199 (“Capital punishment in the Carolinas is so tainted with mistakes, inequities and incompetence that the system risks executing innocent people, while sparing some of the states’ most vicious killers, a [Charlotte] Observer investigation has found.”).

228. For a thoughtful description of the devastating effect on victims of reversible error in capital cases, see William H. Brill, *Finality? Not for Us, and It’s 17 Years Later*, Wash. Post, Apr. 29, 2001 (Outlook), at B3:

When James Strong was sentenced to death in Pennsylvania for the murder of my cousin Jane’s husband nearly two decades ago, there was a bitter sense of relief for our family. It wasn’t that it was over—nothing like that can really be over—but at least Jane and their 9-year-old son, Jay, could begin to reorganize their lives. Jane moved out of the motel near the court, where she had been staying for the trial. She went back to her job, back to her house; and Jay came home from the neighbors’ house where he had been staying while Jane was at the trial.

Justice had been served. The man who killed John Strock, would die for his crime. Or so we believed.

But it didn’t work that way. The death sentence—with its protracted process of appeals—locked Jane into a 17-year relationship with her husband’s murderer and even with his family. And it’s not over yet.

As someone who has made a career of studying violence, I’m as familiar as anyone with the arguments for and against the death penalty: that it is proper vengeance, and that some people deserve to die; or that it is inhumane and often applied unfairly. But I had never given much thought to its effect on members of the victim’s family, to what it really means for them. I don’t think our family was unusual in believing, in the simplest terms, that the death penalty would make us feel better, that it would provide, as they say, “closure.”

Our family approved of the verdict, particularly Jane. She was anguished and she wanted to see her husband’s murderer die. She appreciated the support she got: Members of the local police force where she lived outside Philadelphia even offered to go with her to witness the execution.

But Strong, the convicted man, didn’t die. He stayed on death row.

Oh, we expected that to happen—at least for a time. But the years went on—two, five years, then 10, 15. While Jane waited for some resolution, Jay grew up. She tried not to dwell on Strong or his execution. There were football games to watch, a college to choose, checks to write.

For the extended family, thoughts about Strong gradually receded. It has always been awkward to talk about him, anyway. . . . So we all kind of put it aside. Which meant kind of putting

the memory of Jane's husband aside.

I was no different. Even though I had worked as a professor, a security consultant and an expert witness in criminal behavior, and had traveled the country analyzing serious criminal cases, I found it just as difficult to inquire. To do so would bring it all up again. How John had picked up two hitchhikers on his way home from visiting his parents in West Virginia in 1983; how they made him drive off the highway onto a deserted road near Wilkes Barre, Pa.; how they marched him off into the woods, robbed him and shot him with a sawed-off shotgun.

* * * * *

From time to time after the conviction and sentencing, Jane would speak about the delays. Always with confusion. "I don't know what they're doing," she would say. "It's a legal thing, something about him going into a different pool, getting placed on a different schedule." . . .

I never pressed her. I could tell she hated to call the district attorney's office. But I also knew that she was wondering whether the man who killed her husband was ever going to be executed...

Then, four years ago Jane got a call. It wasn't the call she had expected. "Just a routine hearing," she reported being told. "You don't even need to come up." But Jane felt she had to go. "Someone ought to be there for John," she told me. And so she took time off from her job in marketing and drove back to Wilkes Barre. Same trip, but a different courtroom.

Jane called me when she got back. "It was horrible," she said. "He was there, only this time he looked clean-cut. And he was in a suit, not all scraggly with long, wild hair like he was before." His family was also there, still saying they were sorry about what had happened, still insisting that their "Jimmy" didn't do it.

"But what about the hearing?" I asked. "I could hardly follow it," she said. "It seemed like they went over the same old thing. But they told me when it was over: No problem. That it was all routine."

I heard nothing more until last December, when a large manila envelope arrived in my mail. It looked like a calendar, probably a Christmas present, from Jane. I opened it and discovered it was an opinion from the Pennsylvania Supreme Court. Strong's conviction had been overturned. "This matter is remanded for a new trial," the document read.

The grounds for that decision were that letters had just come to light suggesting that the DA's office had made a deal at the time of Strong's trial with the attorney for his partner in the crime, James Alexander, and had failed to disclose this to the court or to the jury—or even to the assistant DAs who prosecuted the case. Alexander had been hitchhiking with Strong when John picked them up, and it was he who had testified that Strong was the shooter. During Strong's trial, Alexander denied having made any deals. Later, in his own trial, he pleaded guilty to a lesser charge of third-degree murder. He was sentenced to 40 months in prison.

And so it continues. Within the next few months, the whole process will begin again—with a new trial set for September and pretrial motions due in June. Only this time it's quite possible that Strong will leave the courtroom a free man. Alexander, whose testimony probably wouldn't have

been believable anyway once the jury heard there had been a deal, has since died of a heart attack. . . . Who knows what the jury will decide? That it was Alexander and not Strong who shot John?

Of course, there could have been a reversal in any case. Nobody wants a decision to be made on incomplete evidence, or a prosecutor to fail to disclose relevant facts. Nobody wants an innocent man to be put in prison for life, let alone put to death. What's more, I can't help wondering what the possibility of getting a death penalty verdict could mean to a prosecutor. Is it a trophy? In Strong's case, could it have encouraged the prosecutor to overreach, even to the point of cutting a bad deal with a codefendant? Those are all very important questions. But they're not my main point.

My point is that the death sentence had lulled Jane and the rest of us into believing that she could put one part of her life behind her, that Strong's death would indeed bring her closure.

I won't ever know for sure how Jane would have felt if Strong had been executed. Given the questions that have prompted the new trial, that could not have been a good thing for the cause of justice. The delays that have characterized this case are typical, though. And because of them, Jane has watched and waited year after unsettling year, only to discover now that the man whom she believed all these years murdered her husband may go free. Not long ago, she wondered out loud to me what kind of man he is, whether he's been rehabilitated. "You don't suppose he will come after me, do you?" she asked.

So Jane's uneasy relationship with the man convicted of murdering her husband continues. Whatever some people believe about the value of the death penalty, it hasn't solved anything for our family, and has probably made it all the worse.

229. See *supra* p. 75, Figure 7A.

230. See Figure 10, p. 93 below.

231. The relevant studies of the cost of the death penalty and of curing its mistakes are collected in 100 *Colum. L. Rev.*, *supra* note 153, at 2129-33 & nn.243-46.

232. See, e.g., *State Law Seeks to Provide Strong Defense; Most Capital Cases Are Handled Well, But Examples of Inadequate Representation Show Lapses in the System*, *South Bend Trib.*, Oct. 22, 2001 ("Prosecutors tend to agree the death penalty's frequency of being carried out—and any deterrent factor that may result—is lessened when a sentence is overturned.").

233. See, e.g., Paul Craig Roberts, *supra* note 49 (quoted *supra* note 49). Much of the commentary in the wake of the discovery that even Timothy McVeigh's state-of-the-art federal capital prosecution was marred by serious error on the part of the FBI in failing to turn over thousands of pages of documents to which the defense was entitled has focused on the damage done to the integrity of the process. An editorial in *USA Today* is illustrative:

Today, when most Americans expected to be contemplating the death of Timothy McVeigh, they will instead be forced to contemplate his continued existence. It will also be useful if they cast a similarly jaundiced eye on the process that produced today's colossal anti-climax.

McVeigh's execution was delayed because the FBI failed to provide more than 3,000 documents to his defense attorneys before trial. . . .

This is troubling twice over. First, it is possible, although not probable, that McVeigh's lawyers will use the mistake to win a new trial. This would drag out the prosecution of a confessed mass murderer by several more years and millions more dollars. Second, and of deeper significance, the error illustrates that the capital system is far more prone to error than its defenders admit. If the federal government can't prosecute a slam-dunk case without making potentially prejudicial mistakes, imagine what's happening in the states, where capital crimes are tried by less-skilled lawyers with fewer resources.

What's happening is that errors occur at a rate few people realize. Between 270 and 300 people are condemned to die every year in state courts, but many aren't high-quality convictions. From 1973 to 1995, almost 70% were overturned by appeals courts due to serious flaws . . .

The leading cause for reversal in these cases was incompetent counsel. No such risk faced McVeigh, for whom taxpayers supplied a raft of legal talent.

But the second-leading cause was the failure by the prosecuting team to disclose evidence to the defense team. That's exactly what happened in the McVeigh case. And it happened even though the case features top-ranked investigators, prosecutors and defense attorneys at a cost, by some estimates, that was upward of \$80 million.

Concern about errors is causing many states to re-examine the death penalty. Executions are on pace to fall for the third year in a row. Traditionally active states such as California are seeing fewer capital convictions. And after a close legislative debate, Texas, the nation's leading executioner, is considering a referendum on whether to enact a moratorium. Wyoming recently adopted a sentence of life without parole as an option to death.

. . . If McVeigh can't be cleanly convicted and condemned with all of the resources of the federal government, it's certain that the states are also making errors and that not all of them are being discovered. . .

Editorial, *McVeigh Errors Raise Doubts About Other Capital Cases; Our View: FBI Blunders Spotlight How Often Death-Penalty Cases Are Flawed*, USA Today, May 16, 2001. See also Jordana Horn Marinoff, *If FBI Has Erred on McVeigh, What About Other Cases*, Milwaukee J. Sentinel, May 20, 2001. For another example, see Editorial, *Imperfect Justice*, Wash. Post, May 28, 2001; Page A22:

For the latest exhibit of the fallibility of the judicial system, we turn to Joyce Gilchrist, a chemist with the Oklahoma City Police Department who has worked on more than 3,000 cases. Oklahoma has executed 11 persons based at least in part on her work. Twelve more remain on death row. Yet in recent weeks the FBI labs have been sharply critical of her performance in a sample of cases, accusing her of offering testimony "beyond the acceptable limits of forensic science" in several. Jeffrey Pierce recently was released from prison for a rape he didn't commit; Ms. Gilchrist's testimony at his trial had authoritatively linked his hair to samples found at the scene, a claim DNA testing later belied. A comprehensive review of Ms. Gilchrist's work is now underway.

The alleged problems with Ms. Gilchrist's work are not new. According to the Daily Oklahoman, a professional association criticized her as far back as 1987. State and federal courts have overturned convictions on grounds that her testimony went beyond what was knowable scientifically. Last year she was expelled from another professional group. Ms. Gilchrist says she

will be vindicated by the investigation. But questions about her work serve as a reminder of the grave harm that a single person in the criminal justice apparatus can cause—either through malice or incompetence—if the rest of the system offers little more than malign neglect.

* * * * *

It also argues for rigorous post-conviction review. . . . Oklahoma authorities say they are confident that nobody has been wrongly executed as a result of Ms. Gilchrist's testimony. We hope they are lucky enough to be right.

234. See supra p. 52.

235. See supra pp. 40-43 & nn.167, 169.

236. See 100 Colum. L. Rev., supra note 153, at 2101-36, for a detailed demonstration and documentation of this point.

237. See *id.* For evidence that the public is catching on to problems with the capital system that have long been known only to the professionals who operate it, see, e.g., Editorial, *Seattle Post-Intelligencer*, Aug. 12, 2001 (“Last week a special report in the *Post-Intelligencer* . . . shed light on a horrible truth known mostly, but well, to the professionals who prosecute, defend, adjudicate and review the first-degree aggravated murder cases that are the only ones that can lead to lethal injection in this state. Of the existing system’s well-documented flaws, none is greater than the economic disadvantage that some capital defendants face in trying to secure justice in Washington.”).

238. See supra pp. 70-81.

239. We follow the usual convention of noting relationships where the probability that they exist by chance is 10% or less, while placing primary emphasis on, and drawing policy implications from, relationships where the probability that they exist by chance is 5% or less. For additional discussion of statistical significance, see *infra* pp. 109-10, 148.

240. See supra pp. 70-76.

241. See supra pp. 79-80 & Figure 9.

242. See supra pp. 48-61.

243. See *Furman v. Georgia*, 408 U.S. 238 (1972).

244. See *Woodson v. North Carolina*, 428 U.S. 280 (1976); supra p. 18 & n.108.

245. See supra p. 18.

246. See *infra* pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 213, 216-17, 258-59, 372-75.

247. See *infra* pp. 102-03, 222-33 (discussing [Analyses 14](#) and [15](#)), 107-08, 272-78 (discussing [Analyses 16](#) and [17](#)), 334 (discussing one group of explanatory factors studied within the context of [Analysis 19](#) that did not include time).

248. See supra pp. 20-21.

249. See *infra* pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 213, 216-17, 258-59, 372-75.

250. See 100 Colum. L. Rev., supra note 153, at 2129-36.

251. See *infra* pp. 140-42 & Table 4.

252. The one exception is Analysis 10A, which examines state- and county-level factors associated with county reversal rates at the state direct appeal stage.

253. See supra pp. 17-18; infra Appendix C, pp. C-1 to C-2; *A Broken System, Part I*, supra note 101, at 19-20, 26-27 & nn.39, 132; App. C, pp. C-1 to C-2.

254. See infra p. 99 & n. 257.

255. See supra pp. 88-92.

256. See supra pp. 14-81.

257. All of our binomial analyses are over-dispersed and logistic, even where we omit those descriptions for ease of exposition.

258. See supra pp. 89-92; infra pp. 140-42 & Table 4, 154-56, 177, 194-95, 213, 216-17, 258-59, 372-75.

259. By adding an offset to transform the dependent variable into a rate, our Poisson analyses may be used to predict rates as well as counts. All of our Poisson analyses are over-dispersed and logarithmic, even where we omit those descriptions for ease of exposition. For discussions of the rationale for using both Poisson as well as binomial regressions, see supra p.97; infra p. 109.

260. See supra pp. 21, 88-89.

261. See supra pp. 95-96.

262. See supra p. 100.

263. See supra pp. 17-18; infra Appendix C, pp. C-1 to C-2.

264. See supra pp. 17-18 & n.103.

265. See supra p. 99.

266. The numbering of analyses jumps from 6 to 14 because we conducted all our comparisons of the states' 23-year experiences last (Analyses 14-17), after first completing our state analyses (Analyses 1-6) and county-state analyses (Analyses 7-13) in which we disaggregated the analysis by year.

267. Although just over 1000 counties imposed death sentences during the study period, the year of the death sentence is known for verdicts imposed in only 967 of those counties. In studies in which the role of time is studied, we use this 967 cohort of counties. The additional counties are included in studies in which the year the death verdict was imposed is not a consideration. See infra page 107.

268. See infra p. 251 (discussing this feature of Analysis 7).

269. See supra p. 103 & n.267.

270. See supra pp. 88, 100 (discussing the rationale for treating state and year as random effects).

271. See supra pp. 102-03.

272. See also supra pp. 87, 99-100.

273. See supra pp. 97-98.

274. See infra pp. 238, Table 7; pp. 283, Table 8; pp. 286, Table 9.

275. See supra pp. 85-86.

276. See supra pp. 52, 82-83.

277. For more on measuring fit, see infra pp. 149-50 & n.311.

278. Using a random intercept (measuring unexplained variation in reversal rates from one place to another), and a measure of slope (measuring unexplained variation over time), we compare two sets of explanatory factors to see which leaves more unexplained variation. A lower value—*i.e.*, less unexplained variance—is better. In this test, the explanations generated by a given set of factors, and by the baseline analysis, are compared to see if the specific factors leave less unexplained variation than the baseline analysis. In the test discussed in the last item in the list in text, the explanations generated by two or more sets of factors are compared to see which one leaves less or the least unexplained variation.

279. A relationship that is not statistically significant—*e.g.*, one as to which there is an 89% probability that it does not appear by chance but an 11% probability that it does—conveys some useful information about factors that may be related to capital reversal rates. Nonetheless, we choose to base no findings on, and to draw no conclusions from, relationships about which we are not highly confident.

280. Our effect-size estimates are coded as “newestimates” in the detailed results of each of our Analyses 1-18 in Appendix G. For a description of how these effect-size estimates are calculated, see infra notes 283, 286.

281. See generally supra pp. 97-98, 99-100, 110.

282. Assume an increase in the homicide rate from 5 to 6 homicides per 100,000 residents. Assume, as well, that the “odds” of reversal where the homicide rate is 5 per 100,000 residents are 1 to 2, which is equivalent to a probability of 33%. (Odds of x to y can be changed to a probability using the following formula: $x/(x + y)$). So, if the odds are 1 to 2, the probability is $1/(1+2) = 1/3 = .33$.) Given these assumptions, and given a “newestimate”—or effect-size estimate—of 1.4, the regression analysis predicts that the odds of reversal where the homicide rate is 6 per 100,000 residents are (1×1.4) to 2, or 1.4 to 2, which is equivalent to a 41% probability. Where the homicide rate is 7 per 100,000 residents, the predicted odds of reversal are $(1 \times 1.4 \times 1.4)$ to 2, or 1.96 to 2, which is equivalent to a 49% probability.

283. Factors with values that are not integers (*i.e.*, values such as .43, 1.22, 10.54 as opposed to 1, 4 and 7), and particularly factors that are scaled by reference to some other population (*e.g.*, homicides per 100,000 residents) are often logged to make their effect size easier to interpret. In the example in note 282 above, we assumed an increase in the homicide rate from 5 to 6. But states rarely have an actual homicide rate per 100,000 residents that is an interger (*e.g.*, 2) as opposed to, say, 1.3 or 4.6. To help interpret effect size, therefore, the values are logged to the base 10. (For any value, n , we calculate $n = 10^x$, and use x as the new value. So if $n = 100$, then $x = 2$, because $100 = 10^2$.) The effect is to compress the values onto a much narrower

scale. All logged factors in our results are indicated by an “l” at the beginning of the variable name in our study results (e.g., “ldor”).

Effect size for logged factor values in binomial logistic analyses is calculated using the following formula. For each doubling of the value of an explanatory factor, the predicted “odds” of reversal increase by a factor of x , with x being the effect-size estimate (*i.e.*, the “newestimate”) reported in our study results. So, if homicide rates are logged in an analysis, and if the “newestimate” is 1.4, an increase from 2 to 4 homicides per 100,000 residents increases the predicted odds of reversal by a factor of 1.4. If the odds of reversal where the homicide rate is 2 per 100,000 residents are 1 to 2 (33%), the predicted reversal rate of a state with a logged homicide rate of 4 per 100,000 residents is (1×1.4) to $2 = 1.4$ to 2 (41%).

The method we used to calculate the “newestimates” for unlogged variables in binomial logistic analyses is to exponentiate the parameter estimate for the factor of interest (coded “Estimate” in the results in Appendix G). The method we used to calculate the “newestimate” for logged variables in binomial logistic analyses is to calculate 2 to the power of the parameter estimate for the factor of interest.

Numeric effect-size estimates for variables that are also components of interaction effects variables (e.g., “bltot,” which is a component of our “bltot*fac_cls” variable), need to be interpreted in conjunction with the effect-size estimates for the interaction variable. In our effect-size graphs—on which we exclusively rely in the text of this Report when discussing effect size for variables that are components of interaction variables—we display the joint effect of the two variables, but we have not performed that additional calculation in the numeric effect-size estimates.

284. The formula used to calculate effect size in the graphs of factors in our binomial analyses is (all values for the factor of interest) x (that factor’s parameter estimate (coded “Estimate” in the detailed results in Appendix G)) + (the sum of (each other factor’s parameter estimate x that factor’s mean value)). The estimates obtained from this formula then were transformed using an inverse logit transformation, which we multiplied times 100 to derive a percentage, which then served as the y-axis value on the graph. Where a factor of interest was also a component of an interaction effect, the interaction effect was taken into consideration in calculating effect size in the graphs.

285. Assume that the homicide rate increases from 5 to 6 per 100,000 residents, and that the predicted reversal rate where the homicide rate is 5 per 100,000 residents is 33%. In this event, the analysis predicts a reversal rate in states with a homicide rate of 6 per 100,000 residents of $(.33 \times 1.4)$, or .46 (46%). Where the homicide rate is 7 per 100,000 residents, the predicted reversal rate is $(.33 \times 1.4 \times 1.4)$, or .65 (65%). And so on.

286. Effect size for logged factor values in Poisson logarithmic analyses (see *supra* note 283) is calculated using the following formula: For each doubling of the value of an explanatory factor, the predicted rate of reversal increases by a factor of x , with x being the effect-size estimate (*i.e.*, the “newestimate”) reported in our study results. So, if homicide rates are logged in a study, and if the “newestimate” for that factor is 1.4, an increase from 2 to 4 in homicides per 100,000 residents increases the predicted probability of reversal by a factor of 1.4. If the reversal rate associated with a logged homicide rate of 2 is 33%, the predicted reversal rate for a logged homicide rate of 4 is $(.33 \times 1.4) = .46$, or 46%.

The method we used to calculate the “newestimate” for unlogged variables in Poisson logarithmic analyses is to exponentiate the parameter estimate for the factor of interest (coded “Estimate” in the results in Appendix G). The method we used to calculate the “newestimate” for logged variables in Poisson analyses is to calculate 2 to the power of the parameter estimate for the factor of interest. Although we used the same method to calculate “newestimates” for binomial and Poisson analyses, see *supra* note 283, the interpretation of the “newestimates” is different.

Numeric effect-size estimates for variables that are also components of interaction effects variables (e.g., “bltot,” which is a component of our “bltot*fac_cls” variable), need to be interpreted in conjunction

with the effect-size estimates for the interaction variable. In our effect-size graphs—on which we exclusively rely in the text of this Report when discussing effect size for variables that are components of interaction variables—we display the joint effect of the two variables, but we have not performed that additional calculation in the numeric effect-size estimates.

287. The formula we use to calculate effect size in the graphs of factors in our Poisson logarithmic analyses is (all values for the factor of interest) \times (that factor's parameter estimate (coded "Estimate" in the detailed results in Appendix G)) + (the sum of (each other factor's parameter estimate \times that factor's mean value)). The estimates obtained from this formula then were transformed using an exponential transformation, which served as the y-axis value on the graph. Again, where a factor of interest was also a component of an interaction effect, the interaction effect was taken into consideration in calculating effect size in the graphs.

288. The percent change is calculated using the following formula: Moving from a homicide rate of x^1 to a homicide rate of x^2 (as indicated on the horizontal axis) leads to a predicted percent change in reversal rates of $((y^2 - y^1)/y^1) \times 100$ (with the y values being those indicated on the vertical axis). In the example given: $((.4 - .2)/.2) \times 100 = (.2/.2) \times 100 = 100\%$.

289. See *infra* pp. 311-12.

290. See *supra* pp. 37-39, 65-68 & Table 5.

291. Figure 16 is the same as Figure 1A above.

292. Figure 21 is the same as Figure 6 above. As do Figures 6 and 9, Figures 20 and 21 examine non-consensual executions, meaning ones where the prisoner challenged his or her death verdict in court and, as a result, secured a judicial determination of the quality of the verdict. Excluded are executions occurring consensually, because a prisoner decided he preferred execution to prison and did not appeal his verdict or, therefore, secure a judicial determination whether the verdict was seriously flawed. Because our goal is to gauge relationships between various conditions and the presence or absence of serious, reversible capital error, verdicts carried out in the absence of any judicial determination of whether or not they were seriously flawed are less informative in this context.

293. See, e.g., Associated Press State & Local Wires, *supra* note 30 (discussing New Jersey "state Supreme Court report released [in August 2001] that found vast differences in how often prosecutors seek capital punishment" in the state); Stefan C. Friedman, District Attorneys Above the Law, *N.Y. Post*, Aug. 22, 2001 (discussing decision of some district attorneys in New York state to forgo use of the death penalty); *supra* note 30 (discussing study by legislative advisory committee finding evidence of disparities in capital-charging in Virginia); *infra* p. 396 & n.880 (discussing evidence of local disparities in capital-charging found by legislatively commissioned study of Nebraska capital practice).

294. See *supra* pp. 84-85.

295. There are almost no alleged constitutional or other violations that have a statistically significant relationship to the probability of reversal. Claims such as ineffective assistance of counsel that are the most common reasons for reversal (see *supra* pp. 40-41) are raised—and fail—in most cases and thus are not associated with a higher (or lower) probability of reversal. The one claim that is significantly related to a higher probability of relief addresses an idiosyncratic practice of a single state (Texas) that was eventually held to be unconstitutional by the U.S. Supreme Court. See *Estelle v. Smith*, 451 U.S. 454 (1981).

296. See *infra* pp. 157-59, 160-63, 165-66.

297. See *supra* pp. 89-92, 99.

298. See *supra* pp. 89-93 & n.10, 99.

299. See *supra* pp. 20-21, 91-93 & Figure 10.

300. See *supra* pp. 89-92.

301. Additional notes on data and sources:

1. DADB: For a direct appeal to be “finally decided” within the study time period, the highest state court with jurisdiction to review capital judgments in the relevant state must have taken one of two actions during the study period: (1) affirmed the capital judgment, or (2) overturned the capital judgment (either the conviction or sentence) on one or more grounds. If one of those two actions occurred prior to or during 1995, and the United States Supreme Court thereafter denied certiorari review, the case is included in the study, because the Supreme Court’s action did not affect the finality of the state decision. If the Supreme Court instead *granted* certiorari in a case but did not decide the case before or during 1995, the case is omitted from the study because the Supreme Court’s action withdrew the finality of the decision.

2. HCDB: All information on federal habeas petitions was collected from judicial decisions handed down during the study period. Starting with a list of names of individuals sentenced to death, habeas data were collected during the summers from 1994 to 1999 using either a Westlaw or a Lexis search engine. For a habeas case to be “finally decided” within that time period, all of the following events must have occurred in the case within the study period: (1) a United States District Court must have denied habeas corpus relief, thereby approving the capital judgment, or granted habeas relief from the capital judgment (either the conviction or sentence) on one or more grounds; (2) if an appeal was timely filed, a United States Court of Appeals must have approved or reversed action (1); and (3) if certiorari review was timely filed, the United States Supreme Court must have either (a) denied review or (b) granted review and affirmed or reversed action (1).

3. DRCen: Our information on death verdicts is catalogued by the name of the prisoner and state that imposed the death sentence. Some individuals appear on the list more than once because they had multiple capital verdicts during the study period. Multiple capital verdicts for the same individual may result from verdicts imposed at different trials for different offenses; verdicts imposed at different trials for crimes against different victims of the same offense; or new verdicts imposed following court reversal of earlier verdicts. Our unit of analysis is the outcome of appeals of capital verdicts. If a single court judgment was imposed at trial covering more than one victim or offense and was reviewed on appeal as a single judgment, it is counted as a single verdict. If an appellate court disaggregated a judgment imposed at trial into multiple verdicts covering different offenses or victims, affirming one or more and reversing one or more, we followed the same practice.

4. USCen: Census Bureau data on the racial composition of states are from Census Data Set PE-19 1970-79; State Estimates by Age, Sex, and Race; Estimates of the Population of States by Age, Sex, Race and Hispanic Origin: 1981 to 1989; Estimates of the Population of State by Age, Sex, Race and Hispanic Origin: 1990 to 1998; Summary Tape File 3C(STF3C).

5. UCRDB: U.S. Department of Justice, FBI, Uniform Crime Reports for the United States: Crime in the United States, 1973-1996, available in spreadsheets at <http://www.ojp.usdoj.gov/bjs/datast.htm>.

6. HomVic: Data for 1973-78 are from Vital Statistics of the United States, Mortality Detail Files, 1968-1978 (ICPSR STUDY 7632). Data for 1979-87 are from CDC Wonder, the Centers for Disease Control data extraction engine at <http://wonder.cdc.gov>. Data for 1988-95 are from Centers for Disease Control and Prevention National Center for Health Statistics, Compressed Mortality File, 1989-98 CD-ROM Series 20, No 2C ASCII Version. Through 1988, the relevant data sources list homicide victims by county of death. After 1988, the relevant data source lists homicide victims by county of residence.

7. PrisCen: U.S. Department of Justice, Bureau of Justice Statistics, Source Book of Criminal Justice Statistics 1973-1996.

8. CtCaLd: Inter-University Consortium for Political and Social Research, State Court Statistics 1985-1994 (ICPSR 9266, 1995). Information on the filings and dispositions of state court cases are only available for 1985 to 1995.

9. CtExpen: Expenditure and Employment Data for the Criminal Justice System [United States]: CJEE Extract Files 1971-1995, available from the Inter-University Consortium for Political and Social Research, ICPSR datasets: 2257, 2940, 6006, 6459, 6579, 6795, 7618, 8382, 8455, 9160, 9161, 9162, 9396, 9554, 9773.

10. Messner county data: Professor Messner and his colleagues collected data on every county in the United States for the decennial census years 1970, 1980 and 1990. The intervening years were interpolated using SPSS's linear interpolation procedure.

11. PolPres: See C. Flango & D. Rottman, Appellate Court Procedures (Nat'l Center for State Courts 1998).

302. See supra pp. 97, 100, 110.

303. See generally supra pp. 109-17.

304. See supra p. 140.

305. Full results of these analyses and all others for Analyses 1 and 2 are available in Appendix G.

306. $p = .0006$ in both Analyses 1 and 2. We indicate the significance levels here to assist the reader in comparing this first discussion of results to the more fuller description of results in Appendix G. Below, we do not give specific significance levels in every case and instead describe the significance levels more generally (making the usual assumption that a level of .05 or less is "significant," that a level of less than .0001 is "highly significant," and that a level greater than .05 but less than .1 is worth noting. See supra pp. 85-86 & n.239, 109-10; infra p. 148. In regard to the random intercept, see supra note 278.

307. $p = .01$ (Analysis 1) and $.009$ (Analysis 2). In regard to slope, see supra note 278.

308. See supra pp. 85-86 & n.239, 109-10.

309. See supra p. 112; infra note 311.

310. On the baseline analysis, see supra pp. 113 & n.278, 147.

311. Our measure of fit is the -2 Res Log Likelihood. In general, one expects an improvement in fit of 1 for each additional factor tested beyond those included in the baseline analysis. Improvements in fit compared to the baseline inquiry of about 2 or more per added factor tend to be statistically significant in analyses with the number of factors that our analyses add to the baseline inquiry. See Critical Values of the Chi-Square Distribution, in Jeffrey M. Wooldridge, *Introductory Econometrics: A Modern Approach* App. G. (1999). Below we list the number of factors added to the baseline by Analyses 1A-2B and the improvement in fit achieved by each. All are highly significant.

	No. of Factors Added to Baseline	Improvement in Fit
<u>Analysis 1 (binomial):</u>		
<u>Analysis 1A</u>	10	86
<u>Analysis 1B</u>	10	99
<u>Analysis 2 (Poisson):</u>		
<u>Analysis 2A</u>	10	151
<u>Analysis 2B</u>	10	89

312. See supra pp. 146-47 & nn.306-07.

313. Statistical significance is discussed supra pp. 85-86 & n.239, 109-10, 148. As the coefficients decrease, indicating less unexplained variance, significance values (in parentheses in Table 5) get higher, meaning that the probability that the remaining variation appears merely chance is growing. A significance value higher than .05 indicates that one cannot be confident that the variance left unexplained by the set of factors being analyzed is more than a chance occurrence—*i.e.*, that the set of factors has explained most of the variance.

Correlation matrices are provided in Appendix F. They show that our measure of the effectiveness of law enforcement (arrests, convictions and incarcerations per 100 FBI Index Crimes) is correlated with the passage of time, and that our measure of general court cases is correlated with our measure of population structure. We left these factors in the model because we believe that all of them are conceptually important in order to test our hypotheses, and also because the correlated factors operate in distinct conceptual domains—*i.e.*, they measure conditions whose sources are independent. As a check, we omitted the weaker of the two correlated variables (given significance levels and effect size) from Analysis 1A to see if doing so changed our results. We first omitted only the measure of general court caseloads. Then, we omitted both that factor and time trend. In each case, the impact on fit was minimal, and the other factors remained significant or were between the .05 and .1 level. (When the measure of general court caseloads is omitted from Analysis 1A, $p = .06$ for white compared to black homicide victimization rates, and also for the interaction between that factor and the African-American proportion of the population. When both general court caseloads and time are omitted, $p = .09$ for those two factors. All other factors remain significant at the .05 level or better.) Removing the two variables did, however, weaken the explanatory power of the model, leaving substantially more unexplained variance than Analysis 1A, while not substantively changing the interpretation or implications of the analysis or the important variables. Under these circumstances, we conclude that more is learned by including the two variables than by excluding them, for three primary reasons. First, some degree of multi-collinearity is inevitable in measures of real-world processes or phenomena. Second, the correlated variables are conceptually important to test our hypotheses, as is discussed at length below. Finally, the correlated variables are not correlated with the explanatory variable on which we premise our principal

conclusion: states' and counties' death-sentencing rates.

314. See supra pp. 89-91, 99, 140-42.

315. During the study period, 5826 verdicts were imposed. Of those, 2109 were reversed at the first two review stages and thus were removed from the system before reaching the final review phase. 598 verdicts were finally reviewed at the third, federal habeas stage. That leaves 3119 verdicts (54% of the total) that were imposed but were not finally reviewed during study period. We know that during that period, 1280 verdicts (22% of the total) were not finally reviewed at *any* of the three review stages. Our best estimate is that about the same number of verdicts— 1232—cleared the first stage of review but were stuck at the second stage of review as of the end of the study period, and that about 607 verdicts cleared the second stage of review but did not complete the third stage by the end of the study period. See supra note 103.

316. See supra pp. 88-90.

317. See supra pp. 89-90.

318. This factor is coded “bltot” in the detailed results in Appendix G. At pp. 89-90 above, we explain why we made this variable a count, not a rate tied, *e.g.*, to each state's population, number of homicides or number of death sentences. In brief, we designed the variable to treat big and small states the same, given their susceptibility to the same bottle-necking effect of having a single state high court responsible for two of the three review phases. In any event, our use of an explanatory factor for population size, see *infra* p. 168, introduces a control for differences in the size of state populations.

319. This four-part “factor” is coded “fac_csld” in the detailed results in Appendix G.

320. This factor is coded “rgrtbklg” in the detailed results in Appendix G. Both alternative measures of all cases awaiting review overlap our measure of backlogged capital cases, because capital cases are included in the categories of filed felony cases, and all filed cases. Capital cases, however, make up a tiny proportion of all filed, and even all filed felony, cases.

321. As we develop below, effect size for this factor is too small to warrant much attention to it.

322. $p = .02$ (Analysis 1B and 2A); $p = .06$ (Analysis 1A).

323. $p = .01$.

324. See supra p. 93, Figure 10.

325. See supra pp. 89-91, 99, 140-42, 152-53.

326. See supra pp. 20-21, 91-93 & Figure 10.

327. This factor is coded “year-n” in the detailed results in Appendix G.

328. Below, we compare the baseline inquiry and our analyses of specific factors using three measures—*t* values, effect size and significance. A negative sign indicates an inverse relationship between the passage of time and reversal rates. Weaker relationships between the passage of time and error rates as one moves from the baseline to the specific analysis, are indicated by lower *t* values, smaller distances between the effect size coefficient and 1, and declining significance (indicated by a higher probability that the relationship appears by chance). Of the three measures, effect size is the most useful for present purposes.

Strength of the Relationship Between the Passage of Time
and Capital Error Rates: Analyses 1 and 2

	<i>t</i> Value	Effect Size	Significance
<u>Analysis 1</u> (binomial)			
Baseline Analysis	-9.76	.82	<.0001
<u>Analysis 1A</u>	-2.77	.94	.006
<u>Analysis 1B</u>	-2.63	.95	.009
<u>Analysis 2</u> (Poisson)			
Baseline Analysis	-6.36	.89	<.0001
<u>Analysis 2A</u>	-2.47	.96	.014
<u>Analysis 2B</u>	-2.85	.95	.005

329. See *infra* pp. 194-95, 258-59.

330. See, e.g., *McCleskey v. Kemp*, 481 U.S. 279 (1987) (opinion of Stevens, J., dissenting) (summarizing relevant studies); David C. Baldus et al., *Equal Justice and the Death Penalty: A Legal and Empirical Analysis* (1990); David C. Baldus, George Woodworth & Charles A. Pulaski, Jr., *Reflections on the ‘Inevitability’ of Racial Discrimination in Capital Sentencing and the ‘Impossibility’ of its Prevention, Detection, and Correction*, 51 Wash. & Less L. Rev. 359, 386 n.115 (1994) (citing studies); J.C. Beck & R. Shumsky, *A Comparison of Retained and Appointed Counsel in Cases of Capital Murder*, 21 L & Hum. Behav. 525, 534 (1997); William J. Bowers, *The Pervasiveness of Arbitrariness and Discrimination Under Post-Furman Capital Statutes*, 74 J. Crim. L. & Criminol. 1067 (1983); William J. Bowers & Glenn L. Pierce, *Arbitrariness and Discrimination Under Post-Furman Capital Statutes* 26 Crime & Delinq. 563 (1980); Samuel R. Gross & Robert Mauro, *Patterns of Death*, 37 Stan. L. Rev. 27 (1984); Joseph E. Jacoby & Raymond Paternoster, *Sentencing Disparity and Jury Packing: Further Challenges to the Death Penalty*, 73 J. Crim. L. & Criminol. 379 (1982); T. J. Keil & G. F. Vito, *Race and the Death Penalty in Kentucky Murder Trials: 1976-1991*, 20 Am. J. Crim. Justice 17 (1995); Raymond Paternoster & A. Kazyaka, *Racial Considerations in Capital Punishment: The Failure of Evenhanded Justice*, in *Challenging Capital Punishment: Legal and Social Science Approaches* 113-48 (K. C. Haas & J. A. Inciardi eds., 1988); Raymond Paternoster, *Prosecutorial Discretion and Capital Sentencing in North and South Carolina*, in *The Death Penalty in America: Current Research* (R. M. Bohm ed. 1991); J. R. Sorensen & D. H. Wallace, *Capital Punishment in Missouri: Examining the Issue of Racial Disparity*, 13 Behav. Sci. & L. 61 (1995); E. Thomson, *Research Note: Discrimination and the Death Penalty in Arizona*, 22 Crim. Justice Rev. 65 (1977); U.S. Gen. Accounting Office, *Death Penalty Sentencing: Research Indicates Patterns of Racial Disparities*, Report # GGD-90-57 (Feb. 26, 1990).

331. This factor is coded “pctblack” in the detailed results in Appendix G.

332. We also considered whether the proportion of *nonwhites* in a state's population, and not just the proportion of African-Americans, is associated with high error rates, but the proportion of African-Americans had more explanatory power in terms of significance, fit explained variance and effect size.

333. In a number of these analyses, we used factors (focused on the race of the victims of the offenses for which death sentences were imposed) for which we were missing data for 23 of the 519 observed "state-years." This required us to omit those state-years, diminishing the number of observations to 496, which automatically lowers the fit value somewhat. The fit for the Analysis 1A models with 23 missing values thus cannot be compared to the fit for analyses with all 519 observations.

334. One factor focused on the race of people sentenced to die did have a significant effect in some Analysis 1R models, but that relationship tends to confirm the importance of the size of the state's African-American population generally. The factor is the percentage of blacks among those the state sentenced to die minus the percentage of blacks in the state's population (coded "pctpdiffnew" in the detailed results in Appendix G). In some analyses, this factor was negatively related to error rates, meaning states with lower proportions of blacks in their population than on death row tend to have lower reversal rates, while states with higher proportions of blacks in their population than on death row tend to have higher reversal rates. This factor was only occasionally significant, and added nothing to the power of the pre-existing set of factors, so we give it little weight. But it tends to support the finding of Analyses 1 and 2 that it is the proportion of African-Americans in the states' *population*, not their proportion among the people states *sentence to die*, that is associated with higher error rates.

335. We do not calculate these reversal rates by race for the state post-conviction stage, because we do not have sufficient information for that stage to compute them accurately. See *supra* pp. 17-18; Appendix C, pp. C-1 to C-2.

336. The only racial disparity of any note suggested by Table 6 is for death verdicts imposed on defendants who are neither African-American nor white, which have a somewhat *lower* reversal rate, especially at the habeas stage. The numbers are small, however, especially at the habeas stage, where the relationship is not significant when other factors are considered.

337. See, e.g., United States General Accounting Office Report on Pattern of Racial Disparities 330 (collecting and evaluating studies documenting effects of victim's race on decision to seek the death penalty and concluding that, in "82 percent of the studies, race of victim was found to influence the likelihood of being charged with capital murder or receiving the death penalty, *i.e.*, those who murdered whites were found to be more likely to be sentenced to death than those who murdered blacks. This finding was remarkably consistent across data sets, states, data collection methods, and analytic techniques. The finding held for high, medium, and low quality studies."); David C. Baldus et al., Equal Justice and the Death Penalty, *supra* note 330, at 370-93; David C. Baldus, et al., Racial Discrimination and the Death Penalty in the Post-Furman Era: an Empirical and Legal Overview, with Recent Findings from Philadelphia, 83 Cornell L. Rev. 1638, 1658-61 & n.69 (1998) (collecting numerous additional sources, many of them conducting studies of particular capital states); John Blume, Theodore Eisenberg and Martin T. Wells, Explaining Death Row's Population and Racial Composition, Cornell Law School (Draft Oct. 19, 2001); Bowers & Pierce, *supra* note 330, at 594 tb. 2; Deon Brock, Nigel Cohen & Jonathan Sorensen, Arbitrariness in the Imposition of Death Sentences in Texas: An Analysis of Four Counties by Offense Seriousness, Race of Victim and Race of Offender 22 (November 2001) ("Across the state, and within each of the major jurisdictions . . . , the prevalence and consistency of disparities based on the race of the victim indicate a pattern of arbitrary sentencing. These findings are consistent with other studies performed in Texas and elsewhere . . . "); David Cole, No Equal Justice: Race and Class in the

American Criminal Justice System 133-50 (1999); Samuel R. Gross, Robert Mauro, Patterns of Death: An Analysis of Racial Disparities in Capital Sentencing and Homicide Victimization, 37 *Stan. L. Rev.* 27 (1984); Randall Kennedy, Race, Crime and the Law 328-42, 450-51 *nn.*50-51 (1997) (extensively canvassing the literature); Death Row USA, Summer 2001 (reporting that 80.7% of executions were white victim cases), available at <http://www.death-penaltyinfo.org/DRUSA-ExecUpdate.html>; Associated Press State & Local Wires, *supra* note 30 (discussing New Jersey state supreme court report released in August 2001 that “found that those convicted of killing white victims are far more likely to face the death penalty than those convicted of killing black victims” in New Jersey). For other research, see sources cited *supra* note 330.

338. The classic demonstration of this point, extensively documented, is Kennedy, *supra* note 337, at 29-75, 326-50, 400-10. See also Baldus, et al., *supra* note 337; Blume, Eisenberg & Wells, *supra* note 337, at 1-2, 32-33; Bowers & Pierce, *supra* note 330, at 601; Richard H. Burr, III, Representing the Client on Death Row: The Politics of Advocacy, 59 *U.M.K.C. L. Rev.* 1 (1990); James Alan Fox & Jack Levin, *The Will to Kill* 167 (2001); Richard C. Dieter, *The Death Penalty in Black and White: Who Lives, Who Dies, Who Decides* (Death Penalty Information Center, June 1998) (Figure 7, summarizing studies); Gross & Mauro, *supra* note 337; Gary Kleck, Racial Discrimination in Criminal Sentencing: A Critical Evaluation of the Evidence with Additional Evidence on the Death Penalty, 46 *Am. Soc. Res. Rev.* 783 (1981); Raymond Paternoster, Race of Victim and Location of Crime: The Decision to Seek the Death Penalty in South Carolina, 74 *J. Crim. L. & Criminol.* 754 (1983).

339. See *infra* pp. 213, 224-26, 253, 280.

340. This factor is coded “wbrtst” in the detailed results in Appendix G.

341. See *infra* Appendix G (reporting the mean and maximum values on this “wbrtst” factor among states and years in our study, showing that the white homicide victimization rate is usually lower than the black homicide victimization rate).

It is not uncommon in social scientific and criminological research to discover that conditions that might be expected to have an impact on a phenomenon being studied do so only when measured comparatively, as opposed to absolutely. To give a well known example, although absolute differences in average income among different metropolitan areas and different times periods are not significantly related to differences in crime rates, relative differences in income—measured by dividing the income of the most well-off cohort of citizens by the income of the least well-off—is a strong predictor of crime rates (which tend to be higher in communities with greater income disparities than in communities where there is less income inequality). See John Kaplan, Robert Weisberg & Guyora Binder, *Criminal Law* 74 (4th ed. 2000) (summarizing studies). Our measure of comparative homicide rates among white and blacks is in this same tradition.

342. $p = .02$ to $.05$ for Analyses 1A-2B.

343. See *supra* pp. 157-59.

344. These three factors are coded, respectively, “ldfwvrt,” “wvldfst” and “facvic21” in the results of Analysis 1R in Appendix G.

345. See *infra* pp. 313-33.

346. This interaction is coded as $pctblack * wbrtst$ in the detailed results in Appendix G.

347. The interaction was significant at the .02 level in Analysis 1A; at the .05 level in Analyses 2A and 2B; at the .06 level in Analysis 1B; and at the .03 to .04 level in Analyses 1RA, 1RB and 1RC.

348. See supra pp. 158-62 & Table 6.

349. See, e.g., *Chisom v. Roemer*, 501 U.S. 380, 385-86 (1991) (noting that no African-American has ever been elected to the Louisiana Supreme Court); *HLA v. Attorney General of Texas*, 501 U.S. 419, 421-22 (1991) (noting that only 5% of the trial judges in Harris County (Houston), Texas were African-American, although its population is 20% African-American); Nicholas O. Alozie, *Black Representation on State Judiciaries*, 69 Soc. Sci. Q. 979 (1988) (finding that key factor in under-representation of blacks on state judiciaries is their under-representation among members of state bar); Associated Press, *Second Black Alabama Supreme Court Justice Sworn In*, Columbus (Ga.) Ledger-Enquirer, Nov. 2, 1993, at B2 (noting that there was only one African-American among Alabama's 17 appellate court judges, and only 12 blacks among the state's 255 circuit and district court judges); Stephen B. Bright, *Discrimination, Death and Denial: The Tolerance of Racial Discrimination in Infliction of the Death Penalty*, 35 Santa Clara L. Rev. 433, 437-38, (1995) ("Members of racial minorities continue to be excluded as judges, jurors, prosecutors, lawyers, and law enforcement officials in the criminal justice system"); Bureau of Justice Statistics, *Local Police Departments, 1997*, NCJ 17329, at 3 tbl. 5 (1998) (racial makeup of nation's police forces); Law Enforcement Management and Administrative Statistics, 1997: Data for Individual State and Local Agencies with 100 or More Officers, NCJ 171681 (1998) (same) ; Mark Curriden, *Racism Mars Justice in U.S. Panel Reports*, Atlanta J. & Const., Aug. 11, 1991 (observing that only 6 of Georgia's 134 Superior Court judges were African-American, and those six were in three judicial circuits; none of state's elected district attorneys in Georgia are black); Draft Final Report of the Special Committee on Race and Ethnicity to the D.C. Circuit's Task Force on Gender, Race, and Ethnic Bias (Jan. 1995), at 31 n.50 (noting that 3.4% of nation's practicing attorneys are African-American); Dwight L. Greene, *Abusive Prosecutors: Gender, Race & Class Discretion and Prosecution*, 39 Buff. L. Rev. 737, 780-81, 802 n.102, (1991) ("African-American judges constitute approximately 3.8% of all state court judges, but are disproportionately concentrated in part-time and limited jurisdiction courts [without jurisdiction in capital cases]. The percentage of African-Americans on the bench roughly mirrors the percentage of lawyers in the country who are African-Americans, approximately 3%." (citing data from Graham, *Judicial Recruitment and Racial Diversity on State Courts*, 74 *Judicature*, 28, 30, 34 (1990) (discussing "the problem of the lack of racial diversity on the state bench")); Alex M. Johnson, Jr., *The Underrepresentation of Minorities in the Legal Profession*, 95 Mich. L. Rev. 1005, 1007-11 (1997) (comprehensive review of statistics); Joint Center for Political Studies, *Black Judges in the United States* (1986); Lewis A. Kornhauser & Richard L. Revesz, *Legal Education and Entry into the Legal Profession: The Role of Race, Gender, and Educational Debt*, 70 N.Y.U. L. Rev. 829 (1995); Jonathan P. Nase, *Pennsylvania's Appellate Judges, 1969-1994*, at 514 & Exhibit 11, 33 Duq. L. Rev. 377 (1995); Sourcebook of Criminal Justice Statistics 67-68 & tbls. 1.77, 1.78 (2000) (racial makeup of federal judiciary); Rorie Sherman, *Is Mississippi Turning?*, Nat'l. L. J., Feb. 20, 1989, at 1, 24 (only 2.6 % of all state court judges in the United States are black); Jesse Smith & Robert Johns, *Statistical Record of Black America* 774 -75 (3d ed. 1995).

350. See, e.g., *Cole*, supra note 337, at 102-05; Albert Alschuler & Andrew G. Deiss, *A Brief History of the Criminal Jury in the United States*, 61 U. Chi. L. Rev. 867, 894-96 (1994); Joan Biskupic, *The Push Is on for More Diverse Juries*, USA Today, Aug. 28, 2001 ("interviews with more than three dozen court officials, defense lawyers, prosecutors and law professors indicate that minorities are significantly under-represented in jury pools. Scattered lawsuits across the USA include statistics supporting that notion."); Tanya E. Coke, *Race Neutrality and the Ideology of Representative Juries*, 69 N.Y.U. L. Rev. 327, 345-46 (1994); Hiroshi Fukurai, *Race and the Jury* 21-24, 64 (1993); Morris B. Hoffman, *Peremptory Challenges Should Be Abolished: A Trial Judge's Perspective*, 64 U. Chi. L. Rev. 809, 851 & n.192 (1997); Kennedy, supra note

337, at 232-37, 435-36 n. 4 (“Even in the absence of illegal racial discrimination, traditional methods of jury selection often yield substantial ‘underrepresentation’ of blacks . . . mean[ing] the percentage of blacks serving on juries is lower than the percentage of blacks living in the jurisdictions from which jurors are drawn.”); Nancy J. King, *Racial Jurymantering: Cancer or Cure? A Contemporary Review of Affirmative Action in Jury Selection*, 68 N.Y.U.L. Rev. 707, 712-20 (1993) (detailing reasons why “minorities remain underrepresented on juries”); Stephanie Domitrovich, *Jury Source Lists and the Community’s Need to Achieve Racial Balance on the Jury*, 33 Duq. L. Rev. 39, 42 (1994).

351. See, e.g., Blume, Eisenberg & Wells, *supra* note 337, at 32-33; Bright, *supra* note 349, at 454-59 (“This ‘death qualification’ process often results in the removal of more prospective jurors who are members of minority groups than those who are white . . . [because] minority jurors [more often] have reservations about the death penalty”); James Alan Fox et al., *Death Penalty Opinion in the Post-Furman Years*, 18 N.Y.U. Rev. L. & Soc. Change 499, 503 (1990-91); Samuel R. Gross, *Update: America Public Opinion on the Death Penalty—It’s Getting Personal*, 83 Cornell L. Rev. 1448, 1451 (1998) (“Race and sex, the two major demographic predictors of death penalty attitudes, continue to be influential on every survey. On the 1996 GSS, for example, blacks were far less likely to favor the death penalty than whites (51% to 75%).”); Phoebe C. Ellsworth & Samuel R. Gross, *Hardening of the Attitudes: Americans’ Views on the Death Penalty*, 50 J. Soc. Issues 19, 21 Summer 1994. In regard to the automatic exclusion of prospective jurors who oppose the death penalty, see, e.g., *Lockhart v. McCree*, 476 U.S. 162 (1986); *Wainwright v. Witt*, 469 U.S. 412 (1985).

352. See sources cited *supra* note 351.

353. See Albert W. Alschuler, *The Supreme Court and the Jury: Voir Dire, Peremptory Challenges, and the Review of Jury Verdicts*, 56 U. Chi. L. Rev. 153, 210-11 (1989); David C. Baldus, George Woodworth, David Zuckerman, Neil Alan Weiner & Barbara Broffitt, *The Use of Peremptory Challenges in Capital Murder Trials: A Legal and Empirical Analysis*, 3 U. Pa. J. Const. 3 (2001) (“Our findings indicate that venire member race was a major determinant in the use of peremptories”); Bright, *supra* note 349, at 454-59 (“Often the ‘death qualification’ process reduces the number of minority jurors to few enough that those remaining can be eliminated by the prosecutor with peremptory strikes. Even when jurors who express reservations about the death penalty indicate they can put aside their personal views and consider it, the prosecutor may justify his or her strikes with the hesitancy of those jurors to impose the death penalty”; documenting specific instances and systemic policies in particular prosecutor’s offices of using peremptory strikes against blacks because of their tendency to vote against the death penalty); Cole, *supra* note 337, at 115-23; Andrew Hammell, *Discrimination and Death in Dallas: A Case Study in Systematic Racial Exclusion*, 3 Tex. Forum Civ. Rts. & Civ. Lib. 187 (1998); Michael Higgins, *Few Are Chosen*, A.B.A. J. Feb. 1999, at 50-51; Steve McGonigle & Ted Timms, *Prosecutors Routinely Bar Blacks, Study Finds*, Dallas Morning News, March 9, 1986; Barry Siegel, *Storm Still Lingers Over Attorney’s Training Video*, L.A. Times, Apr. 29, 1997.

The evidence in this and the preceding notes suggests another hypothesis: Efforts to keep black jurors from deciding capital cases may be especially common in states with large black populations, and especially likely to lead to capital reversals. Some capital cases are indeed reversed because of unlawful efforts to keep blacks off of capital juries. But the number of such reversals is too small—less than 5% of all reversals at each of the two review stages where the bases for reversal are known, see *supra* pp. 40-42 & n.169—to account for our regression finding. In addition, this explanation predicts that error will be more common in black-defendant and white-victim cases, because it is in those cases that prosecutors have the greatest incentive to keep black jurors from sitting. But the evidence is to the contrary: Error is no more, or less, common in black-defendant and white-victim cases. See *supra* pp. 158-62 & Table 6.

354. See, e.g., Baldus, Woodworth, Zuckerman, Weiner & Broffitt, *supra* note 353, at 124-25; William J. Bowers, Benjamin D. Steiner & Marla Sandys, *Death Sentencing in Black and White: An Empirical Analysis of the Role of Jurors' Race and Jury Racial Composition*, 3 *U. Pa. J. Const. L.* 171, 259-60 (2001); Theodore Eisenberg, Stephen P. Garvey & Martin T. Wells, *Forecasting Life and Death: Juror Race, Religion, and Attitude Toward the Death Penalty*, 30 *J. Legal Stud.* 277, 308-09 (2001) (empirical study of capital trial outcomes concluding that “[d]eath sentences depend not only on the defendant’s race, and not only on the victim’s race but on the juror’s race as well. All else being equal, white jurors are more apt to vote for death than black jurors . . .”).

355. See *supra* pp. 158-62.

356. See, e.g., *infra* pp. 166-67.

357. See sources cited *supra* note 337.

358. The social scientific literature here is voluminous and consistent in regard to the tendency on the part of white individuals, including actors in the criminal justice and trial system, to, in Professor Kennedy’s phrase, “use race as a proxy for an increased risk of criminality.” Kennedy, *supra* note 337, at 137. Extensively canvassing the empirical literature and providing case studies and examples documenting the intensity and inaccuracy of the association between African-Americans and crime, particularly violent crime, are, e.g., Kennedy, *supra* note 337, at 136-67, 420 n.2; Ericka L. Johnson, “A Menace to Society”: The Use of Criminal Profiles and its Effects on Black Males, 38 *Harv. L.J.* 629 (1995); Cynthia Kwei Yung Lee, *Race and Self-Defense, Toward a Normative Conception of Reasonableness*, 81 *Minn. L. Rev.* 367, 402-33 (1996); Jon Hurwitz & Mark Peffley, *Public Perceptions of Race and Crime: The Role of Stereotypes*, 41 *J. of Pol. Sci.* 375, 380, 393-94, 399-401 (1997) (extensive review of literature and citation of sources documenting consensus based on “recent national surveys” “that the image of blacks as a violent underclass has become a central component of contemporary white stereotypes of African-Americans,” and that “one of the most popular negative beliefs expressed about ‘most’ blacks is that they are ‘violent and aggressive’”; reporting results of authors’ own empirical study finding that much thinking about crime in fact is not rooted in racial stereotypes with one exception: “Only when crimes are violent and when policies are punitive are negative stereotypes substantially more likely to see blacks as guilty of crimes, to envision more crimes in the future, and to favor harsher punishments.”). See also, e.g., F.C. Dane & L.S. Wrightsman, *Effects of Defendants’ and Victims’ Characteristics on Jurors’ Verdict*, in *The Psychology of the Courtroom* 83-115 (N.L. Kerr & R.M. Bray (eds. 1982); Birt L. Duncan, *Differential Social Perception and Attribution of Intergroup Violence: Testing the Lower Limit of Stereotyping of Blacks*, 4 *Personality & Soc. Psychol.* 590, 592-96 (1976); Randall A. Gordon, Jennifer L. Michels & Caroline L. Nelson, *Majority Group Perceptions of Criminal Behavior: The Accuracy of Race-Related Crime Stereotypes*, 26 *J. Applied Soc. Psych.* 148-59 (1988) (empirical study in which whites consistently overestimated the number of violent crimes blacks commit, particularly motor vehicle theft, rape and criminal homicide); Mark Peffley & John Hurwitz, *Racial Stereotyping in Contemporary White Society: Sources and Political Consequences*, in *Perception and Prejudice: Race and Politics in the United States* (John Hurwitz & Mark Peffley eds. 1998); M Sunnafrank & N.E. Fontes, *General and Crime Related Racial Stereotypes and Influence of Juridic Decisions*, 17 *Cornell J. Soc. Rel.* 1 (1983).

359. The highest proportion of a state’s population that was African-American in any state and year in our study is 36%.

360. Strong empirical support for this proposition is found in existing social scientific research. See Teven E. Barkan & Steven F. Cohn, *Racial Prejudice and Support for the Death Penalty by Whites*, 31 *J. Res. Crime*

& Delinq. 202-09 (1994) (reviewing study of interviews in connection with general nationwide survey of 1150 white adults indicating that white support for the death penalty is associated with antipathy to blacks and with racial stereotyping and discussing adverse impact these racial attitudes may have on legislative policy making in regard to the death penalty); Hurwitz & Peffley, *supra* note 358, at 393-94 (finding link between racial stereotypes associating blacks with past and assumed future violent crimes and support for harsher punishments).

361. See *supra* p. 125.

362. See John Blume & Theodore Eisenberg, *Judicial Politics, Death Penalty Appeals and Case Selection: An Empirical Study*, 72 S. Cal. L. Rev. 465, 469 (1999).

363. In contrast to our data, which we gathered ourselves from official court reports, the BJS information is provided to the Justice Department by state prison administrators based on their counts of people admitted to death row in state prisons. The BJS information is less complete than ours, as a result of (1) BJS's unconfirmed, word-of-mouth method of collecting data; (2) its failure to count death-sentenced individuals who were not on death row on a reporting date, either because they remained in local jails or entered and left death row between reporting dates; (3) its focus on the number of individuals on death row, not the number of capital verdicts they represent, so that an individual sentenced to die by different juries at different trials for different offenses, or *reconvicted* and/or *resentenced* to death at a new trial by a different jury after the first verdict was reversed only counts as one event; (4) inaccuracies in its scheme for dating death sentences, which sometimes reports death sentences as having occurred in a later year than the person reached death row; (5) BJS's failure to distinguish wholesale reversals due to invalid capital statutes and reversals due to erroneous applications of presumptively valid statutes (see *supra* p. 18); and most importantly for present purposes (6) BJS's failure to collect any information reflecting the stage of review (state direct appeal, state post-conviction or federal habeas) at which court reversals occurred.

364. This factor is coded "dor" in the results collected in Appendix G—following Blume and Eisenberg's "death obtaining rate" terminology.

365. In any event, this factor was highly significant, even when we controlled for the number of death verdicts imposed and awaiting review, see *supra* pp. 153-54, and for the size of the population, see *infra* p. 170.

366. See *supra* pp. 165-66.

367. This factor is coded "pnindx" in the detailed results in Appendix G. FBI index crimes are murder, forcible rape, aggravated assault, robbery burglary, larceny and auto theft.

368. See e.g., Erin Emery, *Mystery Remains in Deputy's Shooting*, Denver Post, Dec. 27, 2001 (noting drive to recall Arapahoe County District Attorney Ed Rogers for failing to seek death penalty in fatal shooting of a county sheriff's deputy); 100 Colum. L. Rev., *supra* note 153 at 2078-82 & nn.138-40 (extensively documenting political pressures that can be, and have been successfully, brought to bear on prosecutors to use the death penalty).

369. See Angela J. Davis, *The American Prosecutor: Independence, Power, and the Threat of Tyranny*, 86 Iowa L. Rev. 393, 450-51 (2001) (discussing widespread use of elections to select district attorneys).

370. Analyses 1A, 2B, and 1RA-C use the first index of political pressure on judges (coded “ppindx” in the detailed results in Appendix G). Analyses 1B and 2A use the second index (coded “ppindx2”). Effect sizes are slightly greater for the second than for the first index, as the “newestimates” in Analyses 1A, 1B, 2A and 2B reveal. From the standpoint of statistical significance, it usually did not matter which index we used in a particular analysis; where one was statistically significant, both usually were. The two indexes did, however, modestly affect the “fit” and amount of unexplained variance. It was on those bases, therefore, that we identified two analyses that included the first political pressure index (Analyses 1A and 2B), and two other analyses with included the second index (Analyses 1B and 2A), as the preferred Analysis 1 and Analysis 2 models.

371. See, e.g., Steven F. Messner, Luc Anselin, Robert D. Baller, Darnell F. Hawkins, Glenn Deane & Stewart E. Tolnay, *The Spatial Patterning of County Homicide Rates: An Application of Exploratory Spatial Data Analysis*, 15 *J. Quant. Criminol.* 423 (1999); Robert D. Baller, Luc Anselin, Steven F. Messner, Glenn Deane, and Darnell F Hawkins (2001); *Structural Covariates of U.S. Country Homicide Rates: Incorporating Spatial Effects*, *Criminology* 39 (3): 561-590. We are grateful to Professor Messner and his colleagues for sharing their county homicide and related data with us. Our state version of this factor is coded “psst” in the detailed results in Appendix G.

372. Analysis 1A (p = .004); Analysis 1B (p = .002); Analysis 2A (p = .008); Analysis 2B (p = .03); Analysis 1RA (p = .002); Analysis 1RB (p = .0003); Analysis 1RC (p = .0002).

373. See supra pp. 152-55.

374. Interaction effects are calculated by multiplying the two component factors—*i.e.*, here, our measure of backlogged capital verdicts, coded “bltot” (described supra pp. 153-54 & n.318) and our four-factor measure of caseloads of all sorts, coded “fac_csld” (described supra pp. 154-55 & n.319). This interaction effect is coded “bltot*fac_csld” in the detailed results in Appendix G. In Analysis 2B, we used the alternative, one-factor measure of general court congestion (coded “rgrtbklg”), see supra p. 154 & n.320, and accordingly included that factor in the interaction effect (coded “bltot*rgrtbklg”).

375. See supra pp. 114-17 & nn.282, 285.

376. See supra pp. 88-98.

377. See supra pp. 20-21, 88-89 (explaining why true error rates are reversals as a proportion of actually reviewed verdicts).

378. See supra pp. 162-63 (discussing the interaction between high proportions of African-Americans in the state population and high rates of white compared to black homicide victimization); supra pp. 170-71 (discussing the interaction between high backlogs of capital cases awaiting review and high per capita rates of court filings). Effect size for factors that also are components of interaction effects are a combination of the main effect and the interaction effect.

379. See supra pp. 89-90, 97, 99, 153-54, 173-74.

380. As is indicated in Appendix F, the average number of capital cases awaiting appeal for all states and years in Analyses 1 and 2 is between 5 and 6, and the maximum is 49.

381. These factors are discussed supra p. 154. Except in Figure 23B, the direction of the (very small) effect on reversal rates is upward—even though in the regressions themselves this factor was negatively related to reversal rates. The reason for this change in direction is that effect size takes into consideration not only the effect of the factor itself, but also the effect of any interaction variable that includes the factor as one of its components. As is discussed above, these measures of general court caseloads are also a component of an interaction variable in Analyses 1 and 2 that measures the combined effect of capital and noncapital backlogs. See supra pp. 170-71 & n.374. Moreover, that interaction variable was *positively* related to reversal rates. See *id.* Evidently, these court-caseload factors' upward influence on reversal rates as components of the interaction effect is stronger than their downward effect on reversal rates, when considered by themselves— accounting for the slight upward direction of the lines in Figures 23A, 23C and 23D.

382. See supra p. 93, Figure 10; p. 142, Table 4.

383. See supra pp. 89-91, 99, 140-42 & Table 4, 154-56.

384. See supra pp. 20-21, 91-93 & Figure 10, 140-42, 155.

385. See supra pp. 89-90, 97, 99, 154-56.

386. See supra pp. 152-53.

387. See *infra* pp. 194-95, 258-59.

388. See supra pp. 91-92.

389. See *infra* pp. 194-95, 258-59.

390. See supra p. 160 (discussing the definition of this factor).

391. In a small number of states and years, there were no homicides of African-Americans. In those instances, the value in the denominator of our calculation of this variable (white homicide rate ÷ black homicide rate) is 0. Because it is inappropriate to divide a value by 0, and because there is no measurable racial disparity in homicide victimization rates in those particular states and years, we recorded a value of 0 in those instances. Because effect-size associated with those few instances is difficult to interpret, we omit them in these graphs. Doing so is also conservative, because the reversal rates associated with those values are very low and tend to exaggerate the degree of change in predicted reversal rates across the spectrum of states and years in the study. We chose to omit them to give a more conservative estimate of the size of the impact of this variable.

392. See supra pp. 166-68.

393. See supra pp. 20-21, 88-89, 172.

394. See supra pp. 173-74.

395. See supra pp. 166-67, 168-69.

396. See supra p. 168-69 & n.367; *infra* Appendix E (defining FBI Index crimes).

397. “When life is at hazard in a trial, it sensationalises the whole thing almost unwittingly; the effect . . . [is] very bad.” Felix Frankfurter, *Of Law and Men* 81 (1956). See, e.g., William J. Bowers, *The Pervasiveness of Arbitrariness and Discrimination under Post-Furman Capital Statutes*, 74 *U. Crim. L. & Criminology* 1067, 1076-77 (1983); Stephen B. Bright & Patrick J. Keenan, *Judges and the Politics of Death: Deciding Between the Bill of Rights and the Next Election in Capital Cases*, 75 *B.U. L. Rev.* 759 (1995); Samuel R. Gross, *Lost Lives: Miscarriages of Justice in Capital Cases*, 61 *Law & Contemp. Probs.*, Autumn 1998, at 125; Norman Lefstein, *Reform of Defense Representation in Capital Cases: The Indiana Experience and its Implications for the Nation*, 29 *Ind. L. Rev.* 495, 511-12 (1996); Tina Roseberg, *Deadliest D.A.*, *N.Y. Times*, July 16, 1995, (Magazine), at 21, 23-24; Richard Willing & Gary Fields, *Geography of the Death Penalty*, *USA Today*, Dec. 20, 1999, at 1A (quoting Florida prosecutor Harry Shorestein); Eric Zorn, *Daley’s Oversight of Prosecutors Didn’t Do Justice to the Job*, *Chi. Trib.*, Nov. 16, 1999, at N1; Symposium, *Politics and the Death Penalty: Can Rational Discourse and Due Process Survive the Perceive Political Pressure*, 21 *Fordham Urb. L.J.* 239 (1994).

398. See supra pp. 171-73; infra Appendix E.

399. See supra pp. 171-73.

400. See supra pp. 20-21, 88-89, 172, 183.

401. See supra pp. 20-21, 88-89, 172, 183.

402. See supra pp. 20-21, 91-93 & Figure 10, 140-42, 155, 177.

403. See supra pp. 17-18 & nn.101, 103; infra Appendix C, pp. C-1 to C-2.

404. See infra note 406.

405. The fit inquiry described supra note 311 generates the following results in regard to Analyses 3A, 3B, 4A-4C:

Analysis no.	Number of explanatory factors added to baseline analysis	Improvement in fit	Significant at what level
<u>3A</u>	10	25	<.01
<u>3B</u>	7	37	<.01
<u>4A</u>	9	28	<.01
<u>4B</u>	9	26	<.01
<u>4C</u>	7	42	<.01

406. The table below indicates the amount of unexplained state-to-state and year-to-year variance in the Analysis 3 and 4 baseline analyses, and in the analyses of more specific explanatory factors.

Analyses 3 and 4: Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better; significance levels are in parentheses)

	<u>variance from state to state</u>	<u>variance from year to year</u>
<u>Analysis 3</u>		
<i>Baseline Analysis</i>	.52 (.002)	.064 (.02)
<u>Analysis 3A</u>	.32 (.01)	.003 (NS)
<u>Analysis 3B</u>	.37 (.004)	.007 (NS)
<u>Analysis 4</u>		
<i>Baseline Analysis</i>	.16 (.002)	.017 (.03)
<u>Analysis 4A</u>	.087 (.01)	0 (NS)
<u>Analysis 4B</u>	.089 (.01)	0 (NS)
<u>Analysis 4C</u>	.11 (.004)	.0003 (NS)

407. In all five analyses, $p < .0001$.

408. As is discussed above, one measure of effect size is the effect-size coefficient for each explanatory condition in each analysis (coded “newestimate” in our detailed results in Appendix G). See supra pp. 114-17 & nn.282, 285; pp. 171-73. In binomial regression studies, each unit increase in a given explanatory condition is expected to change the odds of reversal rates by a factor of x , with x being the amount of the effect-size estimate. In Poisson regression studies, each unit increase in a given explanatory condition is expected to change the reversal rate by a factor of x , with x being the amount of the effect-size estimate. In both types of studies, effect-size estimates less than 1 mean that reversal rates are expected to decline with each unit increase in the explanatory condition. The larger the distance between 1 and the effect-size estimate, the greater is the predicted decline in reversal rates. As the table below reveals, the predicted decrease in reversal rates for each additional death verdict awaiting review is considerably greater (because the distance between 1 and the estimate is greater) in Analyses 1 and 2 than in Analyses 3 and 4.

Effect Size of Backlogged Death Verdicts

	Effect-size estimate
Analyses of Three Review Stages	
Binomial <u>Analysis 1A</u>	.82
Binomial <u>Analysis 1B</u>	.81
Poisson <u>Analysis 2A</u>	.89
Poisson <u>Analysis 2B</u>	.90
Direct Appeal Stage Analyses	
Binomial <u>Analysis 3A</u>	.92
Binomial <u>Analysis 3B</u>	.92
Poisson <u>Analysis 4A</u>	.95
Poisson <u>Analysis 4B</u>	.95
Poisson <u>Analysis 4C</u>	.96

The same point—that effect size for capital backlogs is smaller in direct appeal Analyses 3 and 4 than in Analyses 1 and 2 of all three review stages—is made by comparing the effect-size graphs for this factor in Analyses 3 and 4, see Figures 31A and 31B, p. 201 below, to effect-size graphs for this factor in Analyses 1 and 2, see Figures 22A-22D, at p. 175 above.

409. See supra pp. 89-91, 99, 140-42 & Table 4, 154-56, 177. Analysis 10 below is also a reliable measure of the effect of time trend.

410. In regard to fit, see supra note 405. In regard to unexplained variance, see supra note 406. The table below compares the baseline analyses in Analyses 3 and 4 to the best analyses of specific factors, revealing not only the switch in the direction of the relationship between the passage of time and reversal rates from negative to positive, but also how much stronger the relationship is in the analyses of specific factors. As is discussed in more detail supra note 328, higher t values (irrespective of the sign), effect sizes further from 1 (irrespective of whether they are lower than 1 or higher than 1), and smaller significance values (*i.e.*, a lower probability that the relationship appears by chance) indicate a stronger relationship.

Relationship of the Passage of Time and Capital Error Rates
Analyses 3 and 4

	t Value	Effect Size	Significance
<u>Analysis 3</u> (binomial):			
Baseline Analysis	-3.67	.95	.0003
<u>Analysis 3A</u>	+6.42	1.18	< .0001
<u>Analysis 3B</u>	+6.10	1.17	< .0001
<u>Analysis 4</u> (Poisson):			
Baseline Analysis	-3.62	.97	.0003
<u>Analysis 4A</u>	+6.37	1.09	< .0001
<u>Analysis 4B</u>	+6.39	1.09	< .0001
<u>Analysis 4C</u>	+6.33	1.09	< .0001

411. For Analyses 3B and 4A-4C, $.02 \leq p \leq .05$.

412. For Analyses 3A, $p = .07$.

413. Analysis 3A, $p = .04$; Analysis 3B, $p = .067$; Analysis 4A, $p = .02$; Analysis 4B, $p = .03$; Analysis 4C, $p = .057$.

414. $p = .12$, $.07$ and $.09$. Effect size cannot be reliably calculated for this or other interaction effects.

415. $p = .16$ (Analysis 3B), $.12$ (Analysis 4C).

416. Analysis 3A is the only one in which a second measure of judicial politics appears. This measure (coded “pajid”) measures the “ideology” of state judges based on how they are selected and their political party. This factor was rarely significant, and its occasional significance disappeared when other factors (including our own measure of political pressure) were tested. Characteristically, the factor was not significant in Analysis 3A, but it did modestly improve fit and decrease unexplained variance. Its direction was positive, suggesting that death verdicts imposed by more conservative judges are more likely to be reversed—but, as noted, the effect was not significant.

417. We included this factor in Analysis 3A ($p = .02$), Analysis 4A ($p = .007$) and Analysis 4B ($p = .008$).

418. This factor is coded “dir_ext” in the detailed results in Appendix G.

419. The minimum annual direct expenditure on state courts per resident in the relevant states and years was about \$.02 in Arkansas in 1974; the maximum was \$5.98 in North Carolina in 1994; and the average was \$2.05.

420. During the years studied, the 34 study states spent from \$.02 per resident on their courts (in Arkansas in 1974) to \$5.98 (in North Carolina in 1994), with the average expenditure being \$1.73.

421. See supra pp. 17-18 & nn.101, 103; infra Appendix C, pp. C-1 to C-2 (explaining why state post-conviction reversal rates must be studied in this way). Poisson analysis is used because the rates studied in Analysis 5 are reversals as a proportion of all verdicts available for review, which include many cases that got stuck in the review process and were not finally reviewed in the study period. The measure artificially deflates reversal rates, therefore—in contrast to Analyses 3 and 4, which examine reversals as a proportion of finally reviewed verdicts and provide a more realistic picture of error rates. See supra pp. 20-21, 88-89, 172, 183, 194-95. In addition, the reversal rates we estimate for the second, state post-conviction review stage appear are lower on average than the reversals we count at the first, state direct appeal and second, federal habeas, review stage. See supra pp. 9, 17-18 & n.103.

422. See supra note 103.

423. See infra note 424.

424. Fit for the baseline was 1510. For Analyses 5A and 5B, it was 1507 and 1516. Neither Analysis 5A's improvement of 3 points nor Analysis 5B's decline of 6 points is statistically significant. As for unexplained variance, see the table below:

Analysis 5: Coefficient Indicating Amount of Unexplained Variance (less is better)
(significance levels are parentheses)

	<u>variance from state to state</u>	<u>variance from year to year</u>
<u>Analysis 5</u>		
<i>Baseline Analysis</i>	.46 (.01)	.140 (.07)
<u>Analysis 5A</u>	.38 (.02)	.074 (NS)
<u>Analysis 5B</u>	.36 (.03)	.167 (.06)

On effect size, see *infra* p. 214-15.

425. See *supra* pp. 198-99.

426. See *supra* pp. 89-90, 97, 99, 153-54. This factor is coded “pcbl” in the detailed results in Appendix G. Because we were unable to count state post-conviction affirmances for the reasons discussed *supra* pp. 17-18; *infra* Appendix C, pp. C-1 to C-2, our measure of backlogs in Analysis 5 (verdicts available for review minus verdicts reviewed and reversed) is composed not only of verdicts as to which review was delayed but also verdicts that were reviewed and approved because no reversible error was found. To this latter extent, the factor’s significant negative relationship to reversal rates is obvious and uninteresting because the factor is essentially the same as the condition being studied—whether verdicts were reversed or not reversed.

427. See *supra* pp. pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95.

428. See *supra* pp. 59-61 & Figures 3A and 3B.

429. This factor is coded “wvrtst” in the detailed results in Appendix G.

430. $p = .09$.

431. This analysis is based on the numerical effect-size estimate for this unlogged factor of 1.21. For an explanation of how effect size is calculated using these kinds of estimates, see *supra* pp. 114-17 & nn.282, 285.

432. This analysis is based on the numerical effect-size estimate for this unlogged factor of 8.53, and on the fact that values for this factor in particular states and years range from .05 to .54. For an explanation of how effect size is calculated using these kinds of estimates, see *supra* pp. 114-17 & nn.282, 285.

433. The numerical effect-size estimate for this logged factor in Analysis 5A is 2.45, and in Analysis 5B is 2.07. For an explanation of how effect size is calculated using these kinds of estimates, see *supra* pp. 114-17 & nn.282, 285.

434. This kind of analysis is suited to distributions of rates to be explained that include many at the high as well as the low end of the range from 0 to 100.

435. The baseline fit was 628—the same for Analysis 6A, and 2 points less than (but, statistically speaking, the same as) for Analysis 6B.

436. Analysis 6: Coefficient Indicating Amount of Unexplained Variance
(less is better; significance levels are in parentheses)

	<u>variance from state to state</u>	<u>variance from year to year</u>
<u>Analysis 6</u>		
<i>Baseline Analysis</i>	.49 (.04)	.08 (NS)
<u>Analysis 6A</u>	0 (NS)	0 (NS)
<u>Analysis 6B</u>	0 (NS)	0 (NS)

On effect size, see *infra* Figures 40A, 40B, 40C-1, 40C-2 and 40D, pp. 220-21, below.

437. For discussion of that non-error-related downward effect of the passage of time on reversal rates in Analyses 1, 2 and 5, see *supra* pp. 88-91, 99, 140-42, 152-53, 154-56, 177, 194-95, 213.

438. See *supra* pp. 91-93, 140-42, 155, 177, 193. As of the end of the study period, the average time from imposition to final federal habeas decision was just over 11 years for affirmed death verdicts and 12.5 or more years for reversed death verdicts. Despite this difference, nearly all death verdicts imposed early in the 23-year study period—whether flawed or not—were finally reviewed on federal habeas by the end of that study period (assuming the verdicts were not reversed at an earlier review stage). But as death verdicts became more recent, more of them were likely to have been in federal habeas proceedings towards the end of the study period. Among those verdicts, the shorter period needed to finally review verdicts without discoverable flaws, and the longer period needed to review verdicts with those flaws, meant that a disproportionately large number of unflawed verdicts imposed relatively late in the study period were finally reviewed and counted in our study, while a disproportionately large number of flawed verdicts imposed relatively late in the period were not finally reviewed by the end of the study period or, thus, counted in the study. This systematic undercounting of flawed cases imposed late in the study period would lead the passage of time to be associated with decreasing reversal rates because of discrepancy in the time needed to review flawed and unflawed verdicts and not because fewer flawed verdicts were imposed in later years.

439. $p = .0002$ (Analysis 6A); $p = .0007$ (Analysis 6B).

440. For the size of this at least partly non-error-related effect, see Figure 40A (Analysis 6A), p. 220 below.

441. Federal habeas is the only stage at which this factor was important.

442. See *supra* pp. 16, 37-39, 65 n.208.

443. See *supra* pp. 198-99, 212.

444. See *supra* pp. 169-70 & n.370 (discussing the two political pressure indexes).

445. Effect size can also be compared based on their numeric effect-size estimates (coded “newestimates” in Appendix G). For a discussion of how to interpret these estimates, see supra pp. 114-17 & nn.282, 285. Effect size is compared by comparing the distance between the estimate and 1. The greater that distance, the greater the effect size. For the political pressure indexes in Analyses 1, 3 and 6, that comparison is as follows:

Effect Size for Political Pressure Indexes:
Binomial Analyses 1, 3 and 6

	Effect-size estimate
Binomial <u>Analysis 1</u> (3 review stages combined)	
<u>Analysis 1A</u> (first political pressure index)	1.25
<u>Analysis 1B</u> (second political pressure index)	1.30
Binomial <u>Analysis 3</u> (direct appeal stage)	
<u>Analysis 3A</u> (second political pressure index)	1.19
<u>Analysis 3B</u> (second political pressure index)	1.15
Binomial <u>Analysis 6</u> (federal habeas stage)	
<u>Analysis 6A</u> (first political pressure index)	1.44
<u>Analysis 6B</u> (second political pressure index)	1.63

446. $p = .01$ (Analysis 6A); $p = .0009$ (Analysis 6B).

447. See supra pp. 37-38, 65 & n.209.

448. See supra pp. 194.

449. The numbering of analyses jumps from 6 to 14 because we conducted all our comparisons of the states' aggregate 23-year experiences last (Analyses 14-17), after first completing the state Analyses 1-6 and county-state Analyses 7-13 in which state and time were given more conventional treatment.

450. Analyses 14 and 15: Fit, and Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better)

	Fit (amount and signifi- cance of improvement)	Coefficient indicating amount of unexplained state-to-state variance (significance level in parentheses)
<u>Analysis 14</u>		
<i>Baseline Analysis</i>	1952	.40 (.002)
<u>Analysis 14A</u>	1806 (-146, HS)	.12 (.03)
<u>Analysis 14B</u>	1802 (-150, HS)	.10 (.04)
<u>Analysis 15</u>		
<i>Baseline Analysis</i>	1409	.15 (.002)
<u>Analysis 15A</u>	1111 (-298, HS)	.026 (.04)
<u>Analysis 15B</u>	1113 (-296, HS)	.027 (.03)

451. $p < .0001$.

452. See supra pp. 89-90, 97, 99, 152-53, 173-74, 212.

453. See supra pp. 174, 176.

454. $p < .0001$ (Analyses 14A, 15A), .002 (Analysis 14B), .02 (Analysis 15B). See infra p. 224-25 (linking the higher significance levels in Analyses 14A and 15A, compared to Analyses 14B and 15B, to the inclusion of homicide rates as a factor in the latter two analyses but not the former two).

455. See supra note 454.

456. This factor is coded "hrst" in the detailed results in Appendix G. In Analysis 14B, $p = .11$; in Analysis 15B, $p = .002$.

457. Because there is no homicide rate factor in Analysis 15A, there is no Figure 41E-1.

458. Because homicide rates vary by year with some consistency across states, Analyses 14 and 15's removal of the assumption, programmed into all earlier analyses, that reversal rates for given years are more like each other than ones for other years, may also account for the importance of homicide rates in these, but not prior, analyses.

459. $R = .56$ ($p < .0001$).

460. See supra note 454.

461. See supra pp. 165-66.

462. $p = .08$ (Analysis 14A), .095 (Analysis 14B).

463. See supra pp. 224-25.

464. As with percent black, this factor and homicide rates are correlated. $R = .34$ ($p < .0001$). States with high homicide rates tend to be ones in which the homicide risk to whites approaches or surpasses that to blacks.

465. $p = .10$ (Analysis 14A), $.11$ (Analysis 14B), $.02$ (Analysis 15A), $.06$ (Analysis 15B).

466. See supra p. 167-68.

467. Cf. supra pp. 198-99, 212, 217-18, 227.

468. See supra pp. 224-25 & n.458.

469. This finding does not suggest that time be removed from all of our analyses. As we note above, time is included in many analyses to facilitate simultaneous comparisons of states that began imposing the death penalty under constitutional statutes at different times, starting in 1973. See supra pp. 87-88.

470. See supra p. 146-91.

471. Here, we look to see if one or more “best” sets of explanatory factors within each Analysis 1-6, 14 and 15 satisfied the relevant diagnostic criterion.

472. Unless noted, the baseline analysis left both state-to-state and the year-to-year differences to be explained, and the differences were significant at the .05 level or better. Analyses 14 and 15 only examine state-to-state differences.

473. Year to year variance was significant at the .07 level.

474. State-to-state variance was significant, but year-to-year variance was not.

475. Percent of Significant Factors* in Each Analysis That Were Significant in, and Have Same Relationship to Reversal Rates as, Analyses of One or Two Other Review Stages

	One Other Review Stage	Two Other Review Stages
<u>Analysis 1</u>	89%	67%
<u>Analysis 2</u>	89%	67%
<u>Analysis 3</u>	78% or 89% ⁺	56% or 63% ⁺
<u>Analysis 4</u>	78% or 89% ⁺	56% or 63% ⁺
<u>Analysis 5</u>	100%	100%
<u>Analysis 6</u>	50%	50%
<u>Analysis 14</u>	89%	67%
<u>Analysis 15</u>	89%	67%

* For purpose of this diagnostic test, we treat different measures of capital-cases backlogs applicable to different stages of review as the same factor. We exclude interaction effects from this analysis given the difficulty encountered in interpreting them.

⁺ For reasons discussed supra notes 88-91, 99, 140-41, 149-55, 175, 190, 201. Analyses 3 and 4 are the only ones that are completely reliable measures of the effect of time on error rates, and the only ones in which the passage of time is not only significantly but positively associated with reversal rates. The fact that no other analysis reveals that same relationship arguably should not be held against Analyses 3 and 4. If that factor is omitted, the two analyses' rate of congruence with analyses of one or more other review stages rises from 78% to 89%, and their rate of congruence with analyses of two or more other review stages rises from 56% to 63%.

476. See supra pp. 102-03, 222 (distinguishing analyses that divide all states and years into two different grids—one for all years in the same state, and all states in the same year—and those that divide states and years only into one grid comparing states in different years).

The two percentages presented for Analysis 5

477. The comparable percent for Analysis 6 is 33%. For purpose of this diagnostic test, we treat different measures of capital-cases backlogs applicable to different stages of review as the same factor. We exclude interaction effects from this analysis. The 80% score for Analysis 5 rises to 100% if one treats the “white homicide victimization rate” factor there as sufficiently analogous to the “white-vs.-black homicide victimization rate” factor in Analyses 14 and 15 to qualify as a match.

478. During the study period, there were 2349 reversals, 1852 (79%) of which occurred at the direct appeal stage.

479. See supra pp. 88-91, 99, 140-42, 152-53, 154-56, 177, 194-95, 213.

480. More particularly, the greater the *reviewing* judges' insulation from local political pressures, the stronger the positive relationship between political pressure on state judges and capital error rates. See supra pp. 198-99, 212, 217-18, 227.

481. See supra pp. 218-19.

482.

Notes to Table 7

[†] All significant factors are positively associated with reversal rates (reversal rates tend to increase when the amount or intensity of the explanatory factor increases), unless the factor is preceded by a negative sign (“-”), in which case the factor is negatively associated with reversal rates (reversal rates tend to decrease when the amount or intensity of the condition increases).

[#] Analysis 1R is treated as a variant of Analysis 1, rather than as a separate analysis. Explanatory factors -pctbdiffnew, -dswvrt, and wvdsst only appear in Analysis 1R. Because Analyses 14 and 15 explain variation in reversal rates measured as a composite of each state’s 23-year experience, without regard to particular years in the study period, see supra pp. 102-03, 222, time is excluded as a possible explanatory value in these analyses. As a result, time could only be tested in six of the eight analyzes examined in this table.

[&] The relative homicide rates for whites and blacks just failed to reach significance in Analysis 14 ($p = .08$); the interaction between that factor and the proportion the population that is black also just failed to reach significance in Analyses 4 and 14 ($p = .08$); and the indexes of political pressure were just above significance in Analyses 3 and 4 ($p = .07$, $.06$).

^{**} Explanatory factors that were significant in analyses of all three review stages in which all “state-years” were grouped for analysis both by state and by year (Analyses 1 and 2), and in analyses of all three review stages in which “state-years” were grouped only by states (Analyses 14 and 15).

[^] In Analyses 1 and 2, the relationship of the passage of time to reversal rates is an unreliable indication of the changing quality of death verdicts over time. This is because reversal rates in those analyses are the number of death verdicts imposed in a given year that were reversed, as a proportion of all death verdicts imposed that year, whether or not they were reviewed. Because it takes 2 to 5 years, on average, for final review to occur at even just the first (direct appeal) stage of review, and many more years for later stages of review to occur, there are almost no death verdicts imposed in 1994 and 1995 that secured any review as of the end of the study period in 1995, and only small proportions of death verdicts imposed in 1991, 1992 and 1993 that were reviewed even at the first stage as of 1995. More generally, the later a death verdict was imposed, the less likely it is that the verdict was finally reviewed at any review stage, and especially at all three review stages, by the analysis’s cut-off date of 1995. Death verdicts imposed in later years thus are less likely than those imposed in earlier years to have been reviewed as of the end of the study period, imposing a downward trend on death verdicts over time. That decrease, however, is a function of the successively smaller number of death verdicts imposed in later years that were reviewed, and does not indicate that later verdicts were less likely to be flawed. As a result, any decline in reversal rates over time is at least in part, and may be entirely, a function of the number of unfinished appeals, rather than a function of changes in the amount of serious error. See supra pp. 88-91, 99, 140-42 & Table 4, 152-53, 154-56, 177, 194-95, 213, 216-17. Likewise, in Analysis 5, the measured effect of the passage of time is unreliable because reversal rates there are calculated as the number of state post-conviction reversals of death verdicts imposed in a given year as a proportion of the number of verdicts imposed in that year that were available for state post-conviction review, whether or not they were actually reviewed. The result is the same as is described in the previous paragraph: The rate of reversals as a proportion of all verdicts available for review, whether or not they actually were reviewed, is subject to a downward trend that is a consequence of the time needed to complete state post-conviction review and is not an indication of less error over time.

Analyses 3, 4 and 6 analyze reversals as a proportion of the number of *reviewed* death verdicts. In these analyses, therefore, low rates of review do not automatically lead to low rates of reversal irrespective of the quality or error-proneness of death verdicts. These analyses thus are more reliable measures of the effect of the passage of time on rates of serious error than Analyses 1, 2 and 5. Analysis 6 has a different source of unreliability, however. That analysis examines the federal habeas stage of review, where reversals take one to two years longer on average than affirmances to be ordered. As a result, the pool of death verdicts imposed during the study period that were not finally reviewed on

habeas during that period includes a disproportionately large number of the flawed verdicts imposed in a given year, while the pool of verdicts that were reviewed and whose review results were counted in our study includes a disproportionately large number of the unflawed verdicts imposed in any such year. See *supra* pp. 20-21, 91-93 & Figure 10, 140-42, 155, 177, 193, 216-17 & n.438.

‡ This factor had low effect size.

483. For this purpose, the stages considered were state direct appeal, state post-conviction, federal habeas, and the three combined.

484. In six of the analyses, the measure of capital backlogs was the number of capital verdicts awaiting review at all three stages; in our state post-conviction analysis (Analysis 5) the measure was the number of capital verdicts awaiting review at the state post-conviction stage.

485. In Analysis 5, the homicide rate among whites, by itself, was significant. In that analysis, the white homicide victimization rate relative to the black homicide victimization rate fell just above the .05 significance level.

486. In this one instance, we include a factor where the three-fourths rule is satisfied only by considering results that were significant at above the .05 level. We do so because this factor, which was significant at less than the .05 level in 5 analyses, fell just barely above significance in 2 additional analyses, meaning the factor is not qualitatively different from our very strongest explanations, which were significant in 7 of 8 analyses. In addition, our interpretation of this factor is based on its having only a moderate or intermediate effect at the state direct appeal stage—the stage covered by the two analyses in which the factor fell just above the .05 level—which assures that we are interpreting the factor based on its actual, intermediate outcome in those analyses, rather than ascribing more than they show. See *supra* pp. 198-99, 212, 217-18, 227, 236; *infra* p. 336.

487. In a fourth and a fifth analysis (Analyses 4, 14), this factor fell just above the .05 significance level.

488. See *supra* pp. 224-26.

489. See the effect-size estimates (*i.e.*, “newestimates”) in the detailed results in Appendix G, and by an array of effect-size graphs displayed above.

490. See *supra* pp. 198-99, 212, 217-18, 227, 236.

491. See *supra* pp. 218-19, 236.

492. Only six of the eight analyses undertook to measure the effect of the passage of time. The other 2, by design, rendered the passage of time irrelevant by averaging each state’s reversal rates and explanatory conditions over the entire study period. Those six analyses all used the approach to weighting the states.

493. See *supra* pp. 88-91, 99, 140-42 & Table 4, 152-53, 154-56, 177, 194-95, 213, 216-17, 235-36.

494. See *supra* p. 235-36 & n.478.

495. See *supra* pp. 52-58 & Figures 2A-2D.

496. See supra pp. 194-96.

497. Of the 2349 total reversals, 240 were at the federal habeas stage.

498. See supra pp. 20-21, 91-93 & Figure 10, 140-42, 155, 177, 193, 216-17 & n.438, 439.

499. See supra pp. 65, 67 & Figure 5.

500. See, e.g., Supreme Court decisions cited supra notes 122, 125.

501. See supra pp. 133-34.

502. See supra p. 133.

503. To help readers account for this point, we indicate the number of counties in each state, at the top of each state's bar in Figure 42A.

504. Figures 42A and 42B are based on 1004 capital counties in 34 states. Our regression analyses study 1002 counties. The difference arises because the best available county data on factors such as homicide rates, described supra p. 170 & n.371, aggregates a two small independent cities into adjoining counties, forcing us to do the same in order to make use of the data.

505. This point is developed further at pp. 268-69, 272-82 below.

506. $3054 / 967 = 3.16$.

507. Our earlier analyses leave little reason to think that binomial and Poisson analyses generate different outcomes in important respects. Because [Analysis 7](#) is merely exploratory—aiming to maximize the number of factors to consider in later analyses, rather than to exclude all but the strongest effects—we concluded that a single regression technique was sufficient. We used Poisson analysis because its coefficients are easier to interpret.

508. Some of the county-level data analyzed in our county studies were supplied by Professor Messner and his colleagues, for which we are grateful. See supra p. 170 & n.371; supra note 504.

509. For most counties, homicide is a rare event that does not occur in most years. In order to account for this phenomenon, we calculated death-sentencing rates somewhat differently for counties than for states, by averaging the number of homicides in the year each death verdict was imposed and in the preceding year. In the event that there were no homicides in the death-sentencing and preceding year, we averaged the number of homicides in the death-sentencing year and the two prior years.

510. In the detailed results in Appendix G, these factors are coded, respectively, as “year-n,” “lcntydor2,” “pctblk,” “hr2,” “wbtr” and “wvrt,” “ps,” and “fp.”

511. As noted above, see supra pp. 89-90, 97, 99, 153-54, 173-74, 212, 223, backlogs of capital cases awaiting review automatically lower rates of court review. That, in turn, automatically lowers reversal rates where they are calculated as the proportion of imposed death verdicts that are reversed, not the proportion of reviewed death verdicts that are reversed. This factor is coded “bltot.”

512. Detailed results for the county (and all other) analyses are reported in Appendix G.

513. Analysis 7: Fit, and Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better; significance levels are in parentheses)

	Fit	Coefficient indicating amount of unexplained variance	
		County-to-county variance	Year-to-year variance
<u>Analysis 7</u>			
<i>Baseline Analysis</i>	9373	.17 (.HS)	.003 (HS)
<u>Analysis 7A</u>	9434	.11 (.0007)	.001 (.0003)

514. See supra pp. 88-91, 99, 140-42 & Table 4, 152-53, 154-56, 177, 194-95, 213, 216-17, 235-36.

515. See supra pp. 89-90, 97, 99, 153-54, 173-74, 212, 223.

516. See supra pp. 224-26.

517. See infra pp. 266, 285, 286-87, 336, 363.

518. See supra pp. 250-51.

519. See supra pp. 88-91, 99, 140-42, 152-53, 154-56, 177, 194-95, 213, 216-17, 235-36.

520. Analyses 8-10: Fit, and Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better; significance levels are in parentheses)

	Fit	Coefficient for random intercept indicating amount of unexplained variance
<u>Analysis 8</u>		
<i>Baseline Analysis</i>	13045	.53 (HS)
<u>Analysis 8A</u>	13257	0 (NS)
<u>Analysis 9</u>		
<i>Baseline Analysis</i>	9331	.14 (HS)
<u>Analysis 9A</u>	9497	0 (NS)
<u>Analysis 10</u>		
<i>Baseline Analysis</i>	10129	.24 (HS)
<u>Analysis 10A</u>	10235	0 (NS)

521. In all three analyses, $p < .0001$.

522. See supra p. 194.

523. $p < .0001$.

524. For the difference between the two indexes, see supra pp. 169-70 & n.370.

525. $p < .0001$ (Analyses 8A, 10A); $p = .0017$ (Analysis 9A).

526. $p = .32$ (Analysis 8A); $p = .0009$ (Analysis 9A); $p < .0001$ (Analysis 10A). On the analogous county-level factor, see infra p. 266 & n.533.

527. The latter result is anomalous, given the tendency in other analyses of homicide rates to be either non-significant or positively associated with reversal rates. See supra pp. 224-26, 253; infra p. 280.

528. See supra pp. 88-91, 99, 140-42 & Table 4, 152-53, 154-56, 177, 194-95, 213, 216-17, 235-36, 255-56.

529. $p = .06$ (Analysis 8A), $.38$ (Analysis 9A).

530. As is noted above, see pp. 252 & n.511, Analysis 7 used a county-level factor—the number of verdicts the county imposed in each year that had not been fully reviewed by the end of the study period—as a control for the effect of delay. In county-state Analyses 8-10, that same purpose was served by including the same factor as a *state-level* predictor, which is more appropriate because the factor measures the effect of activities in state supreme courts and other courts with statewide, as opposed to county-wide, jurisdiction.

531. See supra pp. 258-59, 264 & Figures 43P, 43Q.

532. $p = .003$ (Analysis 8A), $.04$ (Analysis 9A), $.009$ (Analysis 10A).

533. $p = .008$ (Analysis 8A); $p < .0001$ (Analysis 9A); $p = .07$ (Analysis 10A). In regard to the analogous state-level factor in Analysis 8, see supra p. 258 & n.526.

534. See supra pp. 224-26. These two racial conditions were significantly related to differences in county reversal rates when measured at the state level but not when measured at the county level.

535. See supra pp. 246-50.

536. The coefficients for unexplained *state-level* variance and their significance levels in Analyses 11 and 12 are derived from the baseline inquiries for Analyses 1 and 2. Those coefficients are displayed both supra p. 151, Table 5, and infra note 540. The nonrandom variance in state reversal rates that is left to be explained by the baseline analyses of state, year and time trend is significant at the .0006 level in both Analyses 11 and 12.

537. See infra note 540.

538. For evidence that this is the case, see supra pp. 246-50; infra pp. 272-78, 279-82, 286-87.

539. See supra pp. 246-50.

540. Analyses 11 and 12: Fit, and Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better; significance levels are in parentheses)

	Fit	Coefficient indicating amount of unexplained variance	
		State-to-state or county-to county variance	Year-to-year variance
<u>Analysis 11</u>			
State-Level Analysis:			
<i>Baseline Analysis</i>	1813	.52 (.0006)	.29 (.01)
<u>Analysis11A</u>	1714	.15 (.01)	.15 (.03)
County-Level Analysis:			
<i>Baseline Analysis</i>	13452	.072 (.015)	N/A
<u>Analysis11A</u>	13745	.064 (.021)	N/A
<u>Analysis 12</u>			
State-Level Analysis:			
<i>Baseline Analysis</i>	1260	.14 (.0006)	.30 (.009)
<u>Analysis12A</u>	1109	.03 (.02)	.22 (.01)
County-Level Analysis:			
<i>Baseline Analysis</i>	9711	.007 (.09)	N/A
<u>Analysis12A</u>	9790	.004 (NS, .17)	N/A

541. See supra pp. 241-45.

542. For evidence that this is the case, see supra pp. 246-50; infra pp. 279-82, 286-87.

543. Effect size is indicated by the “newestimate” coefficients in the detailed results in Appendix G. The interpretation of those estimates is discussed supra pp. 113-15 & nn.282, 285.

544. It does include a random intercept to measure the amount of unexplained variance.

545. Analysis 13: Fit, and Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better; significance levels are in parentheses)

	Fit	Coefficient for random intercept indicating amount of unexplained county-to-county variance
<u>Analysis 13</u>		
<i>Baseline Analysis</i>	9353	.12 (HS)
<u>Analysis 13A</u>	9449	.03 (.002)
<u>Analysis 13B</u>	9575	.01 (.05)

546. $p = .52$ (Analysis 13A), $p = .25$ (Analysis 13B).
547. See supra pp. 88-91, 99, 140-42 & Table 4, 152-53, 154-56, 177, 194-95, 213, 216-17, 235-36, 255-56.
548. See supra pp. 258-59. In fact, although non-significant, the relationship between reversal rates and the passage of time in Analyses 13A and 13B is *positive*, meaning that some upward force on reversal rates over time (after accounting for other factors) has entirely neutralized the downward pull on reversal rates from the effect of unfinished appeals.
549. $p < .0001$ (Analyses 13A and 13B).
550. $p < .0001$ (Analyses 13A and 13B). Effect size is similar to that for the same explanatory factor in Poisson Analysis 2. See supra pp. 183-84 & Figures 27C, 27D.
551. $p < .0001$ (Analyses 13A and 13B). Effect size for this factor is slightly larger in Analysis 13 than in Analysis 2. See supra pp. 179-80 & Figures 25C, 25D.
552. $p < .0001$ (Analyses 13A and 13B). Effect size is about 15 to 25% larger for this factor in Analysis 13 than for the same factor in Analysis 2. See supra pp. 181-82 & Figures 26C, 26D.
553. $p < .0001$ (Analyses 13A and 13B). Effect size is difficult to interpret for interaction effects.
554. $p < .0001$ (Analyses 13A and 13B, both using the second index of political pressure). Effect size is about the same for this factor in Analysis 13 as in Analysis 2A. See supra pp. 187-88 & Figure 29C.
555. $p < .0001$ (Analyses 13A and 13B). Effect size is slightly larger than that for the same factor in Analysis 2. See supra pp. 185-86 & Figures 28C, 28D.
556. $p < .0001$ (Analyses 13A and 13B). Effect size for this factor is about the same in Analysis 13 as in Analysis 2. See supra pp. 189-90 & Figures 30C, 30D.
557. $p < .0001$ (Analysis 13B). Effect size remains negligible in Analysis 13, as in Analysis 2A. See supra pp. 175-76 & Figure 23C.
558. $p < .0001$ (Analysis 13B). Effect size is difficult to interpret for interaction effects.
559. $p = .05$ (Analysis 13A), $.47$ (Analysis 13B).
560. For state homicide rates (Analysis 13A), $p = .86$; for county homicide rates, $p = .13$.
561. See supra pp. 246-50, 268-69.

562. Analyses 16-17: Fit, and Coefficient Indicating Amount of Unexplained Variance
(on both measures, less is better; significance levels are in parentheses)

	Fit	Coefficient for random intercept indicating amount of unexplained state-to-state variance
<u>Analysis 16</u>		
<i>Baseline Analysis</i>	3661	.48 (.0003)
<u>Analysis 16A</u>	3651	.11 (.08)
<u>Analysis 17</u>		
<i>Baseline Analysis</i>	2534	.18 (.0002)
<u>Analysis 17A</u>	2529	.04 (.06)

Analyses 16 and 17 present more potential multi-colinearity questions than other studies, see *infra* Appendix F, and for that reason would not be a basis for any firm conclusions by themselves. The fact that their results are very similar to those of the other analyses, but that effect size is consistently smaller than in other analyses, suggests that multi-colinearity is deflating the estimates (dampening the clarity of the relationships) in these two studies.

563. $p < .0001$ (Analyses 16A, 17A). General court backlogs were tested as an explanatory factor in both analyses but were not significant.

564. $p = .02$ (Analysis 16A); $.04$ (Analysis 17A).

565. $p = .09$ (Analysis 16A), $.07$ (Analysis 17A).

566. $p = .01$ (Analysis 16A), $.006$ (Analysis 17A).

567. $p = .0002$ (Analysis 16A); $p < .0001$ (Analysis 17A). The interaction of this factor and the preceding, “percent black” factor was not significant in this model and was excluded from our best analyses.

568. $p = .0002$ (Analysis 16A), $.0008$ (Analysis 17A). Both analyses use the second political pressure index.

569. $p = .02$ (Analysis 16A), $.008$ (Analysis 17A).

570. $p = .01$ (Analysis 16A), $.02$ (Analysis 17A).

571. See *Furman v. Georgia*, 408 U.S. 238 (1972).

572. Florida, Georgia and Texas as Proportion of 34 Study State Totals

	FL	GA	TX	3 States	34-State Tot.	% of Tot.
No. of counties with ≥ 1 death verdict during study period	53	88	105	246	1002	25%
No. of death verdicts during study period	889	339	734	1962	5826	34%
No. of death verdicts reversed during study period	480	199	218	897	2349	38%
No. of death verdicts finally reviewed during study period	539	233	321	1093	2707	40%

Florida and Texas imposed more death verdicts than any other state during the study period. California was third (with 533), and Georgia was fourth. Because California got started several years after the other three states, however, and its verdicts experience such extended delays in the review process, it has many fewer court decisions than the other three states.

573. $p = .08$ (binomial), $.08$ (Poisson).

574. The situation remained the same when we attempted to maximize variance by dropping year as a random effect, removing time trend from the baseline analysis, and switching county from a random effect to a subject variable.

575. Analysis 18, Florida: Fit, and Coefficient Indicating Amount of Unexplained Variance (on both measures, less is better; significance levels are in parentheses)

	Fit	Coefficient indicating amount unexplained county-to-county variance
<u>Analysis 18: Florida, Poisson</u>		
<u>County and Year = Random Effects:</u>		
<i>Baseline Analysis</i>	849	.016 (.08)
<u>Analysis 18A</u>	809	.001 (NS)
<u>Analysis 18B</u>	822	.001 (NS)

576. See supra pp. 88-91, 99, 140-42, 152-53, 154-56, 177, 194-95, 213, 216-17, 235-36, 255-56. For this factor, $p = .0002$ (Analysis 18A), $.001$ (Analysis 18B).

577. Effect-size estimates are in the detailed results in Appendix G. See supra pp. 114-17 & nn.282, 285 (discussing the calculation and interpretation of effect-size estimates).

578. See supra pp. 89-90, 97, 99, 152-53, 173-74, 212, 223. For this factor, $p < .0001$.

579. $p = .0006$ (Analysis 18A), $<.0001$ (Analysis 18B).

580. $p < .0001$ for both analyses.

581. $p = .004$ (Analysis 18B). The information in note 575 above reveals a significant drop in fit as a result of including this factor—the only factor in Analysis 18B that is not in Analysis 18A.

582. See supra pp. 224-26, 266.

583. See, e.g., supra pp. 268-69, 272-78, 279-80.

584. See supra pp. 246-50.

585. See supra pp. 119-20.

586.

Notes to Table 8

* Although there are 10 county analyses, only 8—Analyses 8-13, 16 and 17—include both county and state factors. Analyses 7 and 18 include only county factors. Those studies are surveyed in Table 9, p. 285 below.

† Analyses 8-10, 13 explain variation among counties treated as entities nested within particular states (meaning the statistical analysis is programmed to assume that there are more similarities among counties within the same state than among counties from different states). Explanatory factors that were significant in these analyses are indicated by “*cnty-in-st.*” Analyses 11 and 12 use the entire set of state explanatory factors from our best state-only Analyses 1 and 2 to identify predicted reversal rates for all counties in each state, then test those predicted rates to see how closely associated they are with counties’ actual reversal rates. Explanatory factors that were significant in these analyses are indicated by “*predval.*” Analyses 16 and 17 explain variation among states, where each state is treated in effect as the composite of all of its counties, which in turn are treated as their aggregate values across all study years in which they had at least one death verdict. Explanatory factors that were significant in this study are indicated by “*st=comp/cntys.*” All but one of the analyses collected here explain variation in aggregate reversal rates at all three stages of review; the one exception is Analysis 10, which explains variation in reversal rates at only the direct appeal stage. Explanatory factors that were significant in that single-stage analysis are indicated by “*da.*”

^ In these analyses, the linear trend for time is unreliable because the base number (the denominator or offset) for reversals is the raw number of death verdicts imposed in each relevant year, not the number of verdicts imposed in that year that were actually reviewed. Because it takes two to five years, on average, for final review to occur at even just the first (direct appeal) stage of review (and even more years for later stages of review to occur), there will be almost no death verdict imposed in 1994 and 1995 that secured any review as of the end of the study in 1995, and only very small proportions of death verdicts imposed in 1991, 1992 and 1993 will have been reviewed as of 1995. See supra p. 142 & Table 4. For this reason, there will be a low number of reversals, and a low percentage of death verdicts imposed in those later years that were reversed—making it appear that the linear trend was toward lower numbers and rates of reversals in the later years and, so, over time. That lower number and rate of reversals for death verdicts imposed in the later years is, however, an artifact of the smaller number of death verdicts imposed in those years that were *reviewed*, rather than an indication that fewer of the verdicts that were reviewed were *reversed*. Because Analyses 16 and 17 explain variation in reversal rates that are aggregates of counties’ 23-year capital experiences, time is excluded as a possible explanatory value. The passage of time thus could be tested in only six of the eight analyses analyzed in this table.

+ See supra note 527.

587. See supra pp. 89-90, 97, 99, 153-54, 173-74, 212, 223.

588. See supra pp. 224-26, 266.

589. Notes to Table 9

* Analyses 8-13, and 16-18.

[†] Analysis 7 studies variation among counties, by year, as entities independent of states. Explanatory factors that were significant in this analysis are indicated by “*cnty.*” Analyses 8-10 and 13 explain variation among counties treated as entities nested within particular states (meaning the statistical analysis is programmed to expect more similarities among counties within the same state than among counties from different states). Explanatory factors that were significant in these analyses are indicated by “*cnty-in-st.*” Analyses 11 and 12 use the entire set of state explanatory factors from our best state-only Analyses 1 and 2 to identify predicted reversal rates for all counties in each state based on the number of death verdicts imposed by each county, then test those predicted rates to see how closely associated they are with the counties’ actual reversal rates. Explanatory factors that were significant in these analyses are indicated by “*predval.*” Analyses 16 and 17 explain variation among states, where each state is treated in effect as the composite of all of its counties, which in turn are treated as their aggregate values in all years in which they had at least one death verdict. Explanatory factors that were significant in this analysis are indicated by “*st=comp/cntys.*” Analysis 18 explains variation in capital reversal rates among Florida counties. Explanatory factors that were significant in this analysis are indicated by “*fl.*” All but one of the analyses collected here explain variation in aggregate reversal rates at all three stages of review. The one exception is Analysis 10, which explains variation in reversal rates at only the direct appeal stage. Explanatory factors that were significant in that single-stage analysis are indicated by “*da.*”

⁺ One factor treated as county-level influence in the two county-only analyses is in fact a state-level variable introduced at the county level for a particular statistical purpose. As is discussed above, in any analysis of all three review stages combined using proportions of imposed as opposed to reviewed death verdicts, the reversal rate is partly a function of the review rate: Lower review rates automatically lead to lower reversal rates. See supra pp. 89-90, 97, 99, 153-54, 173-74, 212, 223. Our analyses use the number of death verdicts awaiting review to winnow out the effect of delay. This explanation in fact operates at the state level, because it is the product of the action and inaction of the state’s unified appellate court system. In county-only Analyses 7 and 18, however, it is necessary to introduce a county-level analogue of that explanation to serve as a control for delay. That factor’s significance in Analyses 7 and 18 does not reveal a separate county-level factor, and simply reflects the importance of a state-level factor the state analyses already demonstrate. See supra p. 252.

[^] The measures of the effect of the passage of time are unreliable in these analyses for the same reasons given above in regard to other analyses of the three review stages combined. See supra pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 258-59; supra note 482, note[^].

590. Of course, the states we study are also only ones that impose the death penalty. But their vastly different rates of doing so, and a variety of other death-sentencing-focused criteria illustrated by Figures 11-21 above, establish a high degree of *inter-state* variability. See *supra* pp. 120-32. Indeed, as we develop in connection with Figures 42A and 42B above, one important way in which capital states differ is in how few or many of their counties use the death penalty. See *supra* pp. 246-49.

591. See *supra* note 504.

592. See *supra* pp. 246-50 & Figures 42A, 42B.

593. See *supra* pp. 250.

594. See *supra* note 504 (explaining that in our regression analyses, we have 1002 counties, not the 1004 discussed here, because the source of most of the county demographic and other data used in our regression analyses combined two contiguous counties in each of two states for purposes of analysis, forcing us to do the same).

595. Thus, if a state adopted a valid post-*Furman* death-sentencing statute in 1973, the number of homicides attributed to a county in that state is the number of homicides committed in the county during the full, 1973-1995 study period. On the other hand, if a state did not adopt a valid death-sentencing statute until 1978, then the number of homicides attributed to that county is the number committed there between 1978 and 1995. In either case, the death-sentencing rate is the number of death verdicts imposed under the modern death-sentencing statute divided by the number of homicides committed in the year that statute was adopted and in every subsequent year through 1995 multiplied by 1000.

596. See *infra* pp. 347-49 & Tables 19 and 20.

597. Overall reversal rates in this section are for the two of the three review stages: direct appeal and federal habeas. Nationally, during the study period, the overall reversal rate at those two stages was 65%. See *supra* p. 9. For discussion of how overall reversal rates are calculated, see *supra* pp. 8 & n.*, 19; *supra* note 103.

598. Murder is the only offense for which state and federal law permit the death penalty. See *Coker v. Georgia*, 433 U.S. 584 (1977) and allied decisions (forbidding executions for rape, armed robbery and kidnaping and suggesting that Court would not permit the penalty for non-homicide offenses); cf. *Enmund v. Florida*, 458 U.S. 782 (1982), as modified by *Tison v. Arizona*, 481 U.S. 137 (1987) (setting some modest limits on the types of murders for which the death penalty may be imposed). Moreover, although some jurisdictions retain the penalty for aggravated rape of minors and for crimes against the state such as espionage and treason, homicide was the only offense for which the 34 states imposed the penalty during the study period. Because murder is a form of homicide, and because homicide is the only relevant category of offenses on which public agencies consistently collect and report statistics, the number of homicides is the best available indicator of the number of the chances each jurisdiction had to impose the penalty. On why it is useful to compare counties with similar number of homicides, see *supra* pp. 287-88.

599. Here and throughout the remainder of this section, we consider only counties with five or more death verdicts during the study period, which are the counties collected in Table 11A in Appendix B.

600. We also conducted this comparison using the four counties' exact numbers of homicides (from 986 to 1361) to establish an upper and lower bound on the number of homicides in comparison counties. The results were nearly identical.

601. Again, these counties are limited to those with five or more death verdicts during the study period.

602. See *supra* pp. 8 & n.*, 19; *supra* note 103 (discussing how overall reversal rates are calculated).

603. The link between high county death-sentencing rates and high capital error rates has been noted in the press recently. See, e.g., Lise Olsen, *Unstable Marshall Is Fighting for his Life—Today; Pierce County Shouldn't Have Sent Mentally Ill Man to Death Row, Court Rules*, *Seattle Post-Intelligencer*, Aug. 7, 2001 (noting that “Pierce County [Washington] authorities, who over the years have sent more men to death row than prosecutors in any other county, . . . haven't been able to make many sentences stick. Of the eight Pierce County men condemned to die since 1981, only three remain on death row. Two of them were sent there this year and have not begun their appeals processes. The sentences of five others were reversed—convictions of three of those five also were overturned, and one of the five was set free. It's a capital-punishment machine that cost Pierce County \$1.3 million in 1999 alone, according to state records. This year, the county—where the sheriff's office regularly has to forgo investigating many crimes for lack of funds—will spend well over \$1 million on capital cases.”). See also Lise Olsen, *One Killer, Two Standards*, *Seattle Post-Intelligencer*, Aug. 7, 2001 (Pierce County Prosecutor “Ladenburg helped send two men to death row for shooting a single victim in a holdup—the only such cases among the 30 men sentenced to death in the past 20 years, though one recently had his conviction overturned on appeal. . . . Ladenburg is the only prosecutor in the state whose death-penalty cases have been overturned for misconduct by his office—it's happened twice. . . . Prosecutors in Pierce [County] lead the pack in seeking the death penalty, doing it in about 60 percent of all aggravated-murder cases, more than twice as often as King County [Seattle], according to . . . a study by the Washington Association of Prosecuting Attorneys.”)

604. See *supra* pp. 134-35.

605. The closest the research community has come to this ideal is the study David Baldus and colleagues conducted of the influence of race on capital sentencing in Georgia in the late 1970s and early 1980s. No similar study, however, has reached across state boundaries and decades.

606. For example, state post-conviction decisions in Nevada, Tennessee (in the first half of the study period) and Texas are frequently unpublished even at the appellate stage. Virtually no trial-level state post-conviction decisions are published.

607. See 28 U.S.C. §§ 2254(b), (c).

608. See *supra* pp. 15.

609. Because the entire case drops out of a multiple regression analysis if *any* of the many conditions under consideration for that case is not known, we had to exclude from consideration factors as to which there were more than a handful of cases where the presence or absence of a trait was unknown. Traits are unknown when the reviewing judges didn't choose to mention it one way or the other in their opinions. Among the traits that were excluded for these reasons were the defendant's age at the time of trial, the last year of school completed by the defendant and the exact number of prior crimes committed by the defendant. In some cases, we could meaningfully change the question from whether a trait was present in the case, to whether any

reviewing decision *referred* to the trait. In this event, “unknown” became “no.” Judges’ decisions to mention particular traits of cases, or not, were rarely significant, however—as one would expect, given the many, essentially conflicting, reasons why a judge might not mention a trait of a case (*e.g.*, it was not present; the judge didn’t know or wasn’t sure it was present; it was present but the judge didn’t think it was important enough to mention; it was present and played a role in the judge’s decision or in the decision of another judge who took part in the case, but the writing judge did not choose or remember to mention it; etc.).

As an example of the problem of insufficient variance, we knew the gender of the defendant in all cases, but there were so few women (6) that there was not enough variance to analyze.

610. See *supra* pp. 85-86 & n.239, 109-10, 148.

611. Aggravating and mitigating factors are treated somewhat differently by law, which accounts for our somewhat non-parallel treatment of them in this index. Most importantly, while essentially all capital statutes enumerate aggravating factors at least one of which must be present to justify a death sentence, not all statutes enumerate mitigating circumstances. On the other hand, while a number of states limit the aggravating factors jurors may consider to those enumerated in the statute, the federal Constitution requires that the sentencer consider all mitigating factors in the case, whether or not enumerated in the statute. In any event, we also constructed another index (the next one noted in text) which added some non-statutory aggravating factors, thus moderating the non-parallel treatment of aggravating and mitigating factors.

612. These factors are discussed further at pp. 320-21 below.

613. Scores on this index were not significantly related to the probability of federal habeas reversal.

614. See *supra* pp. pp. 85-86 & n.239, 109-10, 148 (discussing statistical significance).

615. This test provides effect-size information analogous to that provided in our state and county analyses. See *supra* pp. pp. 114017 & nn.282, 285; 171-73.

616. Some factors have only two possibilities—*e.g.*, the prisoner’s lawyer was or was not from out of state; a state evidentiary hearing was or was not held; a majority of the members of the panel of federal judges who finally decided the case were or were not appointed by Republican Presidents. In that event, the exponentiated B value indicates that the probability of reversal increases (if the value is above 1) or decreases (if the value is below 1) by the amount of the value times the original probability. If the exponentiated B value is 1.94, then the probability of reversal increases by 94% ($1 + 1(.94)$) if the relevant condition is present. On the other hand, if the exponentiated B value is .65, then the probability of reversal decreases by 35% ($1 - 1(.65)$) if the condition is present. Other factors have a range—*e.g.*, the number of aggravating circumstances minus the number of mitigating circumstances, and the number of supplemental aggravating circumstances. Here, the exponentiated B value indicates how much the probability of a reversal either increases or decreases with each increase or decrease of 1 in the relevant condition (*e.g.*, in the number of aggravating circumstances net of mitigating circumstances, or in the number of supplemental aggravating circumstances). So, if the exponentiated B value for supplemental aggravating circumstances is .78, then for every supplemental aggravating that is present in the case, the likelihood of reversal decreases by 22% ($1 - 1(.78)$).

617. This test, like the fit test discussed above, measures the distance between the federal habeas outcomes predicted by the set of explanatory factors and the actual outcomes. See *supra* pp. 149-50. We also examine results of three other diagnostic tests that tend to track the fit test:

- What (roughly speaking) is the overall amount of correlation between the various explanatory factors used and the condition being explained—here, whether each death verdict was overturned on final federal habeas review (chi square analysis)?
- In what percentage of cases is the outcome the set of factors predicts consistent with the actual federal habeas outcome, paying attention to the proportion of correct predictions of reversals, affirmances and all decisions as a whole?
- Very roughly, how much variance among outcomes does the group of factors explain?

618. The relevant factors are coded as follows in the detailed results in Appendix G: state evidentiary hearing held (“seh2”); defense lawyer at final federal habeas stage is not from sentencing state (“dlos_ffd”); federal evidentiary hearing was held (“feh2”); the number of statutory aggravating factors minus the number of mitigating factors (“agg_mit”); and index of seven other aggravating factors relating to the offender and victim (“ofvcindx”).

619. See supra pp. 311-12 & n.616.

620. $Fit = 758.5$ (5 explanatory factors).

621. See supra pp. 311 & n.616 (discussing interpretation of exponentiated B’s as a measure of effect size).

622. See, e.g., *Williams v. Taylor*, 529 U.S. 420 (2000).

623. See, e.g., *Kyles v. Whitley*, 514 U.S. 419 (1995).

624. See, e.g., *Amadeo v. Zant*, 486 U.S. 214 (1988).

625. See, e.g., *Estelle v. Smith*, 451 U.S. 454 (1981).

626. See 28 U.S.C. §§ 2254(b), (c).

627. See 28 U.S.C. §§ 2254(d)(2), 2254(e)(1), and pre-1996 statute, 28 U.S.C. § 2254(d).

628. See, e.g., *Townsend v. Sain*, 372 U.S. 293 (1963). Usually, the relevant hearing must be held after trial. Sometimes, a pre-trial hearing will suffice—as where a defendant claims the police coerced him into confessing or denied him a requested lawyer during post-arrest interrogation and the trial court holds a hearing on the matter before trial and takes testimony from, e.g., the defendant and the arresting police officers.

629. See supra pp. 199-200, 209.

630. See, e.g., supra pp. 170-71, 228, 244.

631. The descriptions of federal habeas practice and practitioners in this section are based on 1 Federal Habeas Practice & Procedure, supra note 203, §§ 2.2, 4, 7.1, 7.2, 12.1-12.5, 13-20, and the many sources cited there.

632. For two recent detailed profiles of some of the “approximately 50” lawyers nationally in this category, see Rare Breed: Death Penalty Lawyers Defend Rights of Politically Invisible, Nat’l Catholic Reporter, Oct. 5, 2001; Dan McAllister, Not Dead Yet: Penalty Cases Are Finding Fewer Takers at Big Firms. But Some Lawyers Still Fight Life-and-Death Battles—and Win, Amer. Lawyer, Dec. 4, 2001.

633. Federal funding for such lawyers did not become available in all cases until 1988, when Congress provided for the mandatory appointment and funding of federal habeas lawyers in capital cases. See 21 U.S.C. § 848(q)-848(r). Many of the cases in our study were decided before this law was passed.

634. See, e.g., 1 Federal Habeas Practice & Procedure, supra note 203, §§ 2.2, 13-20 and sources cited there.

635. In a few states such as Georgia and Texas, members of local private law firms volunteer their services to in-state capital defendants undergoing federal habeas review. Most such federal habeas lawyers, however, are from large law firms in such cities as Boston, New York, Philadelphia, Chicago, Minneapolis, Denver, Los Angeles, San Francisco, Seattle and Washington, D.C., who volunteer to represent capital habeas petitioners in other states.

636. Because of the high travel costs associated with representing a capital prisoner in another state, almost all of the out-of-state lawyers who volunteer to represent such prisoners are from large, well-funded private law firms. Such firms tend to hire only employees with strong academic records or proven track records in other firms, and they also tend to pay their lawyers well and provide ample support for investigators and experts.

637. See supra pp. 308, 311 & n.611; infra notes 639, 643.

638. See supra note 611; infra note 643.

639. The aggravating circumstances present in the case are formally listed in published decisions somewhat more consistently than mitigating circumstances. For that reason, we used two alternative measures of aggravation—one focused entirely on the number of aggravating circumstances found in the case, the other on that number minus the number of mitigating circumstances. The results using each version were consistently very similar, leading us to choose the latter version because it includes a bit more information.

640. This circumstance is not a duplicate of the common statutory aggravating circumstance noted above. This factor considers whether the defendant has *any* criminal record; most statutory aggravating circumstances consider a record of “violent” or “assaultive” crime.

641. High status victims include law enforcement officers, fire fighters, public officials, and well-known and respected personalities in the community.

642. See infra p. 324.

643. The two aggravating circumstance indexes are not highly correlated. $R = .038$; $p = .35$. This indicates that the two indexes analyze different sets of information about the seriousness of the offense, the moral blame attached to it and the extent to which the defendant is responsible for the offense and its morally repugnant characteristics.

As noted above, we constructed a third index of traits of cases, *i.e.*, a count of the types of evidence used to convict defendants, such as fingerprints, eyewitness identification, blood or other bodily fluids, a

weapon. We originally hoped this index might serve as a measure of the strength of guilt, because no direct measure of that condition is available. A review of the cases revealed the index's weakness for this purpose, however. First, unlike aggravation and mitigation, which are routinely and systematically listed in judicial decisions in most capital cases, descriptions of the evidence of guilt are inconsistent across cases and incomplete in most. Second, the presence of many types of evidence (even when we know it was present) is not a good indicator of the strength of the evidence of guilt. Under some circumstances, an eye-witness identification of the defendant as the killer is strong evidence of guilt. Under other circumstances, such identifications are highly unreliable. Complicating matters, analyses show that jurors are not always good at distinguishing reliable from unreliable eyewitness identifications. See, e.g., Richard O. Lempert, et al., *A Modern Approach to Evidence* 243-51 (2000) and sources cited. Habeas decisions' brief summaries of the facts of the case rarely indicate whether an eye-witness identification in the case fits in the "strong" or "weak" category, much less whether the jury accurately *believed* it was strong or weak. The same is true of many kinds of forensic evidence (e.g. hair, carpet sweepings, footprints), weapons and the like. So, when a case is coded as having an eye-witness identification or one of these other types of evidence, there is reason to expect the factor to be inconclusive as an indicator of strength of guilt, because it could point in opposite directions. (Although some kinds of evidence are more consistently strong—e.g., fingerprints and DNA—they are present in so few cases that there is too little variation to study. See *supra* note 609. This is especially so of DNA evidence, which was not available for trials until the early 1990s, and thus did not start appearing in cases under appellate review until the mid-1990s, at the very end of our study period.) Again, evidence of aggravation and mitigation tends to be different in this regard. Which way a circumstance cuts is often stated by law, and when it is not so stated, the issue of relevance—whether the circumstance tends to increase moral blame—is often fairly clear on the face of the circumstance.

The "types of evidence" index consistently had the same relationship to federal habeas outcomes as the level of aggravation: The fewer the types of evidence, the higher the probability of reversal. But unlike aggravation levels, fewer "types of evidence" was not a statistically significant predictor of reversals, because there is more than a 5% probability that the relationship appears by chance. See *supra* pp. 85-86, 109-10, 148 (on statistical significance). Given the difficulty of getting adequate data on this point, and given the ambiguity of available data, this result does not indicate that the strength of the evidence of guilt is irrelevant to the probability of reversal, but only that we cannot tell with any confidence whether it is relevant.

The "types of evidence" factor has a stronger (although still not statistically significant) relationship to the probability of reversal when it is considered by itself than when it is considered along with the two indexes of aggravating circumstances. This suggests that the two indexes may partly measure the strength of the evidence of guilt, leaving less of a role for the "types of evidence" factor in this regard. This is likely, because many aggravating and mitigating circumstances reflect fairly directly on the quality of the evidence of guilt. Aggravating factors do this by establishing motives or mental states that increase the probability of guilt. Mitigating factors do it by establishing mental conditions or statuses that decrease the probability that the defendant had the mental state needed to establish first-degree murder or increase the likelihood that someone besides the defendant planned and committed the killing.

644. See *supra* pp. 165-66, 168-69, 185, 210, 225-26, 243, 266.

645. See *supra* pp. 157-59, 160-63, 165-66, 322-23.

646. See *supra* pp. 22-23 & n.121 (citing sources), 125-35.

647. See, e.g., *Herrera v. Collins*, 506 U.S. 390 (1993); *supra* pp. 22-23.

648. See *supra* pp. 25-35.

649. See supra pp. 43-45.

650. See supra pp. 314-15.

651. See supra pp. 40-41.

652. See supra p. 134 & n.295.

653. Above, for example, we discovered that capital defendants with lawyers with volunteer, out of state lawyers—ones who tend to be more skilled and better funded—are more likely to have their capital verdicts reversed by federal habeas judges. See supra pp. 315-19. This could mean that skilled lawyers are better at uncovering error than other lawyers. Or it could mean that, although they are no better at uncovering error, they are more willing to volunteer to represent capital prisoners with stronger than with weaker legal claims. By controlling for the strength of claims, we can eliminate this latter hypothesis, because then, it appears that out of state lawyers secure reversals more often than other lawyers, even after controlling for the strength of the claims in the federal habeas petition.

654. See, e.g. 28 U.S.C. § 2254(e)(2).

655. See id.

656. See Federal Habeas Corpus Practice and Procedure, supra note 203, at § 20.1.

657. See id.

658. $R = .024$.

659. For example, the stronger the claim is, the more likely it is that the federal court will excuse the failure to raise the claim at trial or on appeal based either on prior counsel's incompetence, or on the state court's unreasonableness in not providing a hearing.

660. See Federal Habeas Practice and Procedure, supra note 203, § 20.4.

661. A table in Appendix F examines whether the various factors found significant in [Analysis 19](#) are correlated. None of those factors is significantly correlated with the "federal evidentiary hearing" factor.

662. This factor is coded "sentyr" in the results in the Technical Appendix.

663. $Fit = 722$ (6 explanatory factors).

664. See supra pp. 20-21, 91-93 & Figure 10, 140-42, 155, 177, 193, 216-17.

665. See supra pp. 91-93 & Figure 10.

666. See supra pp. 20-21, 91-93 & Figure 10, 140-42, 155, 177, 193, 216-17.

667. See supra pp. 194-96, 210, 235, 244, 259, 264 (Figure 43Q). For impressionistic evidence of an increase in state post-conviction reversal rates over time, see supra pp. 59-61 & Figures 3A and 3B. Cf. supra p. 213.

668. See supra pp. 65, 67 (Figure 5), 244-45. If the proportion of flaws remained steady over time, while state direct appeal courts got better at catching them, the resulting increase in direct appeal reversals could explain a parallel decline in federal court reversals. We cannot tell whether this happened.

669. See supra pp. 194-96, 210, 235, 244, 259, 264 (Figure 43Q).

670. See supra pp. 65, 67 (Figure 5), 244-45.

671. See id.

672. This factor is coded “claimno” in the results in the Technical Appendix.

673. There was one case in which both sentence year and the number of claims was missing, so Analysis 19C has 33, not 34, fewer cases than Analysis 19B.

674. $Fit = 656$ (7 explanatory factors).

675. The relationships here are complex, because the correlation between state evidentiary hearings and the number of claims, although positive, is very weak. $R = .114$; $p = .007$.

676. We rejected two other interpretations of this factor. First, we considered the possibility that court decisions granting reversal may understate the actual number of claims raised at the final federal stage, because once reversal is granted on one claim, there may be no need to address, and thus to mention or list, other claims. We discounted this interpretation for two reasons. (a) Federal appellate courts (which usually serve as the final federal habeas stage) are often clear about the number of claims raised at that stage, and where they are not clear the cases were coded as “unknown,” meaning the case was not counted in this analysis. (b) Most of the cases in which courts were unclear about the number of claims raised were affirmances, not reversals, of the defendant’s capital verdict. Second, we considered the possibility that better lawyers may exercise more control over the number claims raised, suggesting a correlation between number of claims and bad lawyering (and between bad lawyering and denials of federal habeas relief). We discounted this hypothesis, after discovering that out-of-state lawyers (who by and large are at the top end of the spectrum of low to high quality lawyers) raised almost exactly the same number of claims as did other lawyers—in other words, the correlation between out-of-state lawyers and number of claims was very close to 0—requiring some other explanation besides the quality of lawyering for variation in the number of claims raised.

677. See 28 U.S.C. § 2253. During the 1973-1995 study period, the general practice was to permit an appeal of as many claims as the lawyer chose to raise, if the federal courts were convinced that at least one of those claims was “substantial.” A 1996 change in the law has led most federal courts to alter this practice, and hear only the particular claims on which appeal was permitted.

678. Sometimes, capital defendants who win at the trial level will “cross-appeal,” arguing that the lower court should have granted relief on more claims that it did. Because this puts lawyers in the awkward position of both defending and attacking the lower court decision, they typically limit cross-appeals to one or two very strong claims.

679. This factor is coded “repmaj” in the results in Appendix G.

680. The deciding judges are not named in 36 decisions reversing capital verdicts, but only 18 affirming them. The statistically significant correlation between unsigned decisions and reversals supports a point made earlier: Judges more often decline to publish or publicly associate themselves by name with decisions reversing capital verdicts than with decisions approving verdicts—perhaps because reversals are more controversial than affirmances. See *supra* p. 16 & n.100.

681. Most of the cases lacking information on the deciding judges also lacked information on the number of claims raised, so the number of cases to be studied dropped by 24, not the full 54.

682. Fit = 613 (8 explanatory factors).

683. See *supra* pp. 38-39.

684. See *supra* pp. 328-30.

685. This factor was significant at the .05 level in some analyses and at other times dropped to between there and .10.

686. For reasons given at pp. 328-30 above, Analysis 19 does not reliably reveal any relationship between the age of capital verdicts and their probability of reversal.

687. See, e.g., *supra* pp. 165-66, 168-69, 185, 210, 225-26, 243, 266.

688. See *supra* pp. 170-71, 228, 244.

689. See *supra* pp. 199-200, 209.

690. See *supra* pp. 25-35, 323-324.

691. See *supra* pp. 323-24.

692. See *supra* pp. 315-18.

693. See *supra* pp. 16, 218-19, 266, 285. See also *supra* pp. 194, 257.

694. See *supra* pp. 198-99, 212, 217-18, 227, 236.

695. See *supra* pp. 216-17.

696. See *supra* pp. 91-93 & Figure 10, 140-42, 155, 177, 193, 216-17, 328-30.

697. See *supra* pp. 25-35.

698. See *supra* pp. 287-306 & Tables 10-16.

699. Brooke A. Masters, Executions Decrease For the 2nd Year: Va., Texas Show Sharp Drops Amid A National Trend, *Wash. Post*, Sept. 6, 2001.

700. A Governor's Role in Death Penalty Cases, Burden of Proof with Greta Van Susteren, CNN, Aug. 21, 2001, www.CNN.Com/Burden.

701. See supra pp. 166-68, 183-84.

702. See supra pp. 166-68, 183-84, 197, 214, 224, 239-42, 252, 256-57, 269, 271, 274, 280-81 (significant to highly significant in Analyses 2-5, 7-15, 18; just barely above significance in Analyses 16 and 17).

703. See supra pp. 166-68, 183-84, 224, 239-42, 252, 256-57, 269, 271, 274, 280-81 (Analyses 1, 2, 7-9, 11-15, 18; just barely above significance in Analyses 16 and 17).

704. See supra pp. 197, 214 (Analyses 3-5).

705. See supra pp. 319-23 (Analysis 19).

706. Regarding state reversal rates, see supra pp. 166-68, 183-84, 197, 214, 224, 239-42, (Analyses 1-4, 14, 15). Regarding county reversal rates, see supra pp. 252, 256-57, 269, 271, 274, 280-81 (Analyses 7-13, 18; just above the .05 significance level in Analyses 16 and 17).

707. See supra pp. 250, 265-66, 272, 281.

708. See supra pp. pp. 166-68, 183-84, 224, 239-42, 252, 256-57, 269, 271, 274, 280-81 (compare Analyses 7-13, 16-18 to Analyses 1-5, 8-17).

709. See supra pp. 287-306.

710. See supra pp. 86-98.

711. See supra pp. 183-84 & Figures 27A-D.

712. See, e.g., supra pp. 183-84 & Figures 27A-D, 197, 205 (Figures 35A, 35B), 229 (Figure 41C), 260 (Figures 43C, 43D), 271 & n.550.

713. In one case, discussed below, we examine rankings and predicted reversal rates based on direct appeal Analysis 3A.

714. Each state's weighted average for each factor is calculated using the following formula: (((number of death sentences in year x^1) x (factor value in year x^1)) + ((number of death sentences in year x^2) x (factor value in year x^2)) + . . . ((number of death sentences in year x^n) x (factor value in year x^n))) ÷ total number of death verdicts in years x^1 through x^n .

715. See supra pp. 70-73 & Figure 6.

716. See supra pp. 246-49 & Figures 42A, 42B. A majority of counties in 59% (20) of the 34 capital states imposed no death sentences during the study period.

717. The data are for the counties in which each city is located. Where county and city names differ, the county names are Maricopa (Phoenix), Harris (Houston), Dade (Miami), Cook (Chicago), Clark (Las Vegas), Pinellas (St. Petersburg), Hillsborough (Tampa), Duval (Jacksonville), Jefferson (Birmingham), and Broward

(Ft. Lauderdale). Sources for this table are DRCen, DADB, HCDB, Vital Statistics.

718. Two exceptions to this caveat are Shreveport, Louisiana and Dayton, Ohio, which had fewer than five death verdicts during the study period.

719. The Florida counties are Leon, Marion, St. Johns and Volusia.

720. The Florida counties are Bay, Brevard, Escambia, Martin, Okaloosa, Pinellas, Putnam and Indian River.

721. The additional Arizona counties are Yavapai and Yuma. The five additional Florida counties are Bradford, Citrus, Columbia, Pasco and Taylor.

722. The Georgia counties are Cook, Douglas, Jones, Meriwether, Seminole and Wayne. The Alabama counties are Blount, Coffee, Colbert, Monroe and Talladega. The Arizona county is Mohave. The Florida counties are Hernando, Santa Rosa, Sumter and Union.

723. The rates set out here are for the counties in which listed cities are located. Where that name is different from the listed city, the counties are as follows, in the order of locales listed in text: Shreveport (Cado Parish, LA), Dayton (Montgomery County, OH), Newark (Essex County, NJ), Atlanta (Fulton County, GA), Kansas City (Jackson, MO), Nashville (Davidson, TN), Albuquerque (Bernalillo, NM), Las Vegas (Clark County, NV), Reno (Washoe County, NV), suburban Baltimore (Baltimore County, MO), Akron (Summit County, OH), Jefferson City (Cole County, MO).

724. These counties are listed supra notes 719-22.

725. For recent articles contrasting relatively high death-sentencing areas like Houston, Philadelphia, suburban Baltimore County, Danville, Virginia, Columbus and Baldwin County, Georgia and Cincinnati with relatively low death-sentencing areas like Dallas, Pittsburgh, Baltimore City, Richmond, Virginia, Atlanta, and Columbus, Ohio, see 100 Colum. L. Rev., supra note 153, at 2068-69 n.114. See also Brooke A. Masters, Death Penalty, Location Are Linked in Va. Study: Execution Sought Most Often in Suburbs, Wash. Post, Dec. 11, 2001 (“Suburban prosecutors are significantly more likely to seek capital murder indictments and ask juries for a death sentence than their counterparts in rural and urban areas, the Joint Legislative Audit and Review Commission concluded after a year-long study. . . . The Virginia study concluded that prosecutors in medium-density jurisdictions, such as Prince William County and Danville City, sought the death penalty in 45 percent of possibly capital cases, compared with 16 percent in urban areas such as Richmond and Norfolk and 34 percent in rural areas.”); Lise Olsen, One Killer, Two Standards, Seattle Post-Intelligencer, August 7, 2001:

Location determines the odds that a criminal will face execution.

Since capital punishment was reinstated in Washington in 1981, it has been used as a prosecution tool in only half the state: 20 of 39 counties. . . .

Within our state, there are huge variations. In 20 years, Yakima County Prosecutor Jeff Sullivan has never taken a capital case to trial—though his county has one of the state's highest murder rates. Yakima, Skagit, Cowlitz and Chelan are all examples of medium-sized counties where the death penalty has never been imposed.

Compare that with Pierce County. As prosecutor for 12 years, John Ladenburg sought the death penalty 21 times before leaving office last year—about twice as often as other prosecutors statewide.

726. See supra pp. 287-306 & Tables 10-16; Appendix B.

727. See supra pp. 250, 265-66, 272, 281.

728. See supra pp. 319-20 & n.639.

729. See supra pp. 313, 319-20, 328, 330, 333.

730. See supra pp. 322-24. See also 157-59, 160-63, 165-66.

731. See supra pp. 168, 185-86, 197, 226-27, 256-57, 268-69, 271, 274. This result was reached by all studies of state-level factors related to state and county capital error rates at all three review stages combined and at the state direct appeal stage (Analyses 1-4, 8-17). Although we express the finding in the text as one about state reversal rates in states with poor law enforcement records, the finding also applies to county reversal rates in such states.

732. See supra pp. 185-86, 197, 206, 226-27, 232, 256-57, 262, 271 & n. 274, 277 & n. 55, & Figures 28A-D, 36A, 36B, 41G, 43K, 43L, 44F.

733. See infra pp. 370-72.

734. See supra pp. 165-66, 168-69, 185, 210, 225-26, 243, 266, 321-22, 335, 349-50.

735. Both a general explanation for high error rates (heavy use of the death penalty) and a related specific explanation (concerns about the ineffectiveness of the state's response to serious crime, triggering heavier use of the penalty) can be significant at the same time, if (1) there are multiple reasons for heavy death-sentencing, and (2) some reasons are more closely linked to error than others. In that event, an indicator of the intensity of one of the important reasons for heavy use of the penalty leading to error (*e.g.*, evidence that non-capital law-enforcement strategies are ineffective) will only partly explain high error rates, leaving the rest to be explained by indicators of the other important pressures, or by a general measure of *all* pressures to use the death penalty (*e.g.*, high death-sentencing rates). Below, we explain why the four separate pressures to use the death penalty addressed in this and the next three sections may be particularly conducive to high rates of capital error, and thus why it is not every additional use of the death penalty, but only the penalty's use in weakly aggravated cases, that increases error rates. See infra pp. 359-60, 367.

736. See supra p. 343.

737. See supra pp. 51 (Figure 1B).

738. See supra pp. 169-70, 187-88, 198, 217-18, 227, 258, 269, 271, 274 (Analyses 1-4, 6, 8-17). In regard to direct appeal Analyses 3 and 4 in which this result fell just barely above the .05 level ($p = .06$), see supra note 486.

739. See *id.*

740. See supra pp. 119-20, 133.

741. See supra pp. 169 & n.369.

742. See supra pp. 187-88, 199, 207, 217-18, 221, 227, 232, 258, 262, 271 & n.554, 274, 277, & Figures 29A-D, 37A, 37B, 40C-1, 40C-2, 41H, 43I, 43J, 44E.

743. See supra pp. 69-70 & n.370.

744. Virginia in fact got only the second lowest score that is possible on the index, namely, a score of 2. But that was the lowest score among the 34 study states. None of the 34 study states scored a 1, because none uses gubernatorial or nonpartisan appointment procedures in addition to immunizing judges entirely from regular or at least retention or recall elections.

745. The formula for calculating this factor is white homicide victims per 100,000 whites ÷ black homicide victims per 100,000 blacks. As is discussed supra p. 160 & n.341, the homicide rate among blacks is usually higher than among whites. In most cases, that is, this factor compares states based on how much lower the white homicide rate is than the black homicide rate—or, conversely, how closely the white homicide rate approaches the black homicide rate.

746. See supra pp. 159-61, 181-82, 196, 213-14, 225-26, 257, 269, 271, 274. This result was reached by analyses studying state and county capital error rates at all three review stages combined and at the state direct appeal stage separately (Analyses 1-4, 8-17). This factor was just above the .05 significance level in our single-stage analysis of the state post-conviction stage, where, in addition, there was a significant relationship between higher reversal rates and higher homicide rates among whites (apart from any comparison to the homicide rate among blacks) (Analysis 5).

747. See supra pp. 181-82, 196, 204, 213-14, 225-26, 231, 257, 262, 269, 271 & n.552, 274, 276, & Figures 26A-D, 34A, 34B, 41F-1, 41F-2, 43G, 43H, 44D.

748. Although a few other states have much lower predicted reversal rates, their extremely low black populations lead us to exclude them from the comparison made in text.

749. See supra pp. 224-26, 253, 280 (Analysis 7, 15 and 18). See also supra p. 213 (significance of homicide victimization rate among whites in Analysis 5).

750. The one exception was Analysis 5, in which the white homicide rate by itself was a slightly better predictor of error rates at the state post-conviction stage than the white/black homicide rate. See supra pp. 213-14.

751. See Cole, supra note 337; Kennedy, supra note 337; other sources cited supra notes 337, 338, 358, 360. See also sources cited supra notes 349-51, 353, 354.

752. See sources cited supra note 337, 338.

753. See supra note 735 (discussing conditions under which not only high death-sentencing rates themselves, but also particular pressures to increase death-sentencing rates, could both be significant).

754. See supra pp. 157-59, 160-63, 165-66 & Table 6.

755. See supra pp. 322-24, 351.

756. The relevant policies appear to be related to the *statewide* distribution of the risk of homicide among whites and blacks, not to its local distribution. In no analysis of county-level factors—not even Analysis 7, which omitted state-level factors, giving county-level factors the maximum opportunity to explain reversal rates—was there any significant relationship between the countywide distribution of the homicide risk between whites and blacks and county reversal rates.

757. See supra pp. 157-59, 179, 196, 224, 257, 269, 271, 274. This result is reached by all our analyses of state factors associated with state and county reversal rates at all three review stages combined and at the state direct appeal review stage by itself (Analyses 2-4, 8-17).

758. See supra pp. 217 (Analysis 6).

759. See supra pp. 179-80, 196, 203, 224, 257, 261, 271 n.551, 274, 276 & Figures 25A-25D, 33A-33D, 41D-1, 41D-2, 43E, 43F, 44C.

760. See supra pp. 217, 220 & Figure 40B (Analysis 6). Like other effects, these ones appear to operate at the level where policy is made, not at the level of individual cases. Capital verdicts imposed on black defendants are no more likely to be overturned due to serious error than those imposed on white or other defendants. See supra pp. 157-59 & Table 6. (Because nearly all capital defendants are poor, but information on how poor is not kept by officials, differences in error rates linked to the economic status of capital defendants cannot be studied.)

Reflecting another pattern noted above, the relevant capital policies seem to be related to the proportion of African-Americans in the *state*, not the county, population. No analysis of county-level factors—not even Analysis 7, which omitted state-level factors, giving county-level factors the greatest opportunity to explain reversal rates—revealed any significant relationship between proportion of blacks in a county's population and its capital reversal rates.

761. See supra pp. 163-65.

762. See supra pp. 157-59 & Table 6, 160-63; supra note 760.

763. This research is collected supra notes 358, 360.

764. See supra p. 361.

765. See supra pp. 5 & n.77, 24-35 & nn.134, 146, 148; supra p. 80 & n.227.

766. See sources cited supra notes 358, 360.

767. See, e.g., supra p. 160 & n.341.

768. See sources cited supra note 358.

769. See *id.*

770. See supra pp. 224-26, 266, 285, 336.

771. See id.

772. This factor was significant to highly significant in Analyses 1, 2, 8-10, 12, and 13 and fell just above the .05 significance level in Analyses 4, 11 ($p = .056$), and 14. See supra pp. 162-63, 226, 257, 269, 271.

773. See sources cited supra notes 337, 338.

774. States' Rank and Value Based on Interaction of Race of Population and of Homicide Victims

State	Interaction of Race of Population and of Homicide Victims	
	Rank	Value
Connecticut	21	0.014
Kentucky	16	0.020
Maryland	8	0.046
Tennessee	11	0.036
Mississippi	2	0.094
Oregon	29	0.003
California	18	0.019
New Jersey	15	0.022
Idaho	32	0.001
Montana	34	0.000
Georgia	4	0.066
Arizona	27	0.007
Alabama	5	0.060
Colorado	26	0.010
Washington	28	0.005
Wyoming	33	0.000
Florida	13	0.030
Oklahoma	17	0.019
Indiana	25	0.010
Arkansas	12	0.034
North Carolina	6	0.059
Nebraska	30	0.002
Nevada	20	0.016
South Carolina	1	0.100
Utah	31	0.001
Louisiana	3	0.070
Illinois	14	0.022
Pennsylvania	23	0.013
Texas	10	0.040
Missouri	22	0.013
Delaware	7	0.047
New Mexico	24	0.012
Ohio	19	0.016
Virginia	9	0.044

Source: Analysis 1A

775. See supra pp. 157-59, 160-63, 165-66, 322-24, 351, 360-61 & n.756, 362 & n.760.

776. See supra pp. 169 & n.369, 356.

777. See supra note 735.

778. See supra pp. 153, 173, 194, 212, 253, 257, 269, 271, 273, 280 (Analyses 1-5, 7-18).

779. See supra p. 173-74 & Figures 22A, 22B. See also pp. 201, 229, 260, 276 (Figures 22A-D, 31A, 31B, 41A, 43A, 43B, 44A). In several analyses, high per capita filings of court cases of all types operate similarly. Effect size is too small to warrant additional attention, however. See supra pp. 154, 174, 176, 223, 229, 258, 263 & Figures 23A-D, 41B, 43N.

780. See supra pp. 90, 173-75 & Figures 22A-D.

781. See supra p. 342 & n.714.

782. See supra pp. 89-90, 153-54, 173-74, 212, 223, 351.

783. See supra pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 213, 216-17, 258-59.

784. See supra pp. 20-21, 88-89, 172, 183, 194-95, 343 & n.713.

785. See, e.g., supra p. 21 & n.116.

786. See supra 91-93 & Figure 10, 140-42, 155, 216-17, 177, 193, 328-30, 336.

787. See supra pp. 194, 257.

788. The table below reports state rankings, and the difference between their predicted reversal rates based only on their capital backlogs, holding other factors at their averages. As is discussed in the text, the high reversal rates predicted for states with low capital backlogs (*e.g.*, Nebraska), and the low predicted reversal rates for states with large backlogs and delays in capital appeals (*e.g.*, California) occurs because of the perverse tendency of delay to depress reversal rates. Because this factor does not accurately reflect the risk of error—and instead reflects the effect of delay—we report its results here, rather than in Table 18.

States' Rank, and Comparison to Predicted 34-State Average Error Rate,
Based on Capital Backlogs (Analysis 1A), Holding Other Factors at the 34-State Average

State	Backlog of Capital Appeals (Higher Value = Lower Review Rate = Lower Reversal Rate)		
	Rank	Value	Difference from 34- State Avg. Error Rate
Connecticut	4	0.8	+40.7%
Kentucky	16	2.8	+30.7%
Maryland	6	1.1	+38.9%
Tennessee	23	5.4	+19.0%
Mississippi	14	2.3	+33.2%
Oregon	12	1.8	+35.3%
California	34	26.9	-10.7%
New Jersey	9	1.4	+37.4%
Idaho	13	1.9	+34.8%
Montana	2	0.5	+41.9%
Georgia	21	4.0	+25.1%
Arizona	25	6.5	+14.4%
Alabama	27	7.9	+9.5%
Colorado	8	1.3	+37.8%
Washington	3	0.5	+41.7%
Wyoming	5	0.8	+40.4%
Florida	32	17.6	-7.5%
Oklahoma	26	6.9	+13.1%
Indiana	17	2.9	+29.9%
Arkansas	7	1.1	+38.8%
North Carolina	29	9.9	+3.9%
Nebraska	1	0.4	+42.1%
Nevada	22	4.4	+23.1%
South Carolina	19	3.5	+27.4%
Utah	11	1.6	+36.4%
Louisiana	15	2.4	+32.3%
Illinois	28	8.7	+7.0%
Pennsylvania	31	14.6	-4.6%
Texas	33	18.5	-8.1%
Missouri	24	5.7	+17.8%
Delaware	18	3.2	+28.7%
New Mexico	10	1.5	+36.7%
Ohio	30	12.4	-1.4%
Virginia	20	3.8	+25.7%

Source: Analysis 1A.

789. See supra pp. 170, 227, 269, 272, 273-74 (Analyses 1, 2, 11-17).

790. See supra pp. 199-200 (Analyses 3, 4).

791. See supra pp. 199-299, 209 & Figures 39A, 39B (Analyses 3 and 4). See also supra p. 54 & n.194.

792. See supra p. 41.

793. See infra pp. 413-18. For citation and discussion of numerous government, bar association, judicial and press reports thoroughly documenting the relationship between low funding levels and incompetent capital lawyering, and the especially high demands that capital cases place on lawyers and legal support services, see 100 Colum. L. Rev., supra note 153, at 2102-10 & nn.175-91.

794. See supra pp. 314-15.

795. See supra p. 342 & n.714.

796. See supra pp. 199-200 & Figures 39A, 39B.

797. States' Value and Rank, Holding Other Factors Constant, for Interaction of Backlog of Capital Appeals and General Court Caseloads

State	Interaction of Backlog of Capital Appeals and General Court Caseloads	
	Rank	Value
Connecticut	13	-0.2
Kentucky	27	-1.6
Maryland	11	0.8
Tennessee	10	0.9
Mississippi	21	-0.9
Oregon	22	-0.9
California	1	69.0
New Jersey	12	0.7
Idaho	28	-1.6
Montana	15	-0.3
Georgia	9	1.9
Arizona	33	-3.5
Alabama	24	-1.3
Colorado	19	-0.8
Washington	14	-0.3
Wyoming	20	-0.8
Florida	2	54.5
Oklahoma	17	-0.5
Indiana	30	-1.9
Arkansas	18	-0.6
North Carolina	6	4.4
Nebraska	16	-0.4
Nevada	34	-4.2
South Carolina	29	-1.8
Utah	25	-1.5
Louisiana	26	-1.5
Illinois	4	12.9
Pennsylvania	5	12.3
Texas	3	20.5
Missouri	8	2.0
Delaware	32	-3.3
New Mexico	23	-1.1
Ohio	31	-2.7
Virginia	7	4.3

Source: [Analysis 1A](#).

798. See supra pp. 52-61 & Figures 2C, 2D, 3A, 3B; *Broken System, Part I*, supra note 101, at 38, Figure 3.

799. As long as some proportion of imposed death verdicts are flawed, and as long as reversal rates are calculated by taking the number of reversals as a proportion of all imposed verdicts (reversals divided by imposed verdicts), any drop in the number of finally reviewed verdicts will drive down reversal rates,

because fewer of the flawed verdicts will be available to be reversed while the number of imposed verdicts stays the same. The numerator (reviewed and reversed verdicts) shrinks while the denominator (imposed verdicts) stays the same, causing the rate to drop.

800. See supra pp. 89-90, 97, 99, 152-53, 154-56.

801. See supra pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 213, 216-17, 258-59.

802. See supra pp. 20-21, 91-93 & Figure 10, 140-42, 155, 177, 193, 216-17, 328-30, 336, 353, 369.

803. In that event, delay affects the denominator as much as the numerator. See supra note 799.

804. See supra pp. 194-95, 202, 258-59, 263 & Figures 32A, 32B, 43Q. See also supra pp. 20-21, 88-89, 172, 173, 352-53. Effect size for Analysis 3 is graphed in Figure 32A, p. 202. Analysis 4 likewise predicts and 8-fold increase in error rates over the study period (Figure 32B, p. 202). When county as opposed to state reversal rates are analyzed, the predicted increase in reversal rates over the 23-year period is 3-fold (Figure 43Q, p. 263).

805. See supra note 194.

806. See supra pp. 89-91.

807. See supra pp. 89-91, 99, 140-42 & Table 4, 154-56, 177, 194-95, 213, 216-17, 258-59, 372-75.

808. See supra pp. 216-17, 328-30 (Analyses 6, 19).

809. See supra pp. 154-56, 213, 216-17, 253, 258-59, 280 (Analyses 1, 2, 5-7, 11, 12, 18).

810. See supra pp. 177-78 & Figures 24A-D (Analyses 1A, 1B, 2A, and 2B). See also the effect-size estimates in Appendix G for Analyses 5, 7, 11 and 12 (infra pp. G-7, G-9, G-13, G-14) and supra pp. 280 and n.577 for a discussion of the low effect size for this factor in Analysis 18.

811. See supra pp. 259, 271 & n.548, 264 (Figure 43P) (discussing and displaying the results of Analyses 8, 9 and 13, in which there was no significant relationship between error rates and the passage of time).

812. See supra pp. 259, 271.

813. See supra pp. 54, 57, 58 & Figures 2C and 2D.

814. See supra pp. 52-53 & Figure 2A; *A Broken System, Part I*, supra note 101, at 38, Figure 3.

815. See supra pp. 65, 67 (Figure 5), 245 & nn.499, 500..

816. See supra pp. pp. 52-53 & Figure 2A. See also supra pp. 89-91, 99, 140-42, 154-56, 177, 194-95, 213, 216-17, 258-59, 351-52.

817. See supra p. 24.

818. See supra pp. 1-5.

819. See *supra* pp. 63-64 & nn.202-03.

820. See Editorial, *State's Record in Death Cases Cause for Study*, Tallahassee Dem., Dec. 14, 2001:

If an automaker led the industry in recalls, then spun the bad news as proof of excellent self-regulation, consumers would be skeptical. The automaker might deserve kudos for its efforts to rectify problems, but the high recall rate still would indicate a serious problem. A responsible company would identify the deficiency before so many recalls were required.

That's why it's so difficult to understand the reasoning of Florida death penalty advocates who resist calls for a moratorium to thoroughly examine the administration of justice in capital cases.

In 2000, nine death sentences in Florida were overturned, the highest number in the nation, according to a U.S. Department of Justice report released Tuesday. Yet, defenders of the system insist that such statistics prove the system works, since defendants in those cases aren't executed—at least until they're retried without legal error.

That's of no small consequence, of course, but Florida's high rate of overturned capital convictions remains troubling. It alone warrants a temporary suspension of executions—as Gov. George Ryan of Illinois ordered in his state—so problems in the process can be identified and fixed.

821. See *supra* pp. 6-7 (explaining why it is much harder in the capital than in these other areas to tell whether egregious harm has occurred—including because officials are permitted to withhold and destroy evidence on the question).

822. Classic treatments of this problem in the *Chicago Tribune* are: Ken Armstrong & Steve Mills, *Flawed Murder Cases Prompt Calls for Probe*, *Chi. Trib.*, Jan. 24, 2000; Ken Armstrong & Maurice Possley, *Break Rules, Be Promoted*, *Chi. Trib.*, Jan. 14, 1999 (detailing patterns of error by Illinois police officers, prosecutors, and judges that went unnoticed and unremedied by reviewing judges), and Ken Armstrong, *"Cowboy Bob" Ropes Wins—But at Considerable Cost*, *Chi. Trib.*, Jan. 10, 1999 (same, Oklahoma City prosecutors). For other examples, see Sara Rimer & Raymond Bonner, *Texas Lawyer's Death Row Record a Concern*, *N.Y. Times*, June 11, 2000 (same, Texas defense lawyer); Shiffman, *supra* note 102 (Tennessee courts' failure to review comparative information kept in capital cases, which has sat in files, entirely unused, for 30 years); 100 *Colum. L. Rev.*, *supra* note 153, at 2089-91 n.151 (various sources discussing pattern of misconduct in capital cases by police at particular Chicago precinct house), 2094-95 n.160 (Armstrong & Possley discussing Chicago prosecutor; Hunt discussing Cincinnati prosecutors; Rosenberg, discussing Philadelphia prosecutors), 2101 n.173 (Armstrong & Possley, discussing New Orleans police and prosecutors), 2104 n.178 (various sources discussing continued appointment of bar-disciplined capital defense lawyers), 2119-29 7 nn.227-33 (esp 231). See also notes 123, 160 (tendency of judges in Illinois, Ohio and Texas to pass over error, as "harmless"); *infra* note 941 (additional examples of repeated appointment of same poorly prepared defense lawyers in Texas and elsewhere).

823. These reports are collected and discussed in 100 *Colum. L. Rev.*, *supra* note 153, at 2119-29.

824. See *id.* at 2120-21.

825. See *id.* at 2121-27.

826. See *id.* at 2078-82 & nn.137-40 (citing numerous examples of susceptibility of prosecutors to political pressures in potentially capital cases); *supra* pp. 169, 187.

827. See *supra* pp. 194, 257, 368-69.

828. See *supra* pp. 315-18.

829. See *supra* p. 63 & n.202.

830. See *supra* pp. 63-64.

831. See *supra* pp. 70-80.

832. See, e.g., *supra* pp. 25-35 (four cases studies).

833. See *supra* pp. 5 n.77, 24.

834. See *supra* pp. 25-35.

835. See *supra* pp. 37-38.

836. See *supra* pp. 319-24.

837. See *supra* pp. 169-70, 187-88, 198, 217-18, 227, 258, 269, 271, 274, 354-56.

838. See *supra* pp. 198-99, 212, 217-18, 227, 230, 240 n.486, 336.

839. See *supra* pp. 218-19, 236. See also *infra* note 876 (cataloguing studies in which this factor was significant and had considerable—in some cases quite large—effect size).

840. By “more” and “less” homicides and death verdicts, we mean numerically, not per homicide.

841. See *supra* pp. 218-19, 236.

842. States' Rank, and Comparison to Predicted 34-State Average Error Rate,
Based on Population Size and Density, Other Factors at the 34-State Average

State	Population Size and Density		
	Rank	Value	Difference from 34-State Avg. Error Rate
Connecticut	8	0.92	+4.5%
Kentucky	19	0.15	-6.8%
Maryland	7	0.94	+4.7%
Tennessee	14	0.39	-3.4%
Mississippi	22	-0.30	-12.8%
Oregon	27	-0.53	-15.6%
California	1	1.62	+14.9%
New Jersey	2	1.61	+14.8%
Idaho	31	-1.50	-25.6%
Montana	33	-1.96	-29.1%
Georgia	13	0.42	-3.0%
Arizona	26	-0.48	-15.0%
Alabama	20	0.12	-7.2%
Colorado	24	-0.39	-14.0%
Washington	17	0.16	-6.7%
Wyoming	34	-2.26	-31.1%
Florida	6	1.15	+7.9%
Oklahoma	21	-0.25	-12.1%
Indiana	12	0.60	-0.4%
Arkansas	25	-0.45	-14.7%
North Carolina	10	0.64	+0.2%
Nebraska	28	-1.01	-20.9%
Nevada	32	-1.59	-26.3%
South Carolina	18	0.16	-6.7%
Utah	29	-1.14	-22.2%
Louisiana	15	0.25	-5.4%
Illinois	5	1.16	+8.0%
Pennsylvania	3	1.29	+10.0%
Texas	9	0.82	+2.9%
Missouri	16	0.23	-5.6%
Delaware	23	-0.34	-13.2%
New Mexico	30	-1.30	-23.7%
Ohio	4	1.24	+9.2%
Virginia	11	0.62	0.0%

Source: Analysis 1A.

843. See *supra* pp. 194, 201, 212, 257.

844. See *supra* pp. 194, 257.

845. The Federalist, No. 83, at 499 and No. 81, at 486 (Clinton Rossiter ed. 1961).

846. See supra pp. 333-34.

847. See supra pp. 321-24.

848. See supra pp. 25-25, 321-24.

849. See supra pp. 6-7.

850. See supra pp. 1-2 & n.3, 4-5 & n.69.

851. See supra p. 11.

852. See supra pp. 350-66.

853. See supra pp. 37-69.

854. See supra pp. 24-25.

855. See supra pp. 16 & n.100, 194, 198-99, 212, 217-18, 227, 230, 257, 240 n.486, 336, 382-84.

856. See supra pp. 5 n.70, 24.

857. See 100 Colum. L. Rev. supra note 153, at 2050-51 n.84 (collecting sources); supra pp. 25-35.

858. See supra pp. 343, 353-54. This is true even when Table 18 is supplemented by the information in the tables in notes 774, 788, 797 and 842 above.

859. See supra notes 774, 788, 797, 842.

860. See supra note 714.

861. Moreover, as is noted above, Connecticut's and Colorado's highest "risk" ranking is, perversely, a result of how few delays the two states have in processing capital appeals, as is reflected by their low backlogs of pending capital appeals. See supra pp. 382-84 & n.797. As we develop above, delays in reviewing capital verdicts decrease the number of reversals (because there are fewer outcomes of any sort) and depress reversal rates measured as proportions of imposed verdicts (for the same reason). Large backlogs of pending capital appeals also seem to make reviewing courts more willing to tolerate error that would lead to reversal if the courts had fewer cases backed-up awaiting review. See supra pp. 194, 257, 382-84. As a result, states like Connecticut and Colorado where reversal rates are solely a function of error, and are not confounded by delay, are at "risk" of having higher reversal rates than states such as California, Florida and Texas, where the number and rate of reversals are held down by low rates of decided appeals.

862. See supra note 714.

863. See supra pp. 194, 257, 382-84; supra note 861.

864. See supra pp. 303-04 & Table 16.

865. See Innocence and the Death Penalty, supra note 77.

866. Georgia has two rankings of 21 out of 34.

867. See sources cited *supra* notes 160, 165. See also Editorial, Harmful Error—Criminal Appeals Court Eroding Justice for all Texans, *Houston Chron.*, Jan. 21, 2002 (“The Texas Criminal Court of Appeals is the state’s highest authority on criminal justice matters. In recent years, a majority of the court’s jurists have demonstrated an affectionate tolerance of incompetent judging and lawyering In an opinion earlier this month, the court ruled that a defendant facing execution has no right to have a competent lawyer handle his appeal.”).

868. See *supra* pp. 68-69.

869. See *id.*

870. See *supra* pp. 341-45, 354-56.

871. These Review Commission’s findings are discussed in note 30 above.

872. See *infra* pp. 405-06, 411-12, 418.

873. See *supra* p. 3.

874. See *supra* pp. 350-66.

875. Such doubts evidently tempt policy makers to broaden the use of the death penalty as a quick and dramatic demonstration of a will to combat serious crime. See *supra* pp. 350-66. Above we explain why this response to the problem of serious crime is likely to make matters worse, given the association between high capital-sentencing rates and high rates of serious capital error. Obviously, erroneously convicting and capitalizing sentencing the wrong person, while leaving the actual killer at large—as has repeatedly occurred, see sources cited *supra* notes 227—is not an effective law enforcement strategy. Moreover, the death penalty is unlikely to provide an effective across-the-board solution to problems with a state’s response to serious crime, given that the Constitution bars the death penalty for any crime other than wilful homicide, including crimes that affect the largest number of citizens—rape, robbery, burglary, car jacking, drug dealing, theft, assault and even most homicides (*e.g.*, ones during domestic arguments and spur-of-the-moment fights between acquaintances). Instead, attempting to cure across-the-board deficiencies in a state’s law enforcement capacity by extending the death penalty to some additional, marginally aggravated homicides is likely to divert millions of dollars per capitalizing sentenced inmate away from effective crime control and into additional trials, multi-stage appeals and retrials that in the vast majority of cases will end with *non-capital* outcomes.

876. See *supra* pp. 170, 189-90 & Figures 30A-D, 199, 206 (Figures 36A, 36B), 215, 227, 232 (Figure 41I), 253, 258, 263 (Analysis 43M), 266, 269, 271 & n.556, 274, 277 (Figure 44G) (Analyses 1-5, 7, 9-17).

877. A cure for the crime problem eluded policy makers for decades beginning in the mid-1960s. Even in the recent period of declining crime rates, there has been widespread puzzlement about why the declines occurred, and insofar as the drop in crime is a result of the recent period of sustained economic prosperity and dropping crime rates, there is reason to fear that the motivating trends have ended. See, *e.g.*, Andy Newman, Giuliani’s Last Crime Report Shows Sharp Drop Despite National Upward Trend, *N.Y. Times*, Jan. 1, 2002 (“Elsewhere in the country [besides New York City], crime seemed to rebound in 2001. As of

mid-December, murders were up by more than 60 percent in Boston and Phoenix and several other big cities, including St. Louis, Houston and Atlanta, which all posted double-digit percentage increases in murders.”); Willing, *supra* note 1 (“The [recent] decline in death sentences has followed a steep drop in the nation’s murder rate, which fell nearly 21% from 1996 to 2000. . . . Analysts say public support for capital punishment could begin rising again, if violent crime—which has ticked upward in some cities—continues to rise and Americans feel less secure, particularly in light of terrorism threats.”).

878. See *supra* pp. 354-56.

879. See *supra* pp. 370-72.

880. See *supra* pp. 195-96, 244.

881. See *supra* pp. 195-96, 244, 322-26.

882. See *supra* pp. 157-59, 160-63, 165-66, 322-24, 350, 359-60 & n.756, 362 & n.760.

883. See *supra* pp. 70-81.

884. See *supra* pp. 392-93.

885. See *supra* pp. 195-96, 244, 322-26.

886. See Ken Hambleton, Researcher: Limit Call for Executions, *Lincoln J. Star*, Oct. 18, 2001. Professor Baldus proposed that the Nebraska Legislature or Nebraska prosecutors limit capital prosecutions to “cases with two or more aggravating factors.” This is similar to proposals made by Professor Baldus, and endorsed by four United States Supreme Court Justices in a dissenting opinion in *McCleskey v. Kemp*, 481 U.S. 279 (1987) (Stevens, J., dissenting) as a cure for pronounced racial disparities—mainly associated with the race of the victims—in who receives the death penalty in Georgia.

887. Several of the proposals made here track ones made recently by the Committee to Prevent Wrongful Executions. This Committee is “a nonpartisan panel of judges, former prosecutors and victims advocates,” some of them “strong death penalty supporter[s],” that recently “spent a year studying capital punishment in the United States” and made “18 proposals . . . intended to make the death penalty more reliable and less open to chance.” Brooke A. Masters, Standards for U.S. Executions Proposed; Nonpartisan Committee Seeks Broad Consensus on Reforming Death Penalty, *Wash. Post*, June 27, 2001, at A4. Proposals made below parallel the Committee’s recommendations that states “[s]et mandatory-minimum standards for defense lawyers and pay them adequately, and make it easier for death row inmates to get new trials if they had bad lawyers”; “[f]ully inform juries by giving them the explicit option of life without parole”; “[p]reserve and test DNA after convictions and make it easier for inmates to get new trials based on newly discovered evidence”; and “urge[] prosecutors to open all their files to the defense before trial.” *Id.*

For further development of some of the proposals made here in the context of a more comprehensive and specific reform proposal, see James S. Liebman, *Opting for Real Death Penalty Reform*, 63 *Ohio St. L.J.* ____ (forthcoming 2002).

888. See *supra* p. 4 & n.66; *supra* note 1. See also Margery Koosed, Averting Mistaken Executions by Adopting the Model Penal Code’s Exclusion of Death in the Presence of Lingering Doubt, 21 *N. Ill. U.L. Rev.* 41 (2001).

889. See *supra* pp. 25-35.

890. See *supra* pp. 25-35. See also 100 Colum. L. Rev., *supra* note 153, at 2114-19.

891. See sources cited in Lempert, *supra* note 643, at 1243-48.

892. See, e.g., *Boyd v. California*, 494 U.S. 370 (1990); *Blystone v. Pennsylvania*, 494 U.S. 299 (1990).

893. See *Walton v. Arizona*, 497 U.S. 639 (1990).

894. Court Finds Death Penalty Is Misused in Kansas, N.Y. Times, Dec. 30, 2001 (“The Kansas Supreme Court has found that a crucial aspect of the way the state’s death penalty is handed down is unfair and must be changed,” requiring new sentencing hearings for all death row inmates in the state).

895. On the need for clear instructions, see Theodore Eisenberg & Martin T. Wells, *Deadly Confusion: Juror Instructions in Capital Cases*, 79 Cornell L. Rev. 1, 11-12 (November 1993) (finding, based on South Carolina data, that “[j]uror comprehension appears to be worse when mitigating factors are considered”); Craig Haney, *The Capital Jury Project: Taking Capital Jurors Seriously*, 70 Ind. L.J. 1223, 1229 (Fall 1995) (same); see also Stephen P. Garvey, et al., *Correcting Deadly Confusion: Responding to Jury Inquiries in Capital Cases*, 85 Cornell L. Rev. 627, 637 (March 2000) (reporting that 41% percent of jurors interviewed “erroneously believed that the law required them to impose a death sentence if [at least one aggravating circumstance was present, e.g., if] the evidence proved that the defendant’s crime was ‘heinous, vile, or depraved’”); Theodore Eisenberg et al., *Jury Responsibility in Capital Sentencing: An Empirical Study*, 44 Buff. L. Rev. 339, 361 (1996) (finding, based on interviews of 153 jurors who sat in South Carolina capital cases, that “[n]early one-third of the jurors were under the mistaken impression that the law required a death sentence if they found heinousness or dangerousness”); *Study Finds Jurors Confused in Capital Trials*, St. Louis Post-Dispatch, Mar. 26, 1995, at D11 (reporting study released by Indiana University School of Law finding that “nearly 43 percent [of jurors] thought they had to impose a death sentence if the crime was ‘heinous, vile or depraved,’ 32.6 percent believed that death was the required punishment if the evidence proved that the defendant posed a future danger to society, [and that] 42 percent mistakenly thought that the jury had to be unanimous before it could decide that a particular factor, such as a defendant’s mental retardation or lack of a prior criminal record, justified a sentence other than death.”); Joseph L. Hoffmann, *Where’s the Buck? — Juror Misperception of Sentencing Responsibility in Death Penalty Cases*, 70 Ind. L.J. 1137 (1995) (Indiana University Law School study findings).

896. See *supra* p. 3 & nn.34-36.

897. Jost, *supra* note 1 (quoting Kent Scheidegger).

898. See *supra* pp. 3 & n.35, 27-34.

899. See *supra* pp. 27-31.

900. See *Thompson v. Oklahoma*, 487 U.S. 815 (1988).

901. See *supra* pp. 2-3 & nn. 14, 19, 37.

902. See, e.g., *Johnson v. Texas*, 509 U.S. 350 (1993); *Eddings v. Oklahoma*, 455 U.S. 104 (1982).

903. See, e.g., the Wallace case in Appendix D. Wallace was convicted and sentenced to die during a period when he was so mentally disordered that he could not understand the proceedings against him or assist his lawyer. After his conviction and death sentence were reversed on this ground and after he was given treatment and restored to mental competence, he was acquitted at a retrial.

904. For examples, see the Buttrum, Christy, Jones, Jurek, Estelle, Smith, Wade, Wallace, Wilkins, Williams and Williamson cases in Appendix D.

905. James Q. Wilson, What Death Penalty Errors?, N. Y. Times, July 10, 2000, at A19.

906. William S. Sessions, Primary Goal Is Justice, Not Execution, San Antonio Express News, July 19, 2000; see also *Beck v. Alabama*, 447 U.S. 625 (1980) (recognizing that the absence of an appropriate sentencing option at the guilt stage of trial creates the danger of an unwarranted death sentence).

907. See, e.g., *Ramdass v. Angelone*, 530 U.S. 156 (2000) (reporting that after the jury delivered a death verdict, members of the jury contacted by petitioner's counsel "expressed the opinion that a life sentence would have been imposed had they known [defendant] would not be eligible for parole"); *Simmons v. South Carolina*, 512 U.S. 154, 159 & 161 (1994) (plurality opinion) (citing public opinion survey finding that more than 75% of those surveyed indicated that the amount of time a convicted murderer actually would have to spend in prison would be an "extremely important" or a "very important" factor in choosing between life and death, and recognizing that jurors might impose death because they underestimate the legally mandated alternative to death in the case before them); William J. Bowers & Benjamin D. Steiner, *Death by Default: An Empirical Demonstration of False and Forced Choices in Capital Sentencing*, 77 *Tex. L. Rev.* 605, 645-48 (1999); Anthony Paduano & Clive A. Smith, *Deathly Errors: Juror Misperceptions Concerning Parole in the Imposition of the Death Penalty*, 18 *Colum. Hum. Rts. L. Rev.* 211 (1987).

908. See *supra* pp. 352-54.

909. See Ala. Code 13A-5-42 to -47 (1982); Del. Code Ann. Tit. 11 4209(d) (1979); Fla. Stat. Ch. 921.141 (1991); Ind. Code 35-50-2-9(e) (1986).

910. See *supra* p. (Table 18).

911. For a recent critical examination of Alabama's override system, see Taylor Bright, *When a Jury's Choice Doesn't Matter: Judicial Overrides Send Many to Chair; Overrides Viewed as Political Leverage; Critics: Popularity Can Outweigh Justice*, Birmingham Post-Herald, Dec. 13, 2001.

912. See *Harris v. Alabama* 513 U.S. 504, 519-22 (1995) (Stevens, J., dissenting) (stating that a jury verdict "expresses a collective judgment that we may fairly presume to reflect the considered view of the community"); *Spaziano v. Florida*, 468 U.S. 447, 487-489 (1984) (Stevens, J., concurring in part and dissenting in part) ("[T]he jury provides a better link to community values than does a single judge . . . , they are more representative institutions than is the judiciary; they reflect more accurately the composition and experiences of the community as a whole, and inevitably make decisions based on community values more reliably, than can that segment of the community that is selected for service on the bench."); *Witherspoon v. Illinois*, 391 U.S. 510, 519 n.15 (1968) ("The jury also is a significant and reliable objective index of contemporary values."); *Gregg v. Georgia*, 428 U.S. 153, 181 (1976) (opinion of Stewart, Powell, and Stevens, JJ.) (calling the jury "a significant and reliable objective index of contemporary values"). See generally *Harris*, 513 U.S. at 521 (Stevens, J., dissenting) ("Death sentences imposed by judges, especially

against jury recommendations, sever the critical ‘link between contemporary community values and the penal system.’ They result in the execution of defendants whom the community would spare.”) (quoting Witherspoon, 391 U.S. at 519, n. 15).

913. See Bright, *supra* note 911 (investigative press report documenting influence of politics on judicial overrides in Alabama).

914. See *Dobbert v. Florida*, 432 U.S. 282, 296 (1977).

915. Statistics reveal that override systems generally result in more judicial overrides of death sentences than non-death sentences. The relevant data for each of the four states are follows:

Alabama. Between 1982 and July 2000, there were 82 cases in which the judge imposed a death sentence over a jury recommendation of life, compared to 6 cases in which the judge rejected a jury verdict of death. Correspondence from Eva Ansley, Equal Justice Initiative of Alabama, Aug. 13, 2000. See also *Harris v. Alabama*, 513 U.S. at 522 (Stevens, J., dissenting) (discussing statistics collected by the Alabama Prison Project showing that, as of 1995, there were only 5 cases in which an Alabama judge rejected a jury verdict of death, compared to 47 in which an Alabama judge imposed a death sentence over a jury recommendation of life). Since 1995, “nearly all of the [Alabama] overrides [35 of 36] are from life to death—there have only been a handful [actually one] in the other direction.” Correspondence from Ruth Friedman, Equal Justice Initiative of Alabama, July 5, 2000. Alabama has 184 people on death row now, “about a quarter of [whom] got there after a judge overturned a jury life sentence.” *Id.*

Florida. Between 1972 and early 1992, Florida judges imposed death sentences over 134 jury recommendations of life imprisonment, but overrode only about 51 death recommendations. See Michael Radelet and Michael A. Mello, *Death-to-Life Override: Saving the Resources of the Florida Supreme Court*, 20 Fla. St. U. L. Rev. 195, 196 & 210-11 (1992). During this same time period, 20% of those sentenced to die had originally received jury recommendations of life. *Id.* at 196. See also Michael A. Mello, *The Jurisdiction to do Justice: Florida’s Jury Override and the State Constitution*, 18 Fla. St. U. L. Rev. 923, 926 (1991) (reporting that between 1972 and 1988, 1 of every 5 death sentences in Florida involved an override of a jury’s life recommendation). In *Boyett v. State*, 688 So. 2d 308 (Fla. 1996), the Florida Supreme Court discouraged the practice of overriding life recommendations. Since 1995, there have been seven overrides in Florida, all overrides of life verdicts. Correspondence from Michael Radelet, July 5, 2000.

Indiana. In Indiana, between 1980 and 2000, judges used overrides to impose 10 death sentences, compared with 9 life sentences. Correspondence from Monica Foster, Defense Attorney, Indianapolis, Indiana, July 18, 2000. See also *Harris*, 513 U.S. at 522 (Stevens, J., dissenting) (citing data from the Indiana Public Defender Council, reporting that between 1980 and early 1994, judges had used overrides to impose eight death sentences, and four life sentences); *Indiana Death Row Statistics*, avail. at <http://www.clarkprosecutor.org/html/death/rowstats.ghtm>. Note that in Indiana, unlike in other states, all jury recommendations must be unanimous. In other words, both a life and a death determination require the concurrence of all 12 jurors. If the jury is hung even by one vote, the judge sentences as if sentencing had been to the court alone (meaning that all available sentencing options are still on the table). See Ind. Code 35-50-2-9(e) (1986). The relatively even balance of “life” and “death” overrides in Indiana may be a function of the discipline imposed both by the Indiana Supreme Court’s insistence on deference to the jury’s verdict absent clear error, and by the unanimity requirement, which puts judges contemplating override in the position of disagreeing with all

12 jurors.

Delaware. Since 1991, there have been seven judicial overrides of jury death verdicts in Delaware, and no overrides of jury life verdicts. Correspondence from Kevin O’Connell, Office of the Public Defender, Wilmington, Delaware, August 11, 2000. Delaware’s unique resistance among override states to overrides of life recommendations may be attributable both to the Delaware Supreme Court’s insistence on strong deference to jury recommendations of life (as in Florida and Indiana) and to the relatively nonpolitical nature of judicial selection in Delaware, as opposed to the other override states. See Correspondence from Kevin O’Connell, Office of the Public Defender, Wilmington, Delaware, July 5, 2000. See also Fred B. Burnside, Comment: Dying to Get Elected: A Challenge to the Jury Override, 1999 Wis. L. Rev. 1017, 1043 (same) (citing telephone interview with Nan Perillo, Attorney, Delaware Public Defenders Office (Feb. 12, 1999)). One of the drafters of Delaware’s override statute is reported to have said: “We would have been substantially more reticent to change to judicial sentencing if judges were elected.” Burnside, *supra* at 1043 (citing telephone interview with Steve Wood, Chief Prosecutor, New Castle County, Delaware (Feb. 16, 1999)).

916. As the data in note 915 above reveal, the disproportionate use of jury overrides to impose death sentences (1) is extremely pronounced in states that give trial judges essentially unfettered discretion to overturn jury votes for life the case for any or no reason at all, and (2) is stronger in states where judges are subject to direct election. In the first regard, compare Florida before 1995 and Alabama throughout the modern death-sentencing era (when both states gave trial judges relatively free reign to override jury verdicts), to Florida since 1995 and to Indiana and Delaware (which have relatively strict standards governing when a judge may override a jury verdict).

In the second regard, compare override results in Alabama, Florida and Indiana, where judges face election, and overrides of life sentences are generally far more common than overrides of death sentences, to Delaware, where judges are not elected, and the few overrides that occur are all in the direction of life; no Delaware judge has ever overridden a jury’s majority verdict for life. Even more specifically, as our analyses would predict, there “appears to be a sliding scale of jury override use depending not only on [whether] elections [are used to select judges], but the type of election.” Burnside, *supra* note 915, at 1049. Judges in Alabama, which has partisan elections and places no constraints on a judge’s discretion to override life verdicts, override almost 10 jury life-sentence recommendations for every vetoed death sentence recommendation; Florida, a state with non-partisan retention elections, has an approximate 3-to-1 ratio in favor of overriding juries’ recommendations for life sentences; and Indiana, also with non-partisan retention elections, has a ratio close to 1-to-1 that only slightly favors overrides of juries’ recommendations for a life sentence. *Id.* at 1043.

The Table below compares the four override states based on the percentage of judge overrides that imposed death sentences in cases in which jurors voted for life (as opposed to overrides imposing life sentences in cases in which the jury imposed death), and on the states’ scores on our two indexes of the

political pressure judicial selection techniques place on judges (with higher scores indicating more political pressure). As our analyses would predict, judges in states where judicial selection methods make them more politically vulnerable have a greater propensity to use overrides to impose additional death verdicts.

State	% Judge Overrides Imposing Death	Score on 1st Political Pressure Index	Score on 2nd Political Pressure Index
Alabama, 1982-2000	93% (82/88)	8	7
Delaware, 1991-2000	0% (0/7)	3	3
Florida, 1972-2000*	73% (141/192)	7	7
Indiana, 1980-2000	53% (10/19)	8	5

In Alabama, there also “is a statistically significant correlation between [the likelihood of] judicial override [in favor of death] and election years in most of the counties where these overrides take place.” Symposium, *Politics and the Death Penalty: Can Rational Discourse and Due Process Survive the Perceived Political Pressure?*, 21 *Fordham Urb. L.J.* 239, 256 (1994).

917. The Florida experience documents the risk of error in override cases and the burden excessive overrides place on appellate courts. See Gerald B. Cope, Jr., *Discretionary Review of the Decisions of Intermediate Appellate Courts: A Comparison of Florida’s System with those of the other States and the Federal System*, 45 *Fla. L. Rev.* 21, 99-100 (1993) (finding that jury recommendations of life constitute the vast majority of override cases, and that the Florida Supreme Court usually reverses death sentences imposed by judges contrary to the life recommendation of a jury, either based on a conclusion that the override was improper or due to other errors in the case; also reporting that in 1991, the reversal rate for Florida capital verdicts imposed following overrides of life verdicts was 91 %, and concluding that based on these high reversal rates, eliminating override cases would reduce the death penalty workload of the Florida Supreme Court by 21% and the court’s *overall* workload by 6 to 8%). See also Gary Caldwell, *Capital Crime Decisions: 1992 Survey of Florida Law*, 17 *Nova L. Rev.* 31, 64 n. 261 (1992) (reporting that from 1986 through 1992, the Florida Supreme Court upheld death sentences in only seven cases where judges overrode life verdicts).

918. See *supra* pp. 321-21, 336, 341, 385.

919. See *supra* pp. 22-23, 323-24 & n.647.

920. See, e.g., . Tennessee article on forgone review. John Shipman

921. See *supra* pp. 394-95.

922. *State Law Seeks to Provide Strong Defense; Most Capital Cases Are Handled Well, But Examples of Inadequate Representation Show Lapses in the System*, *South Bend Trib.*, Oct. 22, 2001 (quoting Professor Karlson). According to Karlson, financial savings are not the only reason to spend more time and energy assuring the accuracy of capital verdicts from the outset: “While prosecutors . . . complain about [spending money on capital defense], Karlson said it is imperative to prevent an innocent person from being executed. Such a mistake, he reasoned would lead to fewer executions of defendants deserving to die.” *Id.*

923. See, e.g., sources catalogued in 100 Colum. L. Rev., *supra* note 123, at 2078-94. See also Lise Olsen, One Killer, Two Standards, *Seattle Post-Intelligencer*, August 7, 2001 (noting, in examining potential causes of problems in Washington state capital cases, that “[p]rosecutors may seek the death penalty before getting a full picture” because “Washington has no standardized process to guide prosecutors in reviewing capital cases,” leaving them free to seek “death sentences quickly, after brief conversations with defense attorneys,” rather than following the practice elsewhere of using “trained teams to review lengthy documents detailing the defendant’s upbringing, criminal history and mental health”).

924. See, e.g., sources collected in 100 Colum. L. Rev., *supra* note 123, at 2078-94.

925. See, e.g., sources collected in *id.* at 2144-45.

926. Lise Olsen, 1 Killer, 2 Standards, Sidebar: Solutions, *Seattle Post-Intelligencer*, Aug. 7, 2001 (listing reforms reportedly proposed by State’s Attorney Birkett).

927. See *supra* p. 6.

928. See *supra* pp. 40-41.

929. See *supra* p. 71.

930. The standard allows police and prosecutors to withhold evidence of innocence or mitigation and evidence undermining the state’s case of guilt or aggravation, if in their judgment there is no “reasonable probability” that but for their suppression of the evidence, “the outcome of the trial”—which has not yet taken place—“would be different.” See, e.g., *Strickler v. Greene*, 527 U.S. 263 (1999).

931. See *supra* pp. 319-24, 349-50.

932. See *supra* note 930.

933. See *supra* pp. 405-06; *infra* p. 418.

934. See *supra* pp. 354-56.

935. See *supra* pp. 16 & n.100, 194, 198-99, 212, 217-19, 227, 230, 236, 240 & n.486, 257, 382-84.

936. See *Ring v. Arizona*, 122 S. Ct. 865 (2002) (granting certiorari to resolve apparent conflict between *Jones v. United States*, 526 U.S. 227 (1999) and *Apprendi v. New Jersey*, 530 U.S. 466 (2000), on the one hand, and *Walton v. Arizona*, 497 U.S. 639 (1990), on the other hand, over whether the Constitution requires a jury finding of the prerequisites for a capital as opposed to a lesser sentence). On the greater insulation of jurors than judges from political pressure, see *supra* pp. 405-06 & nn.911-16.

937. On Indiana, see Norman Lefstein, Reform of Defense Representation in Capital Cases: the Indiana Experience and its Implications for the Nation, 29 Ind. L. Rev. 495, 496-504, 506-07, 509-12, 518-26, 533 (1996) (discussing effect of Indiana’s early 1990s adoption of legislation making state funds available to local jurisdictions that satisfy a state commission’s guidelines for appointment of qualified counsel in capital cases, and the commission’s incorporation within its guidelines of a state supreme court rule (1) requiring the appointment of two lawyers in capital cases with recent extensive training in capital defense and with, respectively, at least five and three years criminal litigation experience that includes at least five and three

felony jury trials, (2) disqualifying lawyers with excessive workloads, (3) setting minimum hourly rates that are relatively generous, though they remain well below the rates prevailing among retained attorneys, and (4) assuring “adequate funds for investigative, expert, and other services necessary to prepare and present an adequate defense at every stage of the proceeding, including the sentencing phase”; noting that prosecutorial requests for death sentences dropped from 23/year in the two years before, to 10/year in the three years after, the adoption of these reforms and reporting agreement among state capital prosecutors and defense counsel interviewed before the data were available that the reforms (1) had improved the quality of capital defense lawyering in the state, especially by increasing the use of expert witnesses at the mitigation phase, (2) had attracted more and better defense lawyers to the work, (3) had probably generated better police and prosecutorial preparation and decreased the likelihood that the resulting (smaller number of) capital judgments would be reversed on appeal, and (4) in the words of prosecutors “ha[d] ‘definitely put a damper on [their] asking for the death penalty,’” “‘put some economic judgment’ into the decision-making about whether to seek the death penalty” and made them “‘think two or three times’ before filing a death penalty request” not only because of the greater cost of trying cases but also because of the increased “risk [of] losing”; comparing the Indiana experience to that of Ohio, which adopted similar reforms but compensates defense lawyers at only two-thirds the rate in Indiana and provides funds for expert witnesses and mitigation specialists far less frequently than in Indiana and has experienced smaller declines in the death-sentencing rate; concluding that there is “strong[]” reason to believe that the “ability of defense counsel, the cost of the prosecution [which in turn is affected by the quality and resources of defense counsel], and the burden on the prosecutor’s staff” affect prosecutorial charging decisions in capital cases). See also *Ten Who Have Faced the Death Penalty in Indiana Have Been Found Not Guilty*, *Evansville Courier & Press*, Oct. 23, 2001 (“There have been few cases resulting in acquittals in recent years because prosecutors are exerting more caution in seeking the death penalty and thus filing it less often, both defenders and prosecutors agree.”).

On Oklahoma, see *supra* note 38.

938. See, e.g., *supra* p. 32 & n.143.

939. See *supra* pp. 40-42.

940. See *supra* pp. 199-200, 370-72.

941. See, e.g., Paul Barrett, *Lawyer’s Fast Work on Death Cases Raises Doubts About System*, *Wall St. J.*, Sept. 7, 1994, at A1; Vivian Berger, *The Chiropractor as Brain Surgeon: Defense Lawyering in Capital Cases*, 18 *N.Y.U. L. Rev. L. & Soc. Change* 245 (1990-91); Stephen B. Bright, *Counsel for the Poor: The Death Sentence Not for the Worst Crime but for the Worst Lawyer*, 103 *Yale L.J.* 1835 (1994); Marcia Coyle, et al., *Fatal Defense: Trial and Error in the Nation’s Death Belt*, *Nat’l L.J.*, June 11, 1990, at 30; Paul Duggan, *Attorneys’ Ineptitude Doesn’t Halt Executions*, *Wash. Post*, May 12, 2000, at A1; Ruth E. Friedman & Bryan A. Stevenson, *Solving Alabama’s Capital Defense Problems: It’s a Dollars and Sense Thing*, 44 *Ala. L. Rev.* 1 (1992); Gary Goodpaster, *The Trial for Life: Effective Assistance of Counsel in Death Penalty Cases*, 58 *N.Y.U. L. Rev.* 299 (1983); Dirk Johnson, *Shoddy Defense by Lawyers Puts Innocents on Death Row*, *N.Y. Times*, Feb. 5, 2000, at A1; Anthony Paduano & Clive Stafford Smith, *The Unconscionability of Sub-Minimum Wages Paid Appointed Counsel in Capital Cases*, 43 *Rutgers L. Rev.* 281 (1991); Albert v. Vreeland, II, Note, *Statutory Fee Limitations and Ineffective Assistance of Counsel in Capital Litigation*, 90 *Mich. L. Rev.* 626 (1991); other sources cited *infra* notes 946-61.

942. Sources suggesting a link between being poor quality representation and an increased probability of a death sentence include, e.g., Associated Press, *Ohio County Paying Defense Counsel the Least Sends the Most to Death Row*, Feb. 3, 2000 (“Hamilton County [Cincinnati], which sends more people to death row

than any other county in [Ohio], pays public defenders less to represent those people than” all but one of Ohio’s other 88 counties; its hourly “rate of \$30 an hour—57% below the state average—is the same whether the case is a minor felony such as theft or a death penalty case”); Alan Berlow, *Lethal Injustice*, *The American Prospect*, Mar. 27-Apr. 10, 2000 (describing a study done for the Texas Judicial Council in the mid-1980s, which found that a defendant’s chances of being convicted of murder in the state were 28% higher if his or her attorney was court-assigned and that 79% of capital defendants with appointed counsel were sentenced to death, compared to 55% of capital defendants with retained lawyers); William J. Bowers, *The Pervasiveness of Arbitrariness and Discrimination under Post-Furman Capital Statutes*, 74 *J. Crim. L. & Criminology* 1067, 1075-77 (1983) (describing interviews with Florida prosecutors revealing that the quality of the defense lawyer is an important factor in their decision whether to accept a noncapital plea in cases charged capitally); *Dead Man Walking Out*, *The Economist*, June 10, 2000, at 21 (describing a 1999 study finding that criminal defendants in Houston with court-appointed lawyers were twice as likely to go to prison as those who were able to hire their own counsel); Michael L. Radelet, *Rejecting the Jury: The Imposition of the Death Penalty in Florida*, 18 *U.C. Davis L. Rev.* 1409, 1414-15 (1985) (describing a Florida study, which found that 40% of defendants who had private counsel received life sentences at capital trials, compared to 23% of capital defendants with public defenders or appointed attorneys); Tina Rosenberg, *Deadliest D.A.*, *N.Y. Times*, July 16 (1995), Magazine, at 46, 50 (comparing capital representation by appointed lawyers who handle close to 80% of Philadelphia capital cases for a flat fee of \$1700 plus \$400 for each day in court and \$300 for an investigator, with an average cost to the county in 1995 of \$2700 per capital case, to representation by the local public defender office in the one in five cases in which that elite office is permitted to represent capital defendants, which provides two attorneys, a mitigation specialist, an investigator, and access to a staff psychiatrist and expert witnesses; although a large proportion of Pennsylvania’s death row is made up of Philadelphia defendants who were represented at trial by appointed attorneys, not a single defendant represented by the elite public defender office received the death penalty in 1993-95 [or, in fact, from 1993 to early 2000]). See also Editorial, *Rising Doubts on Death Penalty, USA Today*, Dec. 22, 1999, at 17A (“You are more likely to receive the death penalty if you are . . . poor.”).

943. A “common thread” in the cases of innocents freed from death row is “poorly financed, often incompetent defense lawyers who failed to uncover and present key evidence.” Dirk Johnson, *Shoddy Defense by Lawyers Puts Innocents on Death Row*, *N.Y. Times*, Feb. 5, 2000. See, e.g., Ken Armstrong & Steve Mills, *Inept Defenses Cloud Verdicts*, *Chi. Trib.*, Nov. 15, 1999 (four of 13 former death row inmates who were exonerated and released were represented at trial by lawyers who have had their licenses suspended or withdrawn); Benjamin Weiser, *Judge Overturns Verdict in 1980 Murder*, *N.Y. Times*, Nov. 12, 1999, at B4. A recent study revealed that ineffective assistance of counsel was a contributing factor in 27% of the American cases in which wrongful convictions were uncovered using DNA evidence. See Vivian Berger, *Actual Innocence: Five Days to Execution, and Other Dispatches from the Wrongly Convicted* (Book Review), *N.Y. L.J.*, May 1, 2000.

944. Unlike 9 out of 10 non-capital charges, which are settled after a few hours of plea bargaining, most capital cases require full investigations and a full-blown trial:

- Capital investigations and trials are more complicated. In the usual non-capital case, the chief evidence against the defendant is either the victim’s testimony (in non-homicide cases) or testimony by witnesses who know both the defendant and the victim (in most non-capital homicide cases, involving domestic disputes or fights between acquaintances). Capital murders, by contrast, usually involve encounters between strangers, where the only eyewitness is the deceased victim, and most of the evidence is circumstantial and much of it requires complex and costly expert assistance.

- Capital trials are in fact “two trials (usually two *jury* trials) in one.” The first trial determines whether a murder of a given degree occurred and whether the defendant committed it. Often crucial are defendant’s mental state and status, requiring additional experts and medical examinations. Then a second trial determines whether the killing was capitally aggravated and, if so, whether the aggravating factors outweigh the mitigating ones. This second trial usually requires a factual investigation at least as involved as the first trial, and even more extensive expert assistance. In most non-capital cases, by contrast, guilt is resolved without trial by a guilty plea, and sentencing is an informal process before a judge where few legal rules apply and most of the evidence is provided to the judge administratively by a court official in a “pre-sentence report.”
- Both phases of capital trials, and especially capital-sentencing proceedings, are governed by highly complex sets of specialized statutory and state and federal constitutional rules that have no analogue in non-capital cases.
- Even jury-selection is more costly and complex in capital than in noncapital trials, because “death qualification” voir dire (which occurs only in capital cases) substantially extends the length of jury selection, and is the subject of a complex legal jurisprudence.

For documentation of these points, see the authorities cited *supra* note 941.

945. See sources cited *infra* note 947.

946. See, e.g., American Bar Association, *Toward a More Just and Effective System of Review in State Death Penalty Cases*, 40 Am. U. L. Rev. 1, 16 & 69 (1990) (detailed study concluding that “inadequate compensation of counsel at trial” is one of the “principal failings of the capital punishment systems in the states today”); *supra* note 942 (Hamilton County, Ohio).

947. See, e.g., Coyle et al., *supra* note 941, at 30 (“Wholly unrealistic statutory fee limits on defense representation—such as Mississippi’s flat, unwaivable \$1,000 cap [on compensation for capital defense lawyers], equivalent to a fee of about \$5 per hour for many lawyers [a provision that was still in effect as late as March 2000, see Sara Rimer, *Questions of Death Row Justice for Poor People in Alabama*, N.Y. Times, Mar. 1, 2000, at A16—act as disincentives to thorough trial investigation and preparation.”); Friedman & Stevenson, *supra* note 941 (criticizing Alabama’s built-in monetary disincentive—maximum compensation of \$20 per hour for any work done out of court and \$40 per hour for in-court activity, with a \$1000 reimbursement cap—against thorough representation at the trial level); Johnson, *supra* note 943 (documenting that capital defendants “are often represented by lawyers who are paid a few thousand dollars, or less, and spend only two days on a case” and that a proper defense in a death penalty case takes months of research and costs \$250,000 or more); Jeb Phillips, *Justice at 50 cents an Hour: Defending Death Row Case Drove Lawyer into Bankruptcy*, Birmingham Post-Herald, Dec. 14, 2001; Rosenberg, *supra* note 942, at 21, 46, 50 (comparing capital representation by appointed lawyers who handle close to 80 percent of Philadelphia capital cases for a flat fee of \$1700 plus \$400 for each day in court and \$300 for an investigator, with an average cost to the county in 1995 of \$2700 per capital case, to the rare representation by a retained lawyer for whom the going rate in Philadelphia is \$50,000 per case); Stan Swofford, *A Reasonable Doubt: Are There Innocent People on North Carolina’s Death Row*, Greensboro News & Rec., Aug. 6, 2000, at A1 (comparing North Carolina’s \$85 per hour cap on compensation for defense attorneys appointed to represent indigent capital defendants, to the going rate of \$200 or more per hour for such representation by experienced retained criminal defense lawyers in the state).

948. See Jonathan Alter, *The Death Penalty on Trial: Special Report*, Newsweek, June 12, 2000 (“Texas provides only \$2,500 for investigators and expert witnesses in death penalty cases (enough for one day’s work, if that).”).

949. Bruce A. Green, *Lethal Fiction: The Meaning of “Counsel” in the Sixth Amendment*, 78 *Iowa L. Rev.* 433, 491-92 (1993). See also Rimer, *supra* note 947 (quoting an Alabama lawyer who was appointed to represent a capital defendant at trial at a maximum \$20 per hour for out of court work, with a \$1000 compensation cap, and who consequently ended up being reimbursed at about \$5 an hour for the work he put in, vowing that “I will go to jail before I handle another capital case”).

950. James Coleman, *Testimony in Support of Title II of the Innocence Protection Act of 2000*, United States House of Representatives, June 21, 2000. In a recent report, the American Bar Association described a variety of disturbing techniques that under-funding has forced state appointing officials to use to provide defense representation in capital cases, including patronage selections off a general list of all local attorneys, regardless of capital, or even criminal, experience; contract systems under which all cases over a particular period go to the lowest bidder (with a flat fee bid covering all experts and other expenses), including complex and unanticipated capital cases that suddenly appear on the county’s docket; reimbursement schemes that limit lawyers to, e.g., \$2500 for the entire representation “plus \$50 for each motion . . . filed *up to five motions*—with the result that the number of motions filed in almost every case is exactly five—or \$1000, including expenses for expert and investigative assistance; or what amounted to “\$15 to \$20 per hour” and \$11.84 per hour” to represent two innocent men who were sentenced to die but were eventually released for lack of evidence of guilt. Randall Coyne & Lyn Entzeroth, *Report Regarding Implementation of the American Bar Association’s Recommendations and Resolutions Concerning the Death Penalty*, 4 *Geo. J.F.P.* 253 (1996) (hereinafter “ABA Report”). For other sources who have comprehensively documented the limited resources available for capital defense, see, e.g., Coyle, *supra* note 941; Lefstein, *supra* note 937; Joe Margulies, *Resource Deprivation and the Right to Counsel*, 80 *J. Crim. L. & Criminology* 673 (1989); Michael D. Moore, *Analysis of State Indigent Defense Systems and their Application to Death-Eligible Defendants*, 37 *Wm. & Mary L. Rev.* 1617 (1996); Lise Olsen, *The Death Penalty: Uncertain Justice*, *Seattle Post-Intelligencer*, Aug. 6, 2001 (“Judges help create the problem [of “inept” capital defense lawyers] by appointing inexperienced local lawyers to capital cases instead of those recommended by the state. Counties often pay these defenders so poorly that they cut corners, putting convictions and sentences on shaky legal ground.”); Douglas W. Vick, *Poorhouse Justice: Underfunded Indigent Defense Services and Arbitrary Death Sentences*, 43 *Buff. L. Rev.* 329 (1995).

951. See, e.g., Barrett, *supra* note 941; *Lawyer’s Fast Work on Death Cases Raises Doubts About System*, *Wall St. J.*, Sept. 7, 1994; Mary Flood, *What Price Justice? Gary Graham Case Fuels Debate Over Appointed Attorneys*, *Hous. Chron.* July 1, 2000, at A1 (stating that in some Texas counties in the 1970s and 1980s, “courthouse appointment lists were often an informal string of each judge’s friends and campaign contributors; some were competent and trained, others were not[; t]here were no requirements for experience, no classes, no tests”).

952. See, e.g., Armstrong & Mills, *Inept Defenses*, *supra* note 943 (finding that 33 defendants sentenced to die in Illinois were represented by a lawyer who has been disbarred or suspended); Berlow, *supra* note 942, at 82 (discussing Kentucky study showing that 25 percent of state’s death row inmates had been represented by attorneys who had since been disbarred or had resigned to avoid disbarment, and Louisiana study showing that lawyers of inmates executed in state had bar discipline rate 68 percent higher than bar members as a whole); Liz Chandler, *Lawyers, Inadequate Defense Cited in a Third of Death Case Reversals*, *Charlotte Observer*, Sept. 11, 2000, at 1A (“Since 1977, when the Carolinas restored capital punishment, at least 15

death verdicts have been overturned because of poor lawyering at trial. And at least 16 other death row inmates—including three who were executed—were represented by lawyers who have been disbarred or disciplined for unethical or criminal conduct”); *Defense Called Lacking for Death Row Indigents*, Dallas Morning News, Sept. 10, 2000, at 1A (reporting, based on examination of 461 Texas capital cases, “that nearly one in four condemned inmates has been represented at trial or on appeal by court-appointed attorneys who have been disciplined for professional misconduct at some point in their careers,” in “about half” of which cases, “the misconduct occurred before the attorney was appointed to handle the capital case”); Lise Olsen, *The Death Penalty: Uncertain Justice*, Seattle Post-Intelligencer, August 6, 2001 (“Washington State authorizes the death penalty, but does little to ensure that defendants are represented equally. Nearly 1/5 of the men to face execution were represented by lawyers who had been, or were later, suspended or arrested. In the past 20 years, 84 people in this state have faced the possibility of capital punishment. Despite the gravity of the cases, some defendants were given inexperienced or inept counsel.”).

953. See, e.g., Liz Chandler, *With Lives on Line, Attorney Turned to Drink*, Charlotte Observer, Sept. 11, 2000, at 6A; Dan Malone & Steve McGonigle, *Questions of Competence Arise in Death Row Appeal: Lawyer With History of Problems Defends Handling of Case*, Dallas Morning News, Sept. 11, 2000, at 1A.

954. See, e.g., Rimer, *supra* note 947 (discussing capital defense situation in Alabama, which “has no state-wide public defender system” and relies on “[c]ourt-appointed capital defense lawyers [who] are paid so little that many lawyers refuse the work[,] . . . leaving many of those charged with capital offenses with inadequate representation” by “lawyers who fail to do the most fundamental tasks, like investigating the crime and their clients’ backgrounds and presenting closing arguments”); Henry Weinstein, *a Sleeping Lawyer and a Ticket to Death Row*, L.A. Times, July 15, 2000, at A1.

955. See, e.g., Michele McQueen & Ted Koppel, *Crime and Punishment, Poor Counsel*, Nightline, ABC News, Feb. 3, 2000; Margulies, *supra* note 950, at 677-82 (citing burdensome workloads as a result of underfunding and under-staffing as the “single greatest obstacle to effective representation” in capital and other cases). See also Stephen Bright, *Neither Equal Nor Just*, 1997 Ann. Survey of Am. L. 783, 817 (1997) (documenting defense attorney caseloads in particular states).

956. See, e.g., Rimer, *supra* note 947 (blaming low maximum hourly rates, and \$1000 fee cap, for fact that (1) appointed capital defense lawyers in Alabama “often” limit themselves to 50 hours or less on each capital case, “even though adequate preparation . . . should take 500 to 1,000 hours,” and (2) lawyers who attempt to put in the necessary hours end up working “at a rate of about \$5 an hour”).

957. See, e.g., Jeanne Cummings, *Bad Lawyers Tip the Scales of Justice Toward Death Row*, Atlanta J. Const., Apr. 1, 1990, at A1; Paul Duggan, *in Texas, Defense Lapses Fail to Halt Executions*, Wash. Post, May 12, 2000, at A1 (reporting on instances in which defense lawyers in capital trials have slept through key testimony, failed to file crucial legal papers correctly or on time, or had been cited for professional misconduct repeatedly in their careers); Editorial: *Rush to Death*, St. Petersburg Times, Feb. 10, 2000, at 16A (discussing Florida case in which a “young, inexperienced lawyer who knew little about presenting a capital defense,” had his fee capped at \$2500 for pretrial and trial proceedings, “was paid the equivalent of \$13 an hour” for what little he did, and failed to present any witnesses to describe his client’s “long history of mental illness” because (as the lawyer later testified in court) “he could not afford to call witnesses”); Bill Freivogel, *Bad Lawyers: Rx for Death*, St. Louis Post-Dispatch, May 4, 2000; ; Johnson, *supra* note 943 (citing accounts from around the nation of defense lawyers who slept through trials, came to court drunk, and had never before tried a criminal case).

958. Coopers & Lybrand Consulting, Report on Costs and Recommendations for the Control of Costs of the Defender Services Program, Jan.28, 1998 (reporting on interviews with a series of judges) (cited in Statement of Judge William Terrell Hodges, Chairman, Executive Committee, Judicial Conference of the United States, June 11, 1998); see also Editorial, No Money for Justice: State Will Help Prosecute Death-Penalty Cases but Falls Short in Ensuring a Fair Defense, *Phil. Inquirer*, Mar. 8. 2000, at A22 (discussing Pennsylvania Legislature's defeat of bill to increase money for defense of capital case in same session in which it appropriated additional funds to assist local prosecutors in such cases).

The problems described here extend beyond cases of appointed counsel to ones in which impecunious defendants, who are fearful of the consequences of accepting an appointed attorney, retain private counsel at fire-sale prices, "often settl[ing], in desperation, for any lawyer who will take a case, regardless of reputation." Johnson, *supra* note 943.

959. Testimony of Professor Lawrence C. Marshall, Director, Northwestern University Center on Wrongful Convictions, to Illinois legislative committee, Sept. 15, 1999, at 10. See also Armstrong & Mills, Inept Defenses, *supra* note 943 (reporting that appointed lawyers "in Illinois capital cases have included a tax lawyer who had never before tried a case, an attorney just two years out of law school and an attorney just 10 days off a suspension for incompetence and dishonesty"; the assigned lawyer two years out of law school handled the case himself, though he had never before tried a murder case, was carrying 100 other appointed criminal cases, and was denied funds for an investigator and experts (by contrast, "the local prosecutor received help trying the case from a lawyer in the Illinois attorney general's office, a common [practice]").

960. *Martinez-Macias v. Collins*, 810 F. Supp. 782, 788-813 (W.D. Tex. 1991), *aff'd*, 949 F.2d 1067 (5th Cir. 1992).

961. *Martinez-Macias v. Collins*, 949 F.2d at 1067 (emphasis added). For another example, see Olsen, Defense for Capital Crimes Often Done on the Cheap, *Seattle Post-Intelligencer*, Aug. 8, 2001 (discussing representation of James Leroy Brett by a contract attorney in Clark County, Washington, which caps pretrial compensation in capital cases at \$12,500, or between "\$12.50 to \$25 an hour, based on the 500 to 1,000 hours of pretrial work experts say the cases usually require"; Brett's death sentence was subsequently overturned because of egregious ineffective assistance of counsel in failing to prepare the case properly, costing the state \$254,889 in appellate expenses).

962. Here again, we agree with the President of the Association of Government Attorneys in Capital Litigation, Joe Birkett, that states and counties should "[i]nsist on a quality defense" in capital cases, because "[s]potting problems early saves time and money on appeals years later." Olsen, *Solutions*, *supra* note 926. See also Editorial, *Seattle Post-Intelligencer*, Aug. 12, 2001 ("A system of death-penalty defense on the cheap not only disadvantages the person with the most to lose, it costs the public in the long run. Front-loading capital cases with experienced, decently paid attorneys with adequate budgets for pre-trial discovery helps ensure that fewer verdicts will be overturned because of bad lawyering. Those years of costly appeals also delay closure for victims of capital crimes and frustrate the public will.").

963. See *supra* pp. 194-95, 202, 258-59, 263, 372-76.

964. See *supra* pp. 65-67 & Figure 5, 254-46.

965. See Gallegos, *supra* note 44:

While he still supports the death penalty, [New Mexico Governor Gary] Johnson said he no

longer supports the idea of a two-year cap on the time death-row inmates can appeal their sentences.

Johnson first introduced the idea of limiting death-row appeals in 1997 as part of a broader package of crime-fighting legislation. The legislation, meant to ensure “swift and sure” punishment, was modeled after similar laws in Texas and Florida.

* * * * *

But Johnson said he now believes limiting death-row appeals would probably lead to innocent people being executed. That is why he now opposes the concept, said Bob Stranahan, one of the governor's attorneys.

“I think his (Johnson's) worry is just that although the appeals process is slow and does stop the thing from being swift and sure, that without it you have the worry in place that you're going to not catch something through the appellate process,” Stranahan said.

Johnson pointed to a case in the 1970s in which four members of a motorcycle gang were wrongly accused and convicted for murdering a University of New Mexico student. The four men were later released from death row after another man admitted to the murder.

“That case proved that had I passed my law, someone like that might have been executed,” Johnson said. “That scared me to death.

“I’ve had several things happen to me causing me to reexamine my position.”

966. See *supra* pp. 377-82.

967. See Editorial, *Ryan’s Tough Call on Executions*, Chi. Trib., Aug. 28, 2001 (endorsing veto of legislation adding new aggravating circumstance to Illinois capital statute for “gang-related killings,” in part because “[a]dding gang-related murder as a so-called ‘aggravating factor’ in sentencing would lengthen Illinois’ long list of what makes a person eligible for the death penalty and invite more arbitrariness in the judicial system.”; noting that “[w]hen Illinois restored capital punishment, it started out with a handful of factors that qualified a defendant” but “[n]ow there are 20 such factors”; recommending that the state “ought to narrow the eligibility list to something resembling the original”). See also Christi Parsons & Ray Long, *House Won’t Push Death Penalty Bill*, Chi. Trib., Nov. 16, 2001 (“House Speaker Michael Madigan said Thursday there will be no attempt to override Gov. George Ryan’s veto of a bill that would have made gang activity one of the aggravating factors that qualifies convicted murderers for the death penalty. . . . ‘I don’t think there’s any question the administration of the death penalty has not been well-done,’ Madigan said.”).

968. See Olsen, *Solutions*, *supra* note 926 (listing Birkett’s reform proposals).

969. See *supra* pp. 379-81.

970. See *supra* pp. 14-35.

971. See *supra* pp. 79-80 & Figure 9.