GETTING TO DEATH: FAIRNESS AND EFFICIENCY IN THE PROCESSING AND CONCLUSION OF DEATH PENALTY CASES AFTER FURMAN

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EXECUTIVE SUMMARY

There is growing concern 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. Questions of "how fast (or slow)?" and "how fair?" persistently confront the administration of the death penalty. Previous studies showed 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 (Liebman, Fagan and West, 2000). These appeals took nearly a decade on average to complete judicial examination. Moreover, available data on the reasons for reversal for state post-conviction and federal habeas stages of review show that more than three in four (76%) were the result of incompetent defense lawyers, suppression of exculpatory evidence or other professional misconduct by police and prosecutors, invalid jury instructions leading to misinformed jurors, or biased judges and jurors. Half of those reversals tainted the verdict finding the defendant guilty of an aggravated capital murder 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 (Liebman, Fagan and West, 2000).

This study addresses two questions: First, we ask why does the death penalty system make so many mistakes? Second, we ask how might these mistakes be prevented, if at all? We hypothesized that the more a jurisdiction uses the death penalty relative to homicide rates, sentences will be found legally invalid and overturned.

METHODS

To address these questions, we computed error rates within states from 1973, when capital punishment was reinstated in the U.S. following the Supreme Court decision in *Furman v. Georgia* (408 U.S. 238 (1972)), through 1995. We framed this period so that cases originating before 1995 would have sufficient time to complete their cycle of judicial examination. We reviewed every death sentence imposed during the study period under a valid post-*Furman* capital statute across three stages of appeal: direct appeal in state court, state post-conviction review in the state's highest court, and federal habeas corpus appeals. For each state and sentence year, we computed reversal rates at each stage, and a composite error rate across the three stages within years and again over time within states.

Potential explanatory factors were identified in several domains, including the supply of cases for death sentencing, characteristics of the criminal justice system, social and demographic characteristics of the states, and the social and political contexts in which death sentences are produced and reviewed. For federal habeas cases, we recorded extensive information on characteristics of the offender, victim, murder, state and fedral review procedures, federal lawyers and judges, and claims for relief.

We employed mulitivariate statistical methods to identify factors that predict where and when death verdicts are more likely to be reversed based on serious error. To validate the analyses and ensure that results are driven by relationships in the data instead of statistical methods, we began with a preferred model, then used alternate statistical procedures with contrasting assumptions about the arrangement of capital reversals and

reversal rates. We analyzed reversals at each separate review stage and at all three stages combined. We used both over-dispersed Poisson logarithmic regression methods and over-dispersed binomial regression methods 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 and case level. Finally, we checked for consistency of results across analyses to determine which factors and sets of significant factors were the most robust and warranted the most confidence.

FINDINGS

We conclude that heavy and indiscriminate use of the death penalty creates a significantly higher risk that reversible 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. We also conclude that that the conditions pressuring 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. We detail these findings below.

Error Rates and the Overproduction of Death Sentences

1. *The higher the rate at which a state 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 central 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, the predicted increase in reversal rates is six-fold—to about 80%.

2. 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 invites unreliability and error.

3. Comparisons of particular states' capital-sentencing and capital-error rates illustrate the strong relationship between frequent death sentencing and error. 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 social and structural 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.

1. The closer the homicide risk to whites in a state comes to equaling or surpassing the homicide risk to blacks, the higher the error rate . Controlling for other social and legal factors, reversal rates are twice as high where homicides are most heavily concentrated on white victims compared to blacks.

2. 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.

3. 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 substantial cost in error and delay.

4. 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.

Other Systemic Factors Affecting Error Rates

1. 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 seem to 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.

2. 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.

3. 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.

4. 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 sometimes slip through. Case studies show that in some cases, judges recognize that proceedings were marred by error but affirmed anyway because of stringent rules limiting reversals.[i]

DISCUSSION

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 adversely affected by so much error, and as a result risks executing the innocent. States that impose death sentences at a lower rate per homicide produce fewer decisions that are flawed by serious error and are reversed. 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 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.

POLICY OPTIONS

High rates of capital error are costly. Many of the factors that produce error are not easily addressed through policy (*e.g.*, the complex interaction of a state's racial make-up, its welfare burden, the distribution of homicides among white and black victims, and the efficacy of its law enforcement policies). Indirect remedies may be unreliable, and may be perceived as infringing on the prerogatives and discretion of officials who in the past have broadened their authority to extend the death penalty to cases that are not highly aggravated. As a result, some states may conclude that the only answer to chronic capital error is to limit its to the very small number of prospective offenses where there is something approaching a social consensus that only the death penalty will do. Other states may decide that error can be reduced only if they stop using the death penalty at all.

For other states, targeted reforms based upon careful study of local conditions might be a preferable means of achieving the central goal of reducing error in death sentences by limiting the death penalty to "the worst of the worst" cases—that is, to defendants who can be shown without doubt to have committed an egregiously aggravated murder in the absence of extenuating factors. Ten reforms that might help accomplish this goal are:

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i. See case studies in James S. Liebman et al., A Broken System, Part II: Why There is So Much Capital Error and What Can Be Done About It, http://law.columbia.edu/brokensystem2, at 25-36.

- 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.

INTRODUCTION

In the 1960s and 1970s, Americans debated whether their criminal justice system could administer the death penalty fairly, leading half of them to oppose it. In the succeeding decade, federal courts implemented reforms to make the penalty fair, and three-quarters of the public came to support it. Recently, the Supreme Court has deregulated the penalty, and new legislation keeps federal courts from enforcing due process protections that remain. Still, popular support for the penalty is relatively high, although it has recently dropped some. Behind these trends lie three assumptions: (1) The 1970s reforms have enabled American criminal justice institutions to administer the death penalty fairly and functionally; (2) juries can tell who deserves to die, and state judges can cure rare mistakes; (3) multiple layers of justice system review may be unnecessary and at cross-purposes with the goals of the criminal justice system.

Recent empirical work at Columbia Law School on appellate review of capital sentences challenges these assumptions. We have analyzed over 5,000 state and federal court decisions reviewing recent death sentences. From the early 1970s through 1995, state courts found 41 of every 100 death verdicts too unreliable to be carried out under law; at least six of the remaining 59 (10%) met the same fate on later state post-conviction review, and 21 of the remaining 53 (40%) met the same fate on later review in federal court. Finally, about 5 of the remaining 32 capital prisoners died of other causes while their sentences were under review. Our findings indicate that although frequent reversals impose costs and delays, they also are necessary to avoid serious harm, including the execution of individuals for whom the law does not authorize death as a permissible penalty.

In this study, we report the results of a comprehensive of analysis capital cases in the U.S. from 1973-95. The analyses were conducted on a unique database compiled by a multidisciplinary team of students and faculty from both law and social science disciplines at Columbia University. We focused on the "supply" of death penalty cases post-*Furman*, the outcomes of state direct appeal, state post-conviction and federal habeas corpus review procedures, and the pathways to execution or reversal in each of these cases. We identify factors that shape variations from state to state in capital reversal rates, and also that shape variation in the outcomes of federal habeas review.

The study is the most comprehensive study to date on post-trial review of capital criminal cases. It covers men and women sentenced to die in all 34 states that were active capital sentencers between 1973 and 1995, which includes the vast majority of states that now have the death penalty and all of the states (*e.g.*, Arizona, California, Florida, Georgia, Illinois, Pennsylvania, Texas, Virginia) that have large death row populations and use the death penalty as a regular part of their criminal justice systems.

In order to be as comprehensive and reliable as possible, the study improves in several ways on the important BJS *Capital Punishment* series.[1] The data allow us to examine separately reversals by state versus federal courts; the study includes the types of and reasons for reversals; it includes temporal dimensions of the three phases of the appellate review process that permits estimates of a number of potential conditions related to error; it examines all reversals, not just those that physically remove prisoners from death row for long enough to register in a once-yearly census of death row inmates; it examines all death

sentences and reversals, including ones that occur so closely together that the prisoner never reached the state's death row; and, it recognizes and takes accurate account of numerous reasons why a single individual on death row (the unit of analysis in the BJS data) may represent multiple death verdicts, each of which is analyzed in this study to generate a full picture of the reliability and legality of judgments of death.

Finally, the research is one of the most comprehensive American studies of the behavior of the appellate courts of any sort, whether criminal or civil. It is broad in the span of years (23) studied, the breadth of jurisdictions included (34 states), the capacity to compare two phases of court review – state court review and federal court review-- and the range of case-specific, court-specific and political factors considered. Similarly, by contrast to the various studies of the behaviors and decisions of trial-level actors in capital cases, this study is not focused on the effect of a particular case-specific factor (such as race, age, the jury-selection technique, strength of the evidence, the type of legal representation, a particular jury instruction, or the like) but instead looks simultaneously at a wide variety of such factors and, in addition, at a broad set of time-specific, court-specific, criminal-justice-system-specific, and political factors.

SPECIFIC AIMS

The project pursued the following specific aims:

- *Identify differences in rates of death penalty reversals and habeas outcomes by states and years, and identify structural and case-level sources of variation.* We catalogued death sentences and outcomes of state direct appeal, state post-conviction and federal habeas corpus appeals by state and by year.

- *Describe extent and magnitude of changes over time between and within states.* The supply of death penalty cases has grown over time, both within and across states, but at an uneven pace. States such as Virginia, Texas and Alabama lead the nation in executions, and their death sentence and execution rates have grown even as homicide victimizations have fallen. Other states have long had constitutionally valid death penalty statutes, but have used these statutes more sparingly despite recurring homicide cycles. Moreover, the error rates found by the same federal circuit judges among capital verdicts from different states within the circuit diverge dramatically. We chart and analyze disparities and convergences in the error rates detected on state direct appeal and state post-conviction review within the state courts and at the federal habeas corpus reviewstage.

- *Identify factors that explain variation between states within and over time.* The supply of death penalty cases has grown unevenly across states, as have the numbers of executions. Moreover, the growth rates have accelerated in some places and remained relatively stable in others. We examine a range of explanatory factors, using a hierarchical framework that incorporates influences of state social structure, court structure and composition, case characteristics, and the political economy of criminal justice administration.

- *Identify factors that influence different rates of error discovered at different stages of review in death penalty cases.* From 1973-95, 41% of the 4,545 capital convictions and/or sentences reviewed by state courts were found flawed and returned for either new trials or lesser sentences. In state post-conviction review, the data are less complete but suggest a reversal rate of 10% or more of capital verdicts that are finally reviewed at that stage. In the federal courts, the error rate was 40%. We assess the sources of error rate and explanations for variation in the error rates at each of the three levels of appellate review. As above, we include factors at several levels of explanation, from the case to the court to larger geographic aggregations. Time is an important consideration, and we will assess whether the factors predictive of errors change over time. That is, we test the assumption that error, as evidenced by reversals on direct appeal or granting of habeas corpus petitions, is explained by different factors as the size of the death row population increases.

- *Identify reform options for moderating the amount of capital error.* We identify policy options for responding to serious capital error and the resulting risk that innocent people will be executed. Policy options are developed that can reasonably assure substantial declines in chronic capital error rates, as well as policy options to avoid because they would probably magnify the problem of serious capital error.

ERROR AND INEFFICIENCY IN OUR SYSTEM OF CAPITAL PUNISHMENT

This study addresses the potential and real discrepancies between the actual system of capital punishment in the U.S. and (1) the goals the Supreme Court, Congress and state legislatures designed that system to achieve, and (2) reform proposals currently dominating public discussion.

A. Capital Punishment After *Furman*

In *Furman v. Georgia*,[2] in 1972, the Supreme Court considered whether the death penalty for murder constitutes cruel and unusual punishment in violation of the Eighth Amendment. Sidestepping that question of *substantive* penological policy, the Court concluded that all 600 death sentences then in effect in the nation were invalid because the *procedures* used to obtain them did not reliably distinguish defendants who deserved to die from those who did not. In the decades since *Furman*, 40 American states have reimposed the death penalty under statutes designed to avoid the procedural defects the Court identified in *Furman*.

During the same period, the Court decided numerous cases aimed at specifying the procedures the Constitution requires at capital-sentencing trials. The resulting array of

procedural requirements unleashed a barrage of state post-conviction and federal habeas corpus litigation attacking the constitutionality of particular capital trials. As a result, the Court — and, more recently, Congress via the Antiterrorism and Effective Death Penalty Act,[3] and state legislatures— have attempted to contain post-sentence review procedures while still enforcing the Court's constitutional rulings.

However, there now is strong evidence that our capital system does not operate in a reliable of efficient manner. High rates and amounts of capital error discovered at both state and federal review are a prime contributor to the problem.[4] This warrants a careful study of the determinants of error.

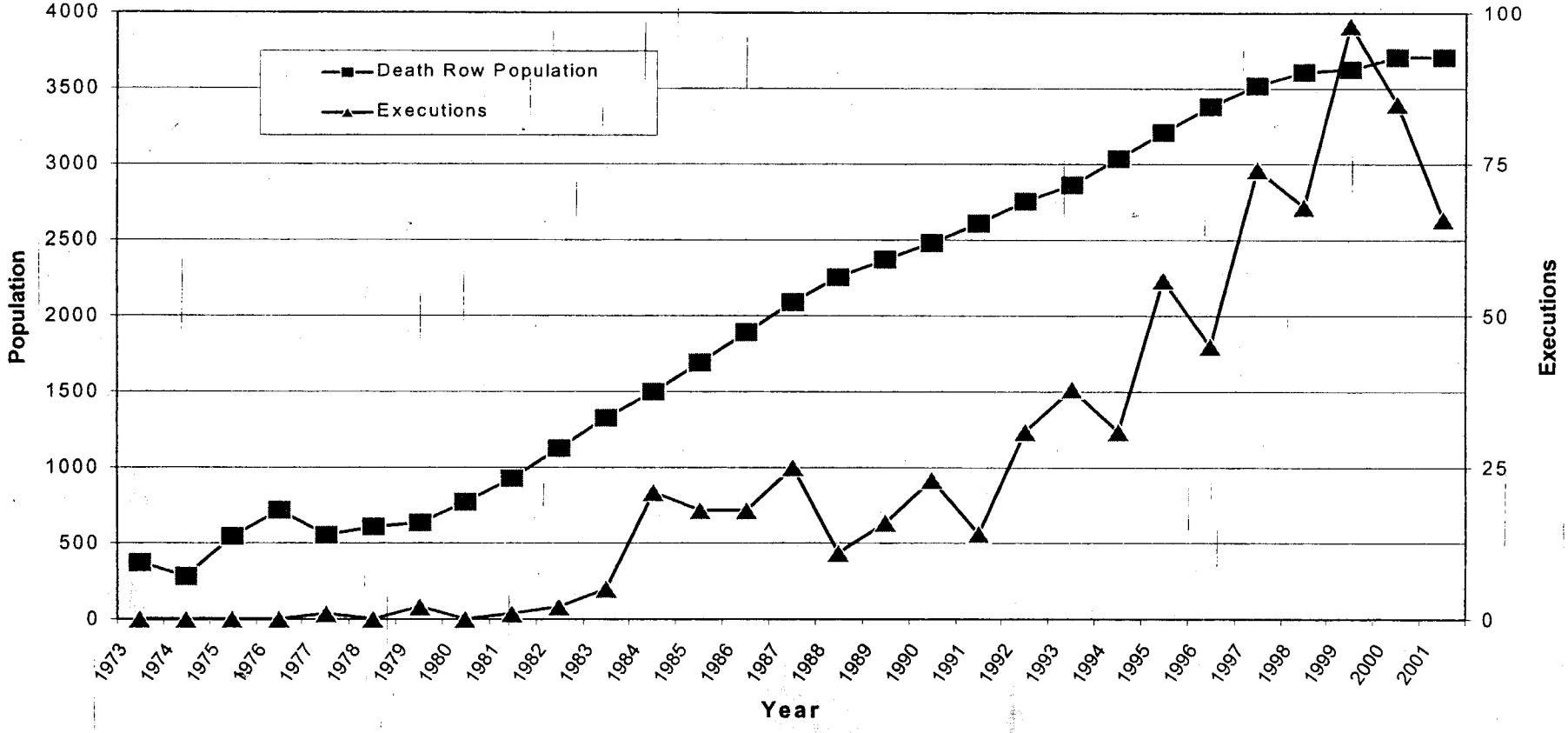
B. Actual Operation

Figure 1a shows the supply of execution-eligible cases through the number of admissions to death row each year from 1977 to 2001, and the percent executed from that base. Since 1982, the number of new death row admissions has rarely deviated from the average by no more than about 15% in any year.[5] Figure 1b shows the steadily increasing death row population over the past 30 years, and the number of nonconsensual executions (i.e., ones preceded by full judicial review) during the same period. Executions were relatively rare (hovering in the mid-teens) and stable between 1984 (when post- *Furman* executions began in earnest) and 1991.

After 1991, there was a gradual increase into the mid-1990s, and a steeper increase through 1999, after which the number of executions decreased significantly. These two figures show two interesting trends. First, many more people reach death row each year than are executed. Second, the number of death row inmates executed rises as the death row population rises, but the percent executed has remained uniformly low – less than 3 percent and averaging about 1.5 percent since *Furman*.

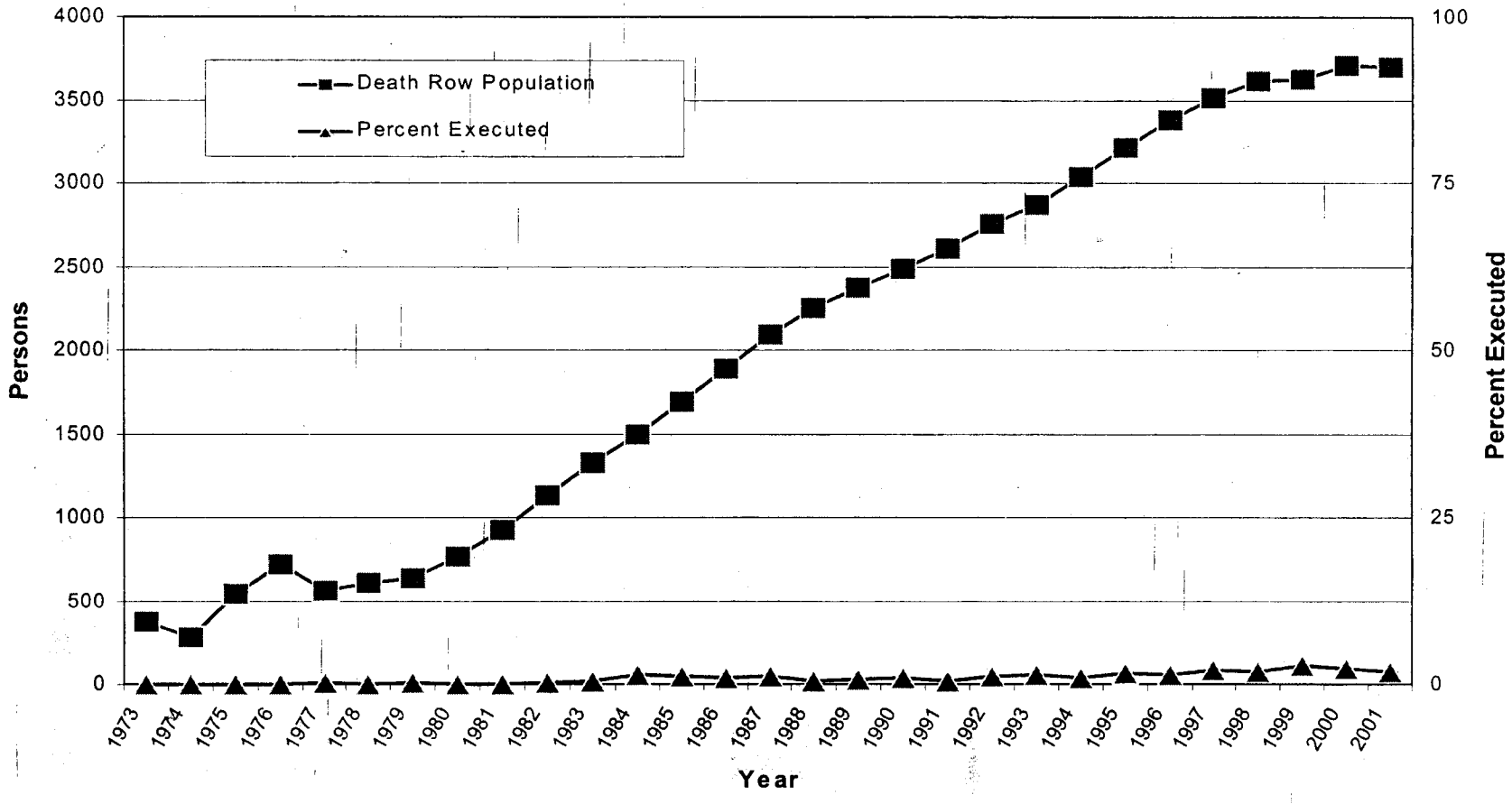
Thus, both figures show the sharp and steady growth in the *number* of prisoners on death row. On the other hand, during the entire post-Furman period, there was a remarkably small proportion of death row inmates who have been executed each year: an average of only 1.36 percent of the prisoners on death row were executed each year between 1984 and 2001, with the proportion executed falling between .5% and 2% during the entire period. The recent increase in the number of executions thus may *not* reflect a greater ability to carry out executions. Instead, it seems to reflect a stable willingness or ability over decades to execute only a *tiny* proportion (no more than 1 out of 50) of a consequently burgeoning death row. Figures 1a and 1b plot the growth of death row and compares the number of executions and proportion of death row executed each year.

Figure 1a. Persons on Death Row and Number of Executions, 1973-2001



Source: Bureau of Justice Statistics, Capital Punishment 2000 (NCJ 190598); Death Row USA; NAACP Legal Defense Fund, Direct Appeal Database (2002).

Figure 1b. Persons on Death Row and Percent Executed, 1973-2001



Source: Bureau of Justice Statistics, Capital Punishment 2000 (NCJ 190598); Death Row USA; Direct Appeal Database (2002).

Why do inmates pile up on death row without being executed? Figure 2 shows the systems for review of capital verdicts. The answer is that upon arriving on death row, inmates do not immediately queue up to be executed. Instead, they wait while their cases are reviewed in state appellate and post-conviction proceedings. And the *vast majority* of their capital judgments are *overturned* by state and federal courts.

Tables 1-4 show the results of over 4,000 state direct appeal cases and approximately 600 capital federal habeas corpus decisions in that same 23-year period. Table 1 shows that state direct appeal courts reversed 40% of the capital judgments that survive state direct appeal and state post-conviction and go on to federal habeas review.[6] Tables 2-4 reveal the fate of the state direct (trial court) appeals and state post-conviction review and go on to federal court habeas review.[7] Analyzing the cases by state of origin, year, and federal court rendering the final decision, those tables reveal that 40% of capital judgments reviewed in federal proceedings during the study period were overturned. In some states, reversal rates were much higher—for example, 76% in Mississippi and 65% in Georgia.

When state post-conviction reversals are also considered, state and federal courts overturned about **2** of every **3** death sentences they reviewed. In Georgia the courts overturned about **3** of every **4** death sentences; in Mississippi, over **9** out of every **10**.[8] Rather than queuing up to be executed, that is, most condemned inmates queue up to be *released* from death row by (for the most part) state courts and (to a lesser extent) federal courts as a result of serious, reversible legal error occurring at their capital trials.[9] (Noncapital reversal rates are much lower.)

FIGURE 2. THE CAPITAL CRIMINAL PROCESS: TRIAL THROUGH STATE POST-CONVICTION AND FEDERAL HABEAS

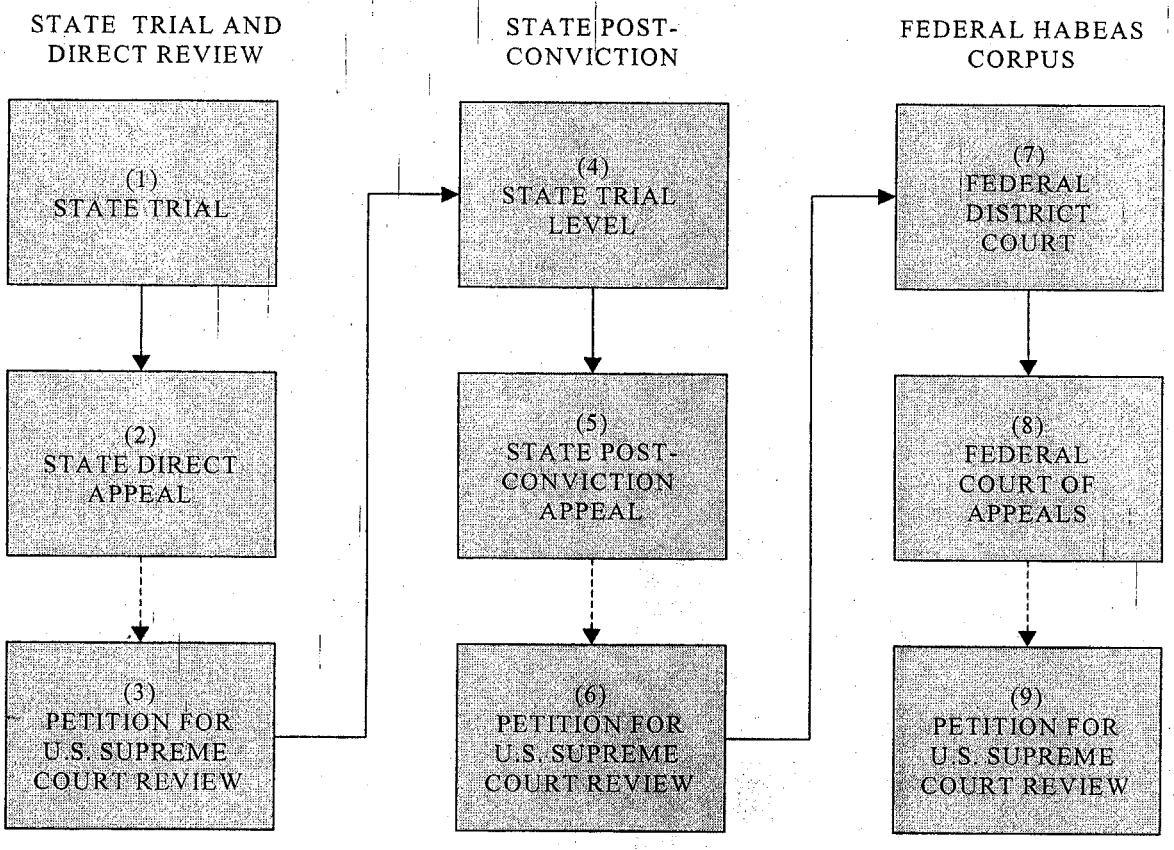


Table 1. Relief Granted By State Higher Courts on Capital Direct Appeal, 1973-1995*

State	Year of First Decision	Relief Granted (%)	Relief Granted (N)		Total
			No	Yes	
Connecticut	1994	100		2	2
Oregon	1981	88	4	28	32
New Jersey	1987	87	5	33	38
Colorado	1984	75	1	3	4
North Carolina	1979	59	94	137	231
Mississippi	1977	57	50	65	115
Alabama**	1977	54	122	144	266
South Carolina	1979	54	57	66	123
Maryland	1980	53	27	30	57
Kentucky	1980	52	25	27	52
Florida	1974	50	382	375	757
Louisiana	1978	47	60	53	113
Oklahoma	1980	46	104	90	194
Arizona	1975	41	116	82	198
Arkansas	1975	40	48	32	80
Idaho	1977	40	21	14	35
Illinois	1980	39	136	88	224
Nebraska	1977	36	14	8	22
Utah	1975	36	14	8	22
Georgia	1973	36	197	112	309
California	1979	32	185	86	271
Delaware	1981	30	16	7	23
Texas	1975	30	373	161	534
Nevada	1977	30	68	29	97
Tennessee	1979	29	79	33	112
Pennsylvania	1981	29	137	56	193
Indiana	1980	26	52	18	70
New Mexico	1983	25	6	2	8
Washington	1984	25	12	4	16
Wyoming	1977	25	3	1	4
Ohio	1984	24	93	29	122
Missouri	1981	17	82	17	99
Montana	1978	15	11	2	13
Virginia	1978	8	100	9	109
Total		41	2,694	1,851	4545

Source: Liebman et al. (2002), Direct Appeals Data Base

* All Direct Appeals Decided by the end of 1995.

** Alabama has regular high court review of its lower court direct appeal decisions. Both court decisions had to occur prior to 1995 to be included.

Table 2. Capital Habeas Relief Granted in Federal Courts By Year of Final Habeas Decision, 1973-1995*

Year	Relief Granted (%)	Relief Granted (N)		Total #
		No	Yes	
1975	100		1	1
1978	67	1	2	3
1980	100		4	4
1981	71	2	5	7
1982	72	5	13	18
1983	35	22	12	34
1984	35	24	13	37
1985	43	23	17	40
1986	42	29	21	50
1987	49	22	21	43
1988	39	35	22	57
1989	35	26	14	40
1990	27	24	9	33
1991	36	25	14	39
1992	33	33	16	49
1993	30	23	10	33
1994	23	47	14	61
1995	53	9	10	19
Total	38	350	218	568

Source: Liebman et al., 2002.

* For 30 verdicts, sentencing year was not reported in the decision.

Table 3. Capital Habeas Relief by Court of Decision, 1973-1995*

	Relief Granted %	Relief Granted (N) No	Relief Granted (N) Yes	Total
CIRCUIT & SUPREME COURTS				
Sixth Circuit	100		2	2
Ninth Circuit	56	12	15	27
Eleventh Circuit	48	94	86	180
Tenth Circuit	47	8	7	15
U.S. Supreme Court	45	17	14	31
Seventh Circuit	42	7	5	12
Eighth Circuit	33	38	19	57
Third Circuit	25	3	1	4
Fifth Circuit	24	125	40	165
Fourth Circuit	9	40	4	44
DISTRICT COURTS				
Middle District of Alabama	100		2	2
Arizona District Court	100		3	3
Delaware District Court	0	1		1
Middle District of Florida	50	1	1	2
Northern District of Florida	100		1	1
Northern District of Georgia	100		2	2
Northern District of Illinois	0	1		1
Maryland District Court	100		1	1
Northern District of Mississippi	1		1	1
Western District of Mississippi	100		1	1
Eastern District of Texas	100		1	1
Northern District of Texas	100		1	1
Southern District of Texas	64	1	2	3
Texas Appellate Court	0	1		1
Southern District of Georgia	100		1	1
Northern District of Alabama	100		1	1
TOTAL	38	349	211	560

*All Final Dispositions of first petitions for which there is information on the court of decision 1973-1995. 30 cases are omitted because of missing information on sentencing year.

Source: Liebman et al. (2002)

Table 4. Capital Habeas Relief by Original State of Verdict, 1973-1995*

State	Relief Granted (%)	Relief Granted (N) No	Relief Granted (N) Yes	Total
Kentucky	100		1	1
Maryland	100		3	3
Tennessee	100		1	1
California	80	1	4	5
Mississippi	76	6	19	25
Montana	75	1	3	4
Idaho	67	1	2	3
Washington	67	1	2	3
Georgia	65	34	63	97
Arizona	60	6	9	15
Indiana	50	2	2	4
Nevada	50	2	2	4
Oklahoma	50	6	6	12
Wyoming	50	1	1	2
Arkansas	48	13	12	25
Alabama	45	12	10	22
Nebraska	43	4	3	7
Florida	39	59	38	97
Pennsylvania	33	2	1	3
Utah	33	2	1	3
Illinois	30	7	3	10
Louisiana	27	27	10	37
Texas	25	103	35	138
North Carolina	18	9	2	11
Missouri	15	22	4	26
South Carolina	14	6	1	7
Virginia	7	28	2	30
Delaware	0	2		2
Total	40	357	240	597

*All final dispositions of federal habeas corpus petitions, 1973-1995

Source: Liebman et al. (2002)

C. Hypotheses

The study hypotheses are animated in part by the empirical observation that states vary widely in their rates of serious, reversible error in capital cases. These differences operate at the aggregate, or state level, and evidently occur independently from case-level characteristics of offense, offender, and court actors. The hypotheses also seek to apply lessons from recent studies that identify factors generally associated with death-sentencing pressures, both from the electorate and in the processing of cases within legal institutions. These include research on patterns of imprisonment[10] and capital punishment.[11] Specifically, we test the following hypotheses:

- ***Higher rates of death sentences per homicide, are associated with higher rates of reversible errors.***

Preliminary analyses show that state and federal court reversal rates are positively correlated with the relevant state's "death-producing" rates.[12] The higher production of cases occurs in the absence of strengthening of regulatory mechanisms at the trial stage to detect and avert error.

Several additional hypotheses predict high rates of serious capital error. Each reflects the potential influence of fears about serious crime, or mechanisms through which those fears can generate political pressure on officials to respond forcefully to serious crime, including through increased use of the death penalty. Some of those fears are based on actual crime and punishment rates, while others reflect social structural factors in states' racial and socioeconomic composition. All of these additional hypotheses reflect heightened pressure to use and overuse the death penalty, which may account for their link to high rates and amounts of serious capital error. These hypotheses are:

- ***Greater political pressure on state trial judges (as reflected in extent to which judicial selection techniques place stage judges at risk of political discipline for unpopular rulings) is associated with higher reversal rates.***

- ***Serious and reversible errors in death sentences will increase as the rates of white homicide victimization rises relative to the black homicide victimization rate;***

- ***Error rates in death sentences will be inversely associated with the ability of law enforcement policies and agencies to apprehend, convict and incarcerate serious criminal offenders;***

- ***The size of African-American and poor communities – which politically influential citizens may associate with higher rates of serious crime – will be associated with the error rate in death sentences;***

- ***Low levels of funding for court systems and higher court caseloads are associated with greater risks of serious capital error.***

For example, punishment trends often responds to political influences. One

dimension of political pressure, for example, may be proximity to elections. Additionally, the dominance of parties that express conservative views on punishment and that openly support harsh sanctions, appears to be associated with higher rates of prison admissions.[13] Social science studies also has demonstrated a link between death sentencing rates, political views, and the rapidity of passage of valid death penalty statutes following *Furman* in 1972. The threat of electoral challenge often pushes both parties to move toward legislation endorsing harsher punishment, including death penalty legislation. In this study, we estimate the extent to which political pressures also may contribute to *flawed* death sentences.

We also address the possibility of change in the administration of the death penalty following *Furman*. The enactment of death penalty statutes within a compressed period of time following *Furman* also raises a potential source of error.[14] Accordingly, we consider the time-sensitivity of reversal rates—based, for example, on their proximity to the state's initial adoption of a post-*Furman* death sentencing statute and its first affirmance of a death sentence, and on the passage of time since *Furman*. We hypothesize that:

- ***Controlling for other relevant factors, the rate of reversible error in death sentences within each state declines over time following the state's passage of its first valid death penalty statute after 1973.***

Finally, we hypothesize that individual case factors are associated with reversible error at the federal habeas stage of review:

- ***Factors related to the case, including the strength of the evidence and the case for death, the number of aggravating and mitigating circumstances, the procedures used in state or federal review, the federal review proceeding itself, and the political affiliations of reviewing judges, together with the social standing of the victim, are associated with the probability of a federal court reversal of a death sentence.***

Together, these hypotheses suggest that political and judicial pressures within states combine to generate comparatively large numbers of substantively and legally inadequate death sentences. The large numbers of flawed capital judgments then become the burden of appellate and post-conviction review courts to correct — at great cost to those courts and to the state's taxpayers, crime victims, wrongly condemned prisoners and, ultimately, to the rationality of the system as a whole. Even while performing this crucial function, however, these review mechanisms themselves are vulnerable to overload and error. Although designed in theory to identify *procedural* miscues, they serve in fact as a second round of *substantive* decision making about who deserves to live and die—a task for which reviewing courts are not practically or democratically suited.

D. Law and Policy Context

These hypotheses have important implications for the contemporary debate about reform of (1) capital punishment, (2) state post-conviction review procedures, and (3) federal habeas corpus. In the late 1980s and early 1990s, the major reform effort was state and federal legislation creating "death penalty resource centers" that provided lawyers for death

row inmates in state and federal *post-conviction* proceedings. More recently, reforms (e.g., the federal Antiterrorism and Effective Death Penalty Act[15] and recent Texas legislation[16]) have aimed to truncate those same *post-conviction* proceedings, shutting down the "resource centers."

One policy context of this research, then, is that the post-conviction focus of all these reforms may be counter-productive. Initiatives instead could focus on making *trial-level* actors "pay their own way" in capital-sentencing, and forestall the production of flawed death sentences that carry enormous monetary, psychological and political costs. Resources spent at the trial level for careful police, prosecutorial and defense investigations, disclosure of police files, adequate defense counsel, necessary expert services, and scrupulous trial rulings and jury instructions may more than pay for themselves in saved expenditures on post-sentence review, retrials and damaged judicial and criminal justice credibility.

Given high levels of flawed death sentences and, consequently, of the length and necessarily painstaking post-sentence review and high levels and proportions of reversals, and the resulting minuscule rates at which death sentences are actually carried out, a system designed to make trials the "main event" in identifying death-deserving offenders, instead relies on post-sentence judicial review to serve that function. Their reversals consequently are controversial; their affirmances may allow undeserving, even innocent, defendants to be executed. And because appellate courts' expertise lies in procedural superintendency, not in substantively determining guilt and desert of the death penalty, the courts' legitimacy may be in doubt.

METHODS

A. Design

The research is a pooled, cross-sectional analysis of all (N=5826) death verdicts and their outcomes at three stages of judicial processing over a 23-year period from 1973-95. The study period concluded at 1995, to allow sufficient time for cases to reach their conclusion. We excluded cases that had not reached a final decision, specifically where appeals had not been filed or. were still pending. We considered (1) the results of 4545 final state direct appeals, (2) 257 state post-conviction reversals (considering them as a proportion of death verdicts that survived state direct appeal), and all 598 final dispositions of initial federal habeas corpus petitions during the study period. The study population is a universe of all eligible cases, not a sample. Error (reversal) rates were computed for each stage, and a composite error rate across all stages was computed for each state. The analysis considered cases both at the state and individual levels. At the individual level, the study again included all 598 cases where final dispositions of initial federal habeas corpus petitions occurred.

To identify factors that explain variation between states and variation over time, we used general linear models with both fixed and random effects to assess the influence of the legal and social context of courts, together with legal and social characteristics of the case, to identify the factors leading to serious, reversible errors in capital sentencing. Models with both over-dispersed Poisson logarithmic regression and over-dispersed binomial regression with logistic error terms were examined. We included contextual information on the state and federal courts where capital sentences are "produced" and appeals decided. In addition to the year of decision, the analyses also explicitly incorporated the passage of time to assess the changing dynamics of capital sentencing and appellate review, or regulation, over time.

More detailed information was collected and analyzed for federal habeas corpus cases. Information on case-level factors that influenced decisions of these cases was gathered from case files, and included information about the victim, offense, offender, the court and the court actors. To identify factors that influenced case outcomes at this stage, we used logistic regression models. Time was again included in these models to account for the changing contexts of capital punishment over the study period.

B. Variables and Measures

1. Dependent Variable: Serious Error

The dependent variable for the state-level analysis is court reversals based on findings of serious reversible error, which are highly correlated with a failure to carry out death sentences. When all three review stages are considered, reversals and their opposite (affirmances) number in the thousands and their rates (as a proportion of all death verdicts and as a proportion of only reviewed death verdicts) vary considerably across place and time. The dependent variable in the individual-level analysis is whether the death verdict was reversed in federal habeas proceedings.

Legal rules on capital punishment 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.[17] In fact, 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. This occurred even in cases in which the defendant was later proven to be factually innocent, but where these legal restrictions on reversals prevented courts from reversing verdicts they realized were unreliable.

Previous analyses of reversals showed that nearly all occurred because of serious trial error.[18] Detailed information on the reasons for reversal or affirmance is available at the latter two of three stages of the capital appeal system: state post-conviction and federal habeas. Earlier analyses of these data show that inspections at these stages included 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. [19]

These two latter review stages also are the ones most likely to reverse due to error not affecting the reliability of verdicts. At the state post-conviction review phase, Liebman et al. (2000) found that four types of error 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%*).

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

At the federal habeas stage, eight types of error account for 81% 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%).

Since many of these errors occur in the same case, the percentages add to more than 100. Together with the four errors, four other types of error 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;[20]
- 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.[21]

At the same time, courts almost never reverse capital verdicts based on technical error freeing demonstrated perpetrators of serious crimes. For example, no 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 search, seizure or presentation in court of reliable evidence.[22]

Accordingly, legal mistakes are rarely a basis for reversing capital verdicts unless they are found to have impaired the reliability of the outcome. The outcomes of these cases on retrial suggest the inherent flaws that were detected upon review. During the study period, most (82%) of the verdicts reversed at the state post-conviction stage (where data were available) ended in non-capital outcomes on retrial. Nearly three in four (73%) of the reversed verdicts ended in a sentence less than death, and almost one in ten (9%) ended in acquittal.[23] The clear tendency of error to undercut the reliability of death verdicts is the primary reason that we conclude that reversible errors are *serious* errors, and include their rates (in state-level analyses) or occurrence (in individual-level analyses) as the dependent variable for this research.

a. Imposed versus Reviewed Verdicts

To calculate reversal rates for purposes of comparing states and years, we include cases that had reached their final verdicts as of 1995, and excluded those that had not completed all possible stages of review. Comparing states based on the proportion of *imposed* verdicts that were reversed as of the end of the study period, regardless of whether they had completed review, provides an incomplete picture of the success or failure of verdicts upon inspection, because it causes all verdicts that were never reviewed for error to be considered error free. That type of comparison also makes it difficult to gauge patterns of *error* over time, as opposed to its relationship to unfinished or delayed review. To improve the power of time trend (the year verdicts were imposed) to gauge whether error is increasing or decreasing over time after controlling for other factors, it is preferable to compare rates of *reviewed* verdicts that were reversed, instead of comparing rates of *imposed* verdicts that were reversed.

To understand the biased estimates that would result from inclusion of imposed but not finally reviewed verdicts, consider the following example. 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 also could (and indeed, almost certainly does) 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 cases still pending review – and as a result – 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.

Unfortunately, even 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 (Liebman et al., 2002, Figure 10).[24] Given these long delays in concluding federal habeas review, and the fact that affirmed cases were decided more slowly than cases where appeals were denied, censoring cases at 1995 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, for example), 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. Table 5, from Liebman et al. (2002), shows that that the number of verdicts from each year that was still under review as of 1995 grows over the study period, and illustrates the potential bias in estimating the effects of time were verdicts imposed *later* in the study period are included in the analyses.

Table 5: Percent of Death Verdicts Still Under Review at Some Review Stage as of the Study Cut-off Date

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%

The result is a misleading impression that later death verdicts have fewer errors than cases earlier in the post-*Furman* period. Instead, error-free verdicts move to the front of the line of cases getting finally reviewed, pushing 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.

On the other hand, it is necessary to have a single, common denominator for verdicts insofar as a single, combined reversal rate for all three stages of review is to be calculated. The only available common denominator is the number of verdicts imposed in a given state in a given year. This is because the same verdict may be affirmed at the first review stage

but reversed at the second review stage, or may have been reviewed at the first stage but still may be under review at the second or third review stage at the end of the study period. The only way to analyze verdicts' fates across all three stages is to ask whether or not it was reversed at some stage along the way. The dilemma thus is that in order to obtain the most complete information – reversal rates at all three stages combined – it is necessary to utilize analytic methods that (1) assure declining rates of review and thus of reversal over time, which in turn (2) makes it difficult to tell whether the relationship between time and declining reversal rates is solely the result of delay in review or whether additionally, it is a result of cleaner death sentences over time. The solutino to this dilemma is to perform both types of analyses– three stage analyses of reversals as a proportion of imposed verdicts, and single-stage analyses of reversals as proportions of verdicts reviewed at that stage. In the analyses reported below, we do both, in the interest of obtaining the most robust possible estimates of the sources of error in capital verdicts.

2. Explanatory Factors – State Reversal Rates

Our choice of factors to study was guided by this question: What types of conditions at the state and case level might plausibly affect how much serious error trial actors make in imposing capital verdicts and how much appellate courts discover when they inspect those verdicts? The hypotheses discussed earlier reflected empirical and theoretical literature on both capital punishment and judicial decision making more broadly. Table 6 lists categories of state factors we incorporated in the analyses of state decision making. Table 7 shows the measures constructed from variables in each of the domains in Table 6, and descriptions.

Table 6. Factors Considered in Analyses of State Capital Reversal Rates

1. Capital Judgements and Appeals
   a. Number and Rates (per homicide) of Capital Verdicts
   b. Number and Results of Direct Appeals (as a proportion of reviewed verdicts)
   c. Number and Results of State Post-Conviction Proceedings (as a proportion of verdicts available for review)
   d. Number and Results of Federal Habeas Review
   e. Number and Results of Capital Verdicts Reviewed at all Three Stages (as a proportion of imposed verdicts)
   f. Number and Rates of Undecided Appeals
   g. Post-Reversal Capital Reprosecution Rates
   h. Index of Timing of Adoption and Initial Use of Death Penalty post-*Furman*

2. Death Row Population (state only)
   a. Race (including relative to state population)
   b. Race of Victim (in proportion to state population and proportion in state)

3. Functioning of the Judicial System Generally (state only)
   a. Caseloads
   b. Dispositions
   c. Backlogs
   d. Expenditures
   e. Presence or Absence of Death Penalty Resource Center (state only)

4. Political Pressure on the Judiciary (state only)
   a. Selections and Retention Methods
   b. Party and Ideological Influences

5. State Demographic Characteristics
   a. Population
   b. Race
   c. Population Density and Urbanization
   d. Median Age

6. 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)

7. Political, Ideological and Religious Characteristics (state only)
   a. Relative Strength of Two Major Political Parties
   b. Extent of Use of Criminal Sanctions

8. Social Characteristics
   a. Poverty Rates
   b. Per Capital Rate of Welfare Recipients and Per Capita Welfare Expenditures
   c. Income Distribution

Table 7. Descriptions of Variables Considered in Analysis of State-Level Characteristics

Death Sentences

dor, dorst	The state's number of death verdicts per 1000 homicides.
dswvrt	The state's number of death verdicts imposed for offenses against at least one white victim per 100 white homicide victims.
facvic2l	A principal component factor (combined measure) of the log of the state's number of death verdicts imposed for offenses against at least one white victim per 100 white homicide victims (dswvrt), and the log of the percent of death verdicts imposed for homicides against at least one white victim divided by the percentage of all homicide victims in the state who are white (wvdsst).

Court Backlogs., Caseloads and Expenditures

bltot, bltotst	The number of death verdicts imposed in the state in the relevant year that were awaiting court review (*i.e.*, were backlogged) as of the end of the study period.
pcbl	The number of death verdicts imposed in the state in the relevant year that had not been finally reviewed at the state post-conviction stage as of the end of the study period. This variable is calculated as: (death verdicts that were not finally reviewed at any of the three review stages + death verdicts that had been reviewed and had been approved on state direct appeal and were available for state post-conviction review) - (death verdicts reversed at the state post-conviction stage).
rgrtbklg	The rate per 1000 population of cases awaiting decision in the state's court system. This is a static measure, estimated as filings minus the dispositions averaged over the 10-year period (1985-1995) in which data are available. This is one of the four measures of general court backlogs in fac_csld.
dir_exrt	The state's direct expenditures on its court system per 100,000 population.
fac_csld	A principal component factor (combined measure) of the state's rate of court case backlogs (civil cases, criminal cases, felony cases, total cases (rgrtbklg)) awaiting decision in the state's courts. Each component of the factor is a static measure estimated as filings minus dispositions averaged over the 10-year period (1985-1995) in which data are available. This variable measures court backlogs caused by all, not simply capital or criminal, cases.

Poverty

fac_welf	A principal component factor (combined measure) of the number of welfare recipients per 100,000 population, the amount of expenditures on welfare per 100,000 population, and the demographic makeup of the state.

Table 7. Variables Descriptions for Analysis of State-Level Characteristics (continued...)

Population Characteristics

pbdifnew	The percent of defendants sentenced to death by the state who are African-American minus the percent of the state's population that is comprised of African-Americans (plus 1, to assure there are no negative values).
blkpctst	The average percent of the state's population comprised of African-Americans during the study period.
pctblack, pctblkst	The percent of the state's population comprised of African-Americans
psst	A principal component factor of the log of the state's population and the log of the state's population density. This is a measure of the state's population structure.

Political Context

pajid	*Judicial Philosophy.* This party-adjusted judge ideology score, used in one of our analyses, is a combined measure of state supreme court justices' liberal versus conservative decision making that was developed by Paul Brace, Laura Langer & Melinda Hall. See *Measuring the Preferences of State Supreme Court Judges*, 62 J. of Politics 387 (2000). The variable compares states based on the party affiliations of their state supreme court justices and based on measures of the ideological disposition of the electorate (for states where judges are elected) and of elite portions of the population (where judges are appointed). The measure is a state mean from 1970 to 1993, scored from conservative to liberal.
pnindx	*Punishment Index.* The ratio of the number of inmates incarcerated in prison in the state in the relevant year per 100 FBI Index Crimes committed in the state in that year. This is measure of the rate at which serious criminals are apprehended, convicted and incarcerated in the state.
ppindx, ppindx2	*Political pressure indexes.* Two similar measures of the extent to which state judges are subject to political discipline for their rulings through political or electoral politics. The indexes include measures of the way in which judges are selected and retained, and the length of the first term. Two indices were created. The first index combines the way in which judges are selected, the way they are retained, and the length of the first term. Selection method consists of a scale of 1 to 4, with 1 being the least political method and 4 being the most political, with scores based on the appointing authority (legislature, governor), whether the appointment is subject to retention elections, or whether elections without appointments are used. Retention is a binary nominal category coded 1 for appointed judges who face constitutionally mandated retention votes, and zero for all other judges. Length of first term scales years from 1 to 4 with the assumption that longer terms diminish political pressure. Years are categorized based on frequency distributions of term lengths. Terms of 10 to 15 years are categorized as 1; 8 years is categorized as 2; 6 years is categorized as 3; and from 1 to 4 years is categorized as 4. In order to account for the short duration of many appointments, a second scale was used based on the length of judges' first elected term, or the longer of retention terms. For example, an appointed first term of 1 year followed by an election term of 15 years is considered a 15 year first term, and scaled as a 1 to reflect lower political pressure.

Table 7. Variables Descriptions for Analysis of State-Level Characteristics (continued...)

Homicide Rate and Characteristics

hrst	The state's number of homicides per 100,000 population.
wbrtst	The state's rate of white homicide victims per 100,000 white residents divided by its rate of black victims per 100,000 black residents. This is a measure of how the threat of homicides in the state is distributed between whites and blacks.
whblrts	The average number of white victims in the state per 100,000 white population divided by the average number of black victims in the state per 100,000 black population during the study period.
wvdsst	The percent of death verdicts imposed by the state for homicides against at least one white victim divided by the percentage of all homicide victims in the state who are white.
wvrtst	The state's rate of white homicide victims per 100,000 white residents.

Time of Sentence

year	The year in which the death verdict being studied was imposed.
yearn	The ~~23~~ years in the study period, taken as a linear trend from the beginning to the end of the study period.
sentyr	The year the death verdict was imposed.

Table 8: Factors Considered Explaining Outcomes of Capital Federal Habeas Cases

1. Sentencing State and County
   a. Offense
   b. Trial

2. 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

3. 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

4. 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

5. 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

Table 8. (Continued...)

6. 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

7. Judges
   a. Trial Judge
   b. State Direct Appeal Judges
   c. State Post-Conviction Judges
   d. Federal Habeas Judges
      i. District or Circuit
      ii. Name and Party of Appointing President
      iii. Education
      iv. Service as Prosecutor or Other Government Official
      v. Years on Bench
      vi. Size and Composition of Panels and Majority
      vii. Caseloads

8. 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

9. Procedural History
   a. Trial Procedures
      i. Length of Trial
      ii. Guilt Determination by Trial or Plea
      iii. Jury or Judge Sentencing
   b. State Appellate and Post-Conviction Procedures
      i. Votes and Outcomes
      ii. Was Evidentiary Hearing Requested, Held
   c. Federal Procedures
      i. Was Evidentiary Hearing Requested, Held
      ii. Lower Court Outcomes
      iii. Appeals

Table 9. Descriptions of Variables Considered in Analyses of Individual-Level Characteristics of Federal Habeas Cases

agg_mit	An additive scale of aggravating and mitigating circumstances. Each of 16 possible aggravating circumstances that was present in the federal habeas case was assigned a value of 1. Each of 12 possible mitigating circumstances that was present in the case was assigned a value of -1. This variable is the sum of those aggravating and mitigating circumstance values present in the case.
claimno	The number of claims (grounds for relief) presented by the habeas petitioner at the final stage of federal habeas review at which there was a decision on the merits.
dlos_ffd	A binary measure, coded 1 for federal habeas petitioners known to have been represented by a lawyer from out of state at the final stage of federal habeas review at which there was a decision on the merits, and coded 0 for petitioners not represented by an out-of-state lawyer at that stage.
feh2	A binary measure coded 1 for federal habeas cases in which a federal evidentiary hearing was held prior to final federal habeas review, and coded 0 for federal habeas cases in which no federal evidentiary hearing was held.
ofvcindx	An additive scale of seven elements relating to the character of the defendant and traits of the victim in a capital federal habeas case—that the defendant had a prior criminal record; the defendant had a history of alcohol abuse; the defendant had a history of drug abuse; the defendant was intoxicated at the time the crime was committed; the defendant had a connection to the community; the victim had one or more of a list of prominent statuses within the community (police officer, fire fighter, elected official, etc.); the victim was female. The value assigned each case is the number of the seven elements present in the case.
repmaj	*Republican Federal Judiciary*. A binary variable coded 1 for federal habeas cases in which a majority of the reviewing judges at the final stage of federal habeas review were appointed by Republican Presidents, and 0 otherwise.
seh2	A binary measure coded 1 for federal habeas cases in which a state evidentiary hearing was held on a claim later raised in the federal habeas proceeding, and coded 0 for federal habeas cases in which no such state evidentiary hearing was previously held.
sentyr	The year the death verdict was imposed.
year	The year in which the death verdict being studied was imposed.
yearn	The 23 years in the study period, taken as a linear trend from the beginning to the end of the study period.

3. Explanatory Factors – Case-Level Reversals

Table 8 lists factors in individual federal habeas cases that we considered as potential predictors of reversible error at that review stage, and Table 9 shows the measures constructed from those factors. 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 9 shows the measures constructed from variables in each of the domains in Table 8, and descriptions of those measures that were included as candidate predictors in the analyses.

4. Temporal Trends

We used model estimation procedures for analysis of panel data to address the effects of time in both within- and between-states trends in reversals. 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. 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. As we showed in Table 5, 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. Also, since review at the federal habeas stage takes longer when reversals are forthcoming compared to affirmances, estimates of the actual error rate are likely to be understated. In other words, the fact of a 1995 cut-off date leads the actual rate of error to be understated by causing more flawed than unflawed verdicts to go uncounted.

In our analyses in which reversal rates are a proportion of imposed verdicts, therefore, the interpretation of the effect of the time trend is more difficult because of the dural possibilities that less review, or possibly less error, will lead to fewer reversals. As noted, however, we sharpen our consideration of the time trend in an alternate set of review-stage-specific analyses in which we analyzed reversal rates as a proportion of only fully reviewed verdicts.[25] The fact that the relationship of time to reversal rates changes from our analyses of the combined stages (where the denominator is the number of imposed verdicts) compared to the analysis of direct appeal (where the denominator is finally reviewed verdicts and where later verdicts have substantially higher reversal rates) provides strong evidence that the principal or only reason reversal rates decline over time in the former analysis is because of the decline in review, not because in the decline of error over time when other factors are accounted for.

C. Data Sources and Measures

To address the aims of the study, we have created a unique and comprehensive database with information on both courts, communities and states, and cases. Table 10 summarizes the data files, sources and contents. Below, we briefly describe their contents.

1. Death Sentences and Appeals

There is no official, complete publicly available list of the decisions in, or outcomes of, capital appeals for any state in the country. This is especially true for pre-1995 decisions. The only systematic way to find those decisions is by using the names of death row inmates as search criteria in data bases collecting judicial decisions. Neither the federal government nor any state with valid capital punishment statutes has a list of all people sentenced to die between 1973 and 1995.[26] For this reason, the U.S. Supreme Court routinely relies on the unofficial list of people on death row across the country kept by the NAACP Legal Defense Fund's Death Row USA ("LDF"), which provides snapshots of the people on death row at four points each year. People on death row for only brief periods in between LDF's reporting dates are missed by that list. We too used that list as a source of names for search criteria in searches of legal databases.

We then supplemented that list with a variety of other sources of names and decisions (e.g., keyword searches of legal databases and newspaper databases; mining of information from judicial decisions we had previously discovered, which other refer to other decisions in capital cases. Also, we used all such information only as search criteria to find official court decisions while limiting our information about court outcomes to the official decisions themselves. In general, we found it more difficult to locate reversal decisions (which are more often unpublished and tend to divert cases from the fishbowl of capital cases) than to locate affirmed decisions (which are more often published and keep cases in the capital system, and where information in the later stages is often more complete and where information on executions is 100% complete).[27]

We used the following sources to obtain data and information about published and unpublished decisions:

- the LDF list described above;
- unofficial lists of death row inmates maintained by state courts, state prosecutors, state defense attorneys, and state NGOs;
- newspaper accounts naming capital prisoners;
- name searches, based on names from the above-listed sources, using Westlaw and Lexis search engines and using the West reporter system, to identify decisions made available through those search engines and through that reporter system;
- keyword searches using Westlaw and Lexis search engines and using the West reporter system, to identify capital decisions available through those search engines and through that reporter system;
- information in judicial decisions identified through the means described above, including information about other cases involving the same prisoner and about the cases of co-participants in the prisoner's offense (which generated information for additional name searches); and information in official but unpublished decisions obtained from court and case records.

From these sources, we assembled three original data bases:

- ***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 study period in which the relevant state had a valid post-*Furman* capital statute. The 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 decision include year of death verdict; year of decision; whether the verdict was affirmed or reversed; subsequent judicial history on rehearing in the state system and on certiorari to the U. S. Supreme Court; and citation. For a direct appeal to have been "finally decided" within the study 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 (so that the Supreme Court's action did not affect the finality of the state decision), the case is included in the study. 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. If the Supreme Court granted certiorari and reached a final decision in the case before or during 1995, the case is included in the data base.

- ***State Post-Conviction Data Base (SPCDB)***. This data base contains a list of capital verdicts that were imposed during years between 1973 and 2000 when the relevant state had a valid post-*Furman* capital statute and that were finally reversed on state post-conviction review between 1973 and April 2000. To be finally reversed on state post-conviction review, that reversal must have been the outcome reached during the specified period by the highest state court with jurisdiction to review the case in which the case was timely filed, or, if the U.S. Supreme Court granted certiorari to review a state post-conviction decision, that reversal must have been the outcome reached by the U.S. Supreme Court during the specified period. For each reversal, information is collected about year of decision; outcome of decision; basis for reversal; stage of trial affected by reversal; outcome on retrial; timing; and citation. Summary information using this data base is broken down by (1) death verdicts imposed and finally reviewed on state post-conviction review between 1973 and 1995, and (2) death verdicts imposed between 1973 and 2000 and finally reviewed on state post-conviction review between 1996 and 2000.

- ***Habeas Corpus Data Base (HCDB)***. This data base contains information on all decisions of initial (non-successive) capital federal habeas corpus cases between 1973 and 1995 that finally reviewed capital verdicts (sometimes referred to as "capital judgments") imposed during years in the 1973 to 1995 period in which the relevant state had a valid post-*Furman* capital statute. For a federal habeas case to have "finally reviewed" a capital verdict within the study period, all of the following events must have occurred in the case within that period: (1) a United States District Court must have denied habeas corpus relief from the capital judgment, thereby approving the 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).

2. Courts and the Criminal Justice System

Court Caseloads. General court caseload data are from 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.

Court Expenditures. Court expenditure data are from 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.

Capital Defense. The Administrative Office of the United States Courts, provides detailed state and federal funding information on death penalty resource centers, by year. Included are total budget, per capita budget, percent provided by the state, percent provided by the federal government, personnel cost, number and type of positions, an overhead expenditures.

3. State Demographic and Socio-Economic Characteristics

Population and Poverty Data. State population, socio-economic and racial composition are from the United States Census Bureau 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).

State Welfare Caseloads. Welfare recipients and expenditures are from The Statistical Almanac of the United States 1973-1996.

4. Crime and Punishment

State Crime Trends. Crime data are from the FBI Uniform Crime Reports 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.

Homicide Trends. State and county homicide and victimization data, including by race, are from the Vital Statistics of the United States or other data compilations generated by the Centers for Disease Control and Prevention National Center for Health Statistics. 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.

Punishment Trends. Annual state prison population data are from the Source Book of Criminal Justice Statistics, U.S. Department of Justice, Bureau of Justice Statistics,

Source Book of Criminal Justice Statistics 1973-1996.

6. Political and Electoral Contexts

Electoral. Several data sources on states' political structure, constructed from from official and a variety of archived and published sources. The file includes data on election cycles, election outcomes, judicial selection methods and party electoral share of legislators.

Judicial Philosophy. We used the combined measure of state supreme court justices' liberal versus conservative decision making, developed by Paul Brace, Laura Langer & Melinda Hall. See *Measuring the Preferences of State Supreme Court Judges*, Journal of Politics, Vol. 62, p. 387 (2000).

Political Pressure. Indices 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 See C. Flango & D. Rottman, Appellate Court Procedures (Nat'l Center for State Courts, 1998).

7. Legal Actors

Judges, Defense Attorneys, and Prosecutors. Data on the defense and prosecuting attorneys was collected from the Martindale-Hubble directory of lawyers, and includes type legal education, years of experience, size of firm and if the attorney is from a state other than the trial state. Data on circuit judges was collected from the Almanac of the Federal Judiciary. The Judges file includes an includes the year of appointment type of legal education, president of appointment and circuit.

8. Post *Furman* Chronology

Data were gathered from published and archived material on the passage death penalty statues following the U.S. Supreme Court's Furman decision. The file includes the date of the first valid post-Furman state, the first death sentence the first voluntary execution and the first non-voluntary execution.

Table 10. Core Data Domains and Sources

File Name	File Source	File Description
"THE HABEAS PROJECT"	Data collection from official court decisions	Data file of information gathered in our study. It is organized on the individual case level and contains data on judicial proceedings, the crime, the defendant, representation, claims raised, outcome of review proceedings, and other relevant information for study purposes.
COURT CASELOADS	State Court Statistics 1985-1994 (ICPSR 9266)	Court case filings and dispositions by state, year, type filing, type of disposition and court. Data are only available from 1985 to 1995 and are averaged over the period for which data are available.
DADB	Data collection from published decisions	Case level data on direct appeals of death verdicts from 1973 -1995 in which the state had a valid post-Furman capital statute. Included are: year of verdict; state and county of verdict; citation to decision on appeal; outcome of appeal
ELECTORAL	Data collection from published and archived materials and from the National Center for State Court Statistics 1998	Data on electoral cycles, judicial selection and retention procedures,
EXPEND_RANKINGS	Expenditure and Employment Data for the Criminal Justice System, ICPSR datasets: 2257,2940, 6006, 6459, 6579, 6795, 7618, 8382, 8455, 9160, 9161, 9162, 9396, 9554, 9773	Information on states' legal and court-related expenditures for the years 1971-1995.
HOMICIDE	Vital Statistics of the United States, (ICPSR 7632) CDC Wonder, http://wonder.cdc.gov and National Center for for Health Statistics, Compressed Mortality file , 1989-98 CD-ROM Series 20 No 2C ASCII Version	Annual compilation of homicides by state, year, and race of victim
INMATES	Sourcebook of Criminal Justice Statistics 1971-1997	Annual prison populations by state
JUDGES	Almanac of the Federal Judiciary	Information on judges' education, previous legal experience, appointment, and court.

POPULATION	United States Census Bureau	Total state population demography for every state, 1960-1996. State populations by year, 1960-1996, with racial breakdowns from 1970-1995. Population density, 1960-96.
POST FURMAN CHRONOLOGY	Data collection from state statutes, official state court decisions, list of executions by date	State-level information concerning the timing of important general events in the use of the state's death penalty following Furman. This includes timing of statutes, first death sentence, affirmed decisions, and execution.
STPC	Data collection	Death Verdicts reversed on State Post-Conviction Review 1973-April 2000 Case level data on reason for reversal, jurisdiction and citation.
UCR	Unified Crime Reports, Federal Bureau of Investigation http://www.ojp.usdoj.gov/bjs/datast.htm	Data on types of reported crimes, divided up into various categories for Type I offenses, reported as both totals and rates per 100,000 population. The data is reported by state and year, 1960-1996.
VERDICTS	Data collection from official court decisions	The number of death verdicts by state and year
WELFARE	Statistical Abstracts fro the United States 1973-1997	Welfare recipients and expenditures by state and year

D. Methods of Analysis

In a more comprehensive report available form the authors,[28] we describe our main analyses and a series of additional analyses that test the robustness of the findings. Here, we report on specific analyses that test the study hypotheses.

1. Descriptive

The initial stages of the analysis present basic data on trends over time in death sentences and relief rates for each of three stages of review.

2. Data Reduction

Data reduction and scaling occurred in several domains. Principal components factor analyses were used to construct variables for population characteristics, poverty rates, and court caseloads. Indices were constructed to estimate "effectiveness" of noncapital law enforcement policies and activities, as measured by states' rates of incarceration relative to the supply of FBI index crimes in the state. For individual or case-level data, we developed counts of aggravating and mitigating circumstances in the cases, and then subtracted the number of mitigating circumstances from the number of aggravating circumstances. We also created an additive scale of other attributes (e.g., defendant's prior record, victim gender and social standing) of cases that may also be aggravating factors in the case.

3. Model Estimation and Fitting

To estimate error rates for the state-level analyses, we use pooled data method to establish state-year data points, then estimated models using over-dispersed Poisson logarithmic and over-dispersed binomial logistic regression models with fixed and random effects. Pooling the data for each state across years creates "a set of new measurements rather than as a more extended set of data on the same observations."[29] Pooling data across time periods has the advantage of increasing the sample size for each model to N(T-1) cases, where N represents the total number of states and T represents the number of years of data in the model. With the full sample of states in these models at 34 for the analysis of three stages combined, and from 27 to 34 for the single-stage models, pooling the data over years greatly increases the sample size.[30] However, this method assumes that the variance over the pool, in this case across years, is constant for the sentencing rates in each state. This may not be the case here, because base rates are low and the rates grow unevenly by state. In order to account for variation over time, dummy variables for each year of data are included to represent the variance unique to each cross-section, or year. This pooled cross-section time series allows for the estimation of time-related effects in the models presented here. Time was estimated as both a fixed effect (time trend) and random effect (specific year) to improve precision of estimates of changes in error rates.

We applied over-dispersed Poisson and binomial regression procedures [31] to test hypotheses on factors that contribute to death sentences. Poisson regression is an ideal method of analysis when analyzing factors that predict *counts* of events, and determining the relationship of the rate to a set of explanatory or predictive variables.

Poisson models estimate the expected value of the number of events in relation to the causal factors and other explanatory variables of interest. For example, in the first set of models, we were interested in the count of events (reversals) in an area (state) relative to the base rates of factors that would influence these events (death sentences, homicide rates,

population and social structure, court resources and caseloads, overall appellate activity in the state, criminal justice expenditures, political pressure, punitiveness). The loglinear model was used for these analyses, run from SAS software's *GLIMMIX* macro.[32] Because we were comparing reversal rates, not numbers of reversed cases, we calculated offsets to permit use of Poisson analyses for model estimation. Binomial regression procedures are perhaps better-suited to the task at hand because they are appropriate for comparing rates and because they are ideal for comparisons of outcomes with a fixed potential range of 0 to 1.[33]

Also, to facilitate comparisons of differences in event probabilities across states, we computed exponentiated coefficients to illustrate the likelihood of an event occurring after controlling for the relevant base rate. Exponentiated coefficients are best understood as "odds" compared to a base rate. For models of appellate relief across all states and all stages of review combined, we used the number of capital verdicts in the trial court as the base rate. For single-stage models of appellate relief across states, we used the number of capital verdicts reviewed at that stage as the base rate.

To assess individual-level factors that contribute to trial error in federal habeas cases, we used mixed effects logistic regression models. In these analyses, we estimated the probability of affirmance or reversal at the federal habeas stage as a function of characteristics of offender, offense, victim, and court actors and procedures. The logistic function is the one estimated for these analyses, run from SAS software's *GLIMMIX* macro.[34] We again computed exponentiated coefficients to express the effects of changes in the predictors on the probability of detection of error.

4. Calculation of Effect Sizes

In addition to statistical significance, parameter estimates and fit statistics, we estimated predicted values of error rates by states over time for variables of interest (e.g., homicide rates), holding all other predictors at their centroids. The results of these effect size analyses were reported graphically for all predictors in a separate publication.[35]

Effect size shows 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, taking into account the simultaneous effects of other variables. 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 substantive or policy attention The procedure we use generates estimates of the increase or decrease in reversible error associated with each measurable increase or decrease in the explanatory factor. Our effect size estimates are coded as "newestimates" in the tables showing model results.[36]

We estimate effect sizes for key variables holding all other variables in the model at their mean.[37] To help readers interpret effect size estimates, we graph the predicted reversal rate associated with each of the range of values for particular explanatory factors of the states in the study. Note that the interpretation of effect size differs for binomial and Poisson regressions. Effect sizes in binomial regressions are the odds that the reversal rate will change given a change in the value of the predictor.[38],[39] Effect sizes for Poisson regressions show the change in the *rate* (not the odds) of reversal when a predictor increases by an amount x.[40],[41]

5. Compression of Zero-Inflated Data

We restricted our analysis to state years with at least one death verdict. For a death verdict to be reviewed it first must be issued. By restricting our analysis to years in which there was at least one verdict in any state, we examined only years where review is possible. This is akin in survey research to eliminating responses that are "not applicable." Once these non-death sentence years were excluded, zero reversals means that none of the death verdicts available for review (or reviewed) were reversed. Had we examined all years (including years in which no death sentences were imposed), a zero could mean that there were no verdicts to review, or that none of the available verdicts for review were reversed. Including the former would lead to biased and inaccurate computation of rates of serious reversible error, and would make model results uninterpretable. Thus, we restricted our analysis to state years in which review was possible. (Nevertheless, and notably, we did estimate the multivariate models with and without the years in which no death sentences were imposed. The explanatory factors that were significant and important were the same in both sets of models.)

Even with restricting the analysis to state years with at least one death verdict, we still had a substantial number of years with no reversals within states. To insure that our test statistics were not inflated, we used over-dispersed binomial and Poisson regressions. The analytic procedures permit adjustments for over-dispersion, thus generating more accurate test statistics and interpretation of results.

STATE-LEVEL FACTORS ASSOCIATED WITH REVERSIBLE ERROR IN CAPITAL TRIALS

A. Descriptive Data and Trends

The means, standard deviations and range for all variables used in the analyses are reported in Appendix A, Tables A.1 and A.2. We report descriptive statistics in two ways – with data points included for all years, and again excluding years when no states had any capital sentences reviewed. Correlation matrices are similarly reported in Appendix A., both with the full data set (Table A.3) and with the censored data set (Table A.4). Tables 10 and 11 on the following pages show trends in state-level error rates by year and state-level error rates by state. Each table decomposes error rates by stage, and then presents a combined error rate that reconciles errors across all three stages.

B. State-Level Analyses

This section presents the results of the main analyses of the sources of variation in error rates between states and over time.[42] The analyses are limited to states and years with at least one 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.).

We first report analyses using over-dispersed binomial logistic regression methods.[43] We then re-estimated the models using an over-dispersed Poisson logarithmic regression. One advantage of this alternate estimation method is that its results provide a more easily interpreted description of the expected change in reversal rates based on change in specific predictors. Each analytic technique provides unique information about the predictors and their effects on reversal rates. We report overall model fit for a baseline model with only state, time and year, and then model fit with predictors. All models were estimated first with a full set of predictors, and then re-estimated with a trimmed set of predictors to maximize model fit. We show the trimmed models in the sections below.

Table 10. State-by-State Comparisons of Direct Appeal, State Post-Conviction, and Federal Habeas Corpus Reversal Rates and Overall Error Rates, 1973-95*

State	% Reversed on State Direct Appeal	% Reversed on State Post-Conviction*	% Reversed in State Courts,	Percent Reversed on Federal	Overall Error Rate, Excluding State PC	Overall Error Rate, Including
Alabama	54	8	58	45	75	77
Arizona	41	11	48	60	77	79
Arkansas	40	4	43	48	69	70
California	32	4	35	80	86	87
Colorado	75	NA	NA	NA	NA	NA
Connecticut	100	NA	NA	NA	NA	NA
Delaware	30	unknown	unknown	0	30	unknown
Florida	50	17	58	39	69	75
Georgia	36	12	44	65	78	80
Idaho	40	5	43	67	80	81
Illinois	39	10	46	30	58	62
Indiana	27	27	46	50	63	73
Kentucky	52	0	52	100	100	100
Louisiana	47	5	50	27	61	63
Maryland	53	48	75	100	100	100
Mississippi	57	26	68	76	90	92
Missouri	17	4	20	15	30	32
Montana	15	9	23	75	79	81
Nebraska	36	14	45	43	64	69
Nevada	30	7	35	50	65	68
New Jersey	87	NA	NA	NA	NA	NA
New Mexico	25	NA	NA	NA	NA	NA
No. Carolina	59	10	63	18	67	70
Ohio	24	NA	NA	NA	NA	NA
Oklahoma	46	2	47	50	73	74
Oregon	88	NA	NA	NA	NA	NA
Pennsylvania	29	1	30	33	53	53
So. Carolina	54	18	62	14	60	67
Tennessee	29	16	41	100	100	100
Texas	30	6	34	25	48	51
Utah	36	21	50	33	58	67
Virginia	8	3	11	7	14	17
Washington	25	unknown	unknown	67	75	Unknown
Wyoming	25	33	50	50	63	75

* Because state post-conviction data are incomplete and reversal rates are calculated as proportions of verdicts available for review (not as proportions of actually reviewed verdicts), the figures in these columns are in most cases lower than the actual figure.[44]

Sources: DADB, DRCen, Appendix C, HCDB

Table 11. Relief Rate By Year of Death Verdict, Direct Appeal, State Post-Conviction, Federal Habeas, and all three Combined

	Direct Appeal Review		State Post-Conviction Review		Federal Habeas Review		Combined Relief Rates	
Year of Verdict	No. Reviewed	% Granted Relief	No. Available for Review*	% Granted Relief	No. Reviewed	% Granted Relief	State Courts	Overall
1973	17	59	2	29	4	50	61	80
1974	67	49	3	9	28	61	52	81
1975	84	39	16	31	38	82	55	92
1976	93	56	8	20	32	66	64	88
1977	131	51	8	13	48	50	59	80
1978	208	63	7	9	52	29	70	79
1979	179	50	15	17	52	44	65	80
1980	218	46	16	14	58	40	62	77
1981	257	41	30	20	75	36	71	82
1982	311	33	26	12	72	29	59	71
1983	278	31	22	11	49	33	53	68
1984	295	34	29	15	32	22	63	71
1985	306	44	27	16	26	38	71	82
1986	307	39	8	4	16	13	47	53
1987	272	41	15	9	8	0	56	56
1988	253	40	7	5	3	0	47	47
1989	199	47	4	4	2	50	51	75
1990	170	42	2	2	1	0	44	44
1991	149	36	5	5	0	0	41	41
1992	114	32	1	1	0	0	33	33
1993	69	30	1	2	0	0	31	31
1994	11	18	0	0	0	0	18	18
1995	2		0	0	0	0		0
Total	3990	41	252	11	596	40	47	68

* The number of state post-conviction reviews is unknown. We use the number of cases upheld on Direct appeal review as the number of verdicts available for review. This deflated the percent granted relief as not all verdicts available for post-conviction reviewed have been reviewed.

1. Probability of Reversal at All Three Review Stages Combined

This analysis identifies factors that explain 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. Time is included as a linear trend, which asks whether a pattern of increasing or decreasing amounts of error over time explains changes in reversal rates. As discussed earlier, 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. [45] In each analysis, including the analyses of decomposed stages below, we compare model fit statistics against a baseline estimate with only state and year to assess fit improvement resulting from the predictors.

For this analysis of review at three stages combined, we estimate models first using state and year as random effects, and again with only state as a random effect. In both analyses, all other predictors, including time trend, are treated as fixed effects.

a. State and year as random effects

Table 12 shows results of both types of regressions. Overall model fit at baseline (-2ResLL=1813.0) improves (-2ResLL=1726.7) with predictors. The effect of the time trend declines from the baseline to the full models, suggesting that the effects of time are more closely related to the presence of the explanatory factors in the models, and not to either changing rates of review over time or to the quality of death verdicts in later years of the study period.[46]

Table 12. Binomial Regression of Error Rates at All Three Stages of Review Combined (State and Year as Random Effects)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	1.7860	0.7561	21	2.36	0.0279	5.96573
2	yearn	-0.05694	0.02056	454	-2.77	0.0059	0.94465
3	ldor	0.6406	0.09402	454	6.81	<.0001	1.55894
4	BLTOT	-0.2017	0.01530	454	-13.18	<.0001	0.81738
5	FAC_CSLD	-0.2368	0.1242	454	-1.91	0.0571	0.78913
6	BLTOT*FAC_CSLD	0.04844	0.004972	454	9.74	<.0001	1.04964
7	lpctblack	0.8039	0.1777	454	4.52	<.0001	1.74581
8	lwbrtst	0.7015	0.3092	454	2.27	0.0237	1.62618
9	lpctblack*lwbrtst	0.1470	0.06840	454	2.15	0.0322	1.10725
10	PPINDX	0.2259	0.05818	454	3.88	0.0001	1.25343
11	lpnindx	-1.1875	0.2153	454	-5.52	<.0001	0.43908
12	PSST	0.5178	0.1821	454	2.84	0.0047	1.67826

Several factors influence error rates, controlling for time effects. Several systemic factors influence error rates in this analysis: aggressive use of the death penalty relative to homicide rates (LDOR), low capital case backlogs in the courts that hear death penalty cases and appeals (BLTOT), and lower general court caseloads (FAC_CSLD). The interaction of caseloads and backlog is also significant, suggesting that the combination of both high capital backlogs and a crowded court system generally produce higher error rates.

Several social and political factors also are associated with high error rates: states with higher proportions of African Americans in their populations (LPCTBLACK), states with higher populations and greater population density (PSST), states that have relatively inefficient criminal justice systems (LPNIDX), states where the rate of white homicide victims approaches the rate for black homicide victims (LWBRST), and states with higher political pressure on judges also have higher error rates (PPINDX). The interaction of white-black homicide victimization ratios with black population also is significant, suggesting a particular form of social threat or social conflict that is associated with the production of bad capital sentences. We re-estimated these models using a modified version of the Political Pressure Index (PPINDX2),[47] and obtained nearly identical results.[48]

Table 13 shows that consistent results were obtained when the models were re-estimated using over-dispersed Poisson logarithmic regression methods. Model fit here improves again from baseline (-2ResLL=1260.2) to the model with predictors (-2ResLL=1109.4). Error rates are likely to be higher when states aggressively seek the death penalty, where there is greater political pressure on judges, when there is a higher percentage of African Americans in the population, when the ratio of white to black homicide victims is higher, when the criminal justice system has a lower conviction-to-arrest ratio, and in states with higher and denser population structures. The same factors of backlogs and court caseloads also affect error rates across states.

Table 13. Poisson Regression of Error Rates at All Three Stages of Review Combined (State and Year as Random Effects)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	0.2074	0.4126	21	0.50	0.6204	1.23046
2	yearn	-0.04305	0.01744	454	-2.47	0.0139	0.95787
3	ldor	0.2863	0.04485	454	6.38	<.0001	1.21948
4	BLTOT	-0.1141	0.007494	454	-15.23	<.0001	0.89214
5	FAC_CSLD	-0.1411	0.05845	454	-2.41	0.0162	0.86839
6	BLTOT*FAC_CSLD	0.02691	0.002451	454	10.98	<.0001	1.02728
7	lpctblack	0.3937	0.08485	454	4.64	<.0001	1.31379
8	lwbrtst	0.3037	0.1545	454	1.97	0.0500	1.23431
9	lpctblack*lwbrtst	0.06631	0.03418	454	1.94	0.0530	1.04704
10	PPINDX2	0.1443	0.03258	454	4.43	<.0001	1.15524
11	lpnindx	-0.6626	0.1015	454	-6.53	<.0001	0.63172
12	PSST	0.2352	0.08798	454	2.67	0.0078	1.26519

b. Only State as Random Effect

In this section, we treat only state as a random effect. It responds to a simpler question than the previous analyses: What factors account for differences between a given state's 23-year of experience with capital reversals and the 23-year experience of all other states? This analysis assumes that reversal rates for all years in each state are 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. Here, we 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 of attention is on the various states' overall 23-year experiences with capital reversal rates, the fixed effect of the passage of time is also omitted.

To address this question, we cluster the 519 observed reversal rates (one for each relevant state and year) into 34 groups based on the state where the verdicts were imposed. From this, we explain differences among those states, without estimating year-within-state effects of the factors on the reversal rates. Comparing of the previous analyses (with state and year as random effects) with these analyses (with only state as a random effect), estimates the importance of time by gauging how much results differ when reversal rates are and are not clustered based on death-sentencing year. We again analyzed the data using both over-dispersed binomial regression and over-dispersed Poisson regression.

When only state is treated as a random effect, the results of the binomial regression in Table 14 reveal a pattern to results similar to results obtained when both state and year are treated as random effects. Model fit in the trimmed model (-2ResLL=1806.2) improves substantially compared to fit of the baseline model (-2ResLL=1952.0). The substantive results do, however, add to our understanding of factors that produce error in capital verdicts. One modest difference from the previous analysis (with state and time as random effects) is that the ratio of white to black homicide victims is not significant at the traditional $p < .05$ level in this model, and neither is its interaction with the minority population variable. (Both predictors do remain significant at the $p < .10$ level). Once again, states with more death verdicts backlogged in the courts waiting review had lower reversal rates. The difference between the previous analysis, with state and year as random effect, compared to this analysis with only state as random effect, implies that these homicide victimization variables are somewhat more important in predicting error in some years during the 23 year study period than in others.

The differences in Tables 14 and Tables 12-13 disappear when we re-estimate the model in Table 14 using over-dispersed Poisson regressions. We now obtain a result without time that is identical to the result obtained when both state and year are treated as random effects. The results in Table 15 show that both the ratio of white to black homicide victimization and the percentage of blacks in the state population are significant predictors of higher error rates at the .05 level. As in the binomial regression model fit in the Poisson regression for the trimmed model (-2ResLL=1111.2) is a substantial improvement over the fit of the baseline model
(-2ResLL= 1408.6). The substantive results again add to our understanding of factors that produce error in capital verdicts. The interaction of black population and white-black homicide victimization ratio is not significant in this model, although the separate variables are. Among the other predictors, the same pattern of significant predictors is obtained in this model.

Table 14. Binomial Regression of Error Rates at All Three Stages of Review Combined (State as Random Effect)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	1.1261	0.7675	31	1.47	0.1524	3.08346
2	ldor	0.8016	0.09562	477	8.38	<.0001	1.74302
3	BLTOT	-0.2221	0.01537	477	-14.45	<.0001	0.80080
4	FAC_CSLD	-0.2943	0.1236	477	-2.38	0.0177	0.74504
5	BLTOT*FAC_CSLD	0.05046	0.005430	477	9.29	<.0001	1.05175
6	lpctblack	0.8051	0.1759	477	4.58	<.0001	1.74722
7	lwbrtst	0.5660	0.3194	477	1.77	0.0771	1.48039
8	lpctblack*lwbrtst	0.1190	0.07153	477	1.66	0.0968	1.08600
9	PPINDX2	0.2604	0.06558	477	3.97	<.0001	1.29744
10	lpnindx	-1.5173	0.1670	477	-9.09	<.0001	0.34933
11	PSST	0.6079	0.1830	477	3.32	0.0010	1.83654

Table 15. Poisson Regression of Error Rates at All Three Stages of Review Combined (State as Random Effect)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	-0.04690	0.3655	31	-0.13	0.8987	0.95418
2	ldor	0.3992	0.04480	477	8.91	<.0001	1.31878
3	BLTOT	-0.1208	0.007139	477	-16.92	<.0001	0.88621
4	FAC_CSLD	-0.1764	0.05702	477	-3.09	0.0021	0.83827
5	BLTOT*FAC_CSLD	0.02744	0.002562	477	10.71	<.0001	1.02782
6	lpctblack	0.4222	0.08456	477	4.99	<.0001	1.33996
7	lwbrtst	0.4002	0.1569	477	2.55	0.0111	1.31969
8	lpctblack*lwbrtst	0.08201	0.03528	477	2.32	0.0205	1.05849
9	PPINDX2	0.1359	0.03243	477	4.19	<.0001	1.14551
10	lpnindx	-0.8040	0.07727	477	-10.40	<.0001	0.57277
11	PSST	0.3396	0.08646	477	3.93	<.0001	1.40433

These models with only state as a random effect closely replicate the results of models where both state and time are treated as random effects. Here, an alternate measure of homicide victimization among communities – a comparison of the homicide victimization rate of whites compared to the black homicide victimization rate – produces a similar effect regarding homicide that was evident in the previous analyses. Both homicide measures are 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.

Perhaps most important, this analysis suggests that 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.[49] The important differences to be explained in capital reversal rates are between states, not between years, or between earlier and later years.

c. Summary of Analyses of Combined Error Rates

These analyses tell a consistent story about the factors that produce higher error rates across states and over time.[50] First, greater backlogs lead to fewer reversals. But they also lead to fewer affirmances, since cases that are backlogged are by definition not yet resolved on appeal. The clogging of the system does not improve the quality of justice, it only delays it. 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.

Second, racial factors are important predictors of higher error rates. States with higher proportions of African-Americans in their population tend to have higher rates of capital error. And, 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. States with both a high proportion of blacks in their population and a high concentration of homicides on 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. All three racial factors tend to generate or amplify crime fears among members of politically influential communities.

Several empirical studies show that those fears may generate pressure on officials to adopt harsher crime control policies that may in turn 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.[51] The larger a state's African-American minority,[52] for example, 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.[53] 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.

Third, a poorer record of arresting and punishing serious criminals is strongly associated with higher capital-error rates. 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.

Fourth, and consistent with our hypotheses, more aggressive use of the death penalty is associated with higher capital-error rates. 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 hypothesize a reason for this relationship: 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. That is, the rate of death sentences per 1000 homicides is a residual measure of pressures to use the death penalty broadly in response to crime fears and other generalized factors apart from the seriousness of the offense. This overuse of the death penalty is a measure of pressures that is not captured, for example, by our racial predictors or measures of the effectiveness of law enforcement in controlling crime.

Fifth, and finally, heavier political pressure on state judges is associated with higher capital-error rates. 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. State judges, in particular, are selected in a variety of ways that might make them more or less susceptible to political pressure. 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 ones placing less to ones placing more political pressure on judges to conform their rulings to the desires of politically influential groups.

The importance of this factor offers an answer to a question posed by our interpretation of the other factors: How are such pressures generated by crime fears and other generalized pressures to use the death penalty broadly? The answer appears to be that the political process provides a mechanism for communicating and distorting those pressures to the legal actors who participate in capital sentencing.

2. Probability of Reversal at Separate Stages

Next, we used a similar logic to identify factors that predict reversal at each of the three stages of appellate review. Limiting analysis to one stage allows us to identify factors related to reversals as a proportion of death verdicts that were *reviewed* during the study period, and to determine if reversal rates at one review stage are influenced by a unique combination of predictors from each other or from the three stages combined.

The decomposition of relief rates by stage offers three advantages to the study. First, it allows us to see if results differ when we analyze *reviewed*, not *imposed*, verdicts. This is an alternate test, and one that could offer confirmation of the previous analyses based on imposed verdicts. Second, by examining factors that produce error rate at the single stages, we are able to check the substantive completeness and power of the explanations developed in the analysis of the combined stages of review. Third, some of the stage-specific analyses

more reliably assess the effect of time at the state direct appeal stage. As noted, analyses of the combined stages 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. In this analysis, examining reviewed verdicts, we avoid those biases. As before, we estimated these models using both over-dispersed binomial regressions and Poisson regressions.

a. State Direct Appeal

The N for this analysis is 453, which is the number of state-year combinations with at least one death verdict reviewed. As we show below in Tables 16 and 17, using *reviewed* verdicts that were reversed as the basis for these analyses, both models at this stage generate nearly the same set of significant explanatory factors as the analyses of the combined error rates. One exception is that the passage of time (which measures the changing levels of error) is now positively associated with error rates. We see here that, controlling for other factors, later verdicts are more likely to be reversed. Also significant in these models is a measure of the per capita expenditures in the criminal justice system within each state and year. This variable was not significant in the earlier models of combined review across all three stages.

Table16 shows results of the binomial regression of error rates of reviewed verdicts at state direct appeal. Overall fit of the model (-2ResLL) improves from 1522.2 to 1484.6 in the trimmed model. Similar results were obtained in the Poisson regression, shown in Table 17. Overall fit (-2ResLL) improves from 1018.6 at baseline to 993.4 in the trimmed model.

Table 16. Binomial Regression of Error Rates at State Direct Appeal based on Reviewed Verdicts (State and Year as Random Effects)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	-0.3712	0.6786	21	-0.55	0.5901	0.68992
2	yearn	0.1603	0.02627	390	6.10	<.0001	1.17385
3	ldor	0.3458	0.1038	390	3.33	0.0009	1.27085
4	BLTOT	-0.07932	0.009448	390	-8.40	<.0001	0.92374
5	lpctblack	0.3717	0.1566	390	2.37	0.0181	1.29390
6	lwbrtst	0.2074	0.1131	390	1.83	0.0674	1.15459
7	PPINDX2	0.1358	0.09748	390	1.39	0.1645	1.14541
8	lpnindx	-1.4151	0.2375	390	-5.96	<.0001	0.37497
9	ldir_exrt	-0.3375	0.08291	390	-4.07	<.0001	0.79140

Table _17. Poisson Regression of Error Rates at State Direct Appeal based on Reviewed Verdicts (State and Year as Random Effects)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	-0.9700	0.4914	22	-1.97	0.0610	0.37907
2	yearn	0.08524	0.01335	387	6.39	<.0001	1.08898
3	ldor	0.2254	0.05824	387	3.87	0.0001	1.16910
4	BLTOT	-0.04897	0.005392	387	-9.08	<.0001	0.95221
5	lpctblack	0.2347	0.1207	387	1.95	0.0525	1.17665
6	lwbrtst	0.4413	0.1984	387	2.22	0.0268	1.35778
7	lpctblack*lwbrtst	0.07444	0.04472	387	1.66	0.0968	1.05295
8	PPINDX2	0.09554	0.05056	387	1.89	0.0595	1.10025
9	lpnindx	-0.7137	0.1269	387	-5.62	<.0001	0.60977
10	PSST	0.2604	0.09837	387	2.65	0.0084	1.29749
11	ldir_exrt	-0.1921	0.04179	387	-4.60	<.0001	0.87534

Together, these analyses show several factors that contribute to error rates in states at the direct appeal stage. (a) States with lower court backlog have higher error rates. 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 discontent at how slowly capital cases move through the courts. (b) Error rates were lower in the earlier part of the study period. 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, after controlling for other factors. (c) The larger the relative size of the black community, the higher the rate of serious capital error found on state direct appeal. (d) 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. In other words, states with high homicide risks to their white communities relative to their black communities tend to have especially high reversal rates. This factor was significant in the Poisson analysis and close to significant in the binomial regression. (e) 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. (e) States with higher death-sentencing rates had higher rates of flawed death verdicts. (g) Political pressure on judges again contributes to high error rates, but the parameter for this effect is significant only in the Poisson regression, and then only at a more relaxed standard ($p < .06$). (h) Verdicts from highly and more densely populated states were more likely to be reversed than those from more sparsely populated states.

One factor that was significant in these single-state models though not in the three-stage combined analysis, is states' per capita direct expenditures on their court systems. 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. 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.

b. State Post-Conviction

The analysis of error rates at state post-conviction considers the number of reversals of death verdicts at this stage as a proportion of the number of verdicts available for state post-conviction review after being approved on direct appeal. The analysis 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. We use only 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, as well as the larger base rate—that is, the number of verdicts available for review, not the number actually reviewed.

As noted earlier, 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. 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 got stuck awaiting review at that stage. These limitations led us to doubt that we could obtain useful information by separately studying the state post-conviction phase. The diagnostics and model fit statistics from these analyses support these doubts to a degree, but less than we expected – the trimmed model fit the data (-2ResLL=1509.9) no better than the baseline analysis of state, year and time trend alone (-2ResLL=1509.6). Table18 shows the results of this model.

Table 18. Poisson Regression of Error Rates at State Direct Appeal based on Reviewed Verdicts (State and Year as Random Effects)

Effect	Estimate	StdErr	DF	t	P(t)	Newestim
Intercept	-6.0530	0.8006	22	-7.56	<.0001	0.00235
yearn	-0.09860	0.02075	301	-4.75	<.0001	0.90610
ldor	1.2831	0.2001	301	6.41	<.0001	2.43361
PCBL	-0.1431	0.01980	301	-7.23	<.0001	0.86666
WVRTST	0.1892	0.04030	301	4.69	<.0001	1.20824
PSST	0.9695	0.2258	301	4.29	<.0001	2.63664

Several factors contribute to higher reversal rates at the stage of state post-conviction review. (a) As before, 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). [54] (b) Also as in previous analyses, the year of the death verdict is a significant predictor of error rates. The significant negative relationship between the passage of time and reversal rates may reflect the fact that 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.

(c) Higher threat of homicides to the white community also contribute to higher reversal rates. This analysis uses a slightly different measure from the previous analyses of race differences in homicide victimization. Here, we use a measure of the homicide victimization rate among only members of the white community (WVRTST). A higher homicide risk to the white community is signficantly associated with higher reversal rates at the stage of state post–conviction review. A separate analysis[55] shows that a higher homicide victimization risk to the white community relative to the black community also is associated with higher state post-conviction reversal rates, although the relationship is not quite statistically significant ($p<.08$). Using either measure, capital error rates increase as the threat of homicide to the white community increases, politically potent pressures to use the death penalty may increase the likelihood that any given death verdict will be flawed. (d) Aggressive use of the death penalty again increases the risk of reversible error. The higher the number of death verdicts a state imposes per 1000 homicides, the higher its rate of serious capital error. Again, less judicious use of the death penalty is associated with higher rates of capital error. (e) Consistent with previous analyses, 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.

c. Federal Habeas Review

We analyze 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. 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. Accordingly, the total number of combinations of states and years examined in this analysis is 161. Because this analysis 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. The smaller number of observed reversal rates to be explained—161 state-years—makes it more difficult for the predictors to reach statistical significance, especially given the treatment of states and years as random effects. It is not surprising, then, that the trimmed model does not fit the reversal rate data any better or worse than the baseline analysis of only state, year and time trend. The fit of the baseline model (-2ResLL=627.6) is no worse than the fit of the trimmed model (-2ResLL=628.2). The results are shown in Table 19.

Table 19. Binomial Regression of Error Rates at Federal Habeas Review Based on Reviewed Verdicts (State and Year as Random Effects)

	Effect	Estimate	StdErr	DF	t	P(t)	Newestim
1	Intercept	-1.2952	0.6354	17	-2.04	0.0574	0.27384
2	yearn	-0.1279	0.03321	113	-3.85	0.0002	0.87991
3	PPINDX	0.3623	0.08360	113	4.33	<.0001	1.43657
4	PSST	-0.4393	0.1704	113	-2.58	0.0112	0.64451
5	FAC_WELF	0.8147	0.1655	113	4.92	<.0001	2.25858

Nevertheless, the analysis produces results that are consistent with previous stages and the reinforce the emerging narrative of factors producing elevated error rates in capital verdicts. (a) This model examines only reversal rates among death verdicts that were actually reviewed at the federal habeas stage, not all verdicts available for review. 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. Accordingly, as before, we interpret the effects of time cautiously. The parameter estimate for time in this model 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.

(b) States with high ratings on an index measuring the proportion of a state's population receiving welfare, its per capita expenditures on welfare, and its population structure (of larger and denser populations) have higher federal habeas reversal rates than other states.[56] (c) 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, and the effect is greater here than at any other review stage or at the three review stages combined.[57] (d) 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.

This set of explanatory factors weaves an interesting story regarding error rates in federal habeas cases. We noted above the reasons why political pressures on state judges may be more strongly associated with higher error rates at federal stages of review. The difference in the effect of population structure on state direct appeal (or post-conviction) review versus federal court review is predictable based on similar political factors. 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.

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. Overall, this analysis 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.

3. Summary of State-Level Effects

The comparisons and analyses 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—provide a reasonable and conservative basis to explain capital reversal rates. The factors — set out below in order of robustness—satisfy this conservative approach:

- 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.

- More densely populated states have higher reversal rates at the state court stages of review than do less populous states. At the third, federal habeas stage of review, the effect is reversed. At the first and second stage, though, when most reviews occur, capital error rates are sensitive to population size and density.

- 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. Having too many cases to decide likely means having too few resources to decide them either reliably or with the celerity that the public demands. It thus is reasonable to treat the two points as aspects of a single explanation: insufficient resources for capital trials.

- 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. Also, States where a high homicide risk to whites relative to blacks interacts with high numbers of blacks in the population relative to whites have especially high capital error rates. [58]

- 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.

- States that arrest, convict and imprison fewer criminals for every 100 serious crimes have higher rates of serious capital error than states bring fewer serious criminals to justice.

- States whose judicial selection methods give judges more of an incentive to conform their rulings to popular sentiment have higher capital 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.

- Death verdicts imposed by states with over-burdened and under-funded courts are more likely to be seriously flawed than ones imposed by states with average or better caseloads and funding. Courts with high capital and non-capital caseloads have high error rates. Poorly funded state courts generate more capital error.

- 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. 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. Analyses of error rates at State Direct Appeal are the most reliable measure of changing amounts of reversible *error* found over time, giving us confidence in this result. The result is important because direct appeal reversals are about 80% of all reversals between 1973 and 1995.

The interlocking factors can be unified into a single robust explanation for higher reversal rates in death sentences: excessive use of the death penalty as opposed to other responses to generalized fears of crime. 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.

FACTORS LEADING TO REVERSIBLE ERROR IN INDIVIDUAL HABEAS CASES

The final component of this study asks a different question: What are the traits of the *particular cases* in which death verdicts are approved or reversed? Case-level analysis is extremely labor intensive and expensive, requiring researchers to collect data directly from multiple sources. There is no public or private source in the nation that systematically and comprehensively collects and distributes such information for even for a single county, state or stage of review of capital cases. The only comprehensive source of information in each case(apart from the informed participants at each capital trial) are the transcripts and files. But those records run to thousands, or even hundreds of thousands, of pages in each case and are located in storage facilities in 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 fiscal and logistical capacity of the research community, and has never been attempted.[59]

The impossibility of tapping the original sources 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 where state and federal judges explain why they approved or reversed capital verdicts under review. Even here, the data collection task is enormous, given the multiple court jurisdictions in many individual cases which together aggregate to more than 7500 lower, intermediate and high court decisions reviewing death verdicts during the 1973-1995 study period. Each 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 that multiplies the number of decisions that must be read in each case—and it is hampered in some states by the absence of published state post-conviction decisions.[60]

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 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, where 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 observation 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.

A. The Habeas Study

We estimated models to explain 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 the probability of reversals of death verdicts on federal habeas review when other factors are considered simultaneously. We collected data on the 598 federal habeas cases from 1577 separate court decisions. 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.[61] An average of about three decisions were read in each case, because we extracted information, not only from the final federal habeas decision in each case, 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.

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 is better and more accessible information about particular conditions at the case than at the jurisdiction level. Not much can be learned about the attributes of capital 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. In addition, a case-level analysis can provide important information about whether factors that *were* effectively tested at the jurisdiction level also are effective predictors of outcomes at the case level.

Accordingly, 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. 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. Table 8 described earlier listed the categories of information that we assessed for each case, and Table 9 showed the variables that were constructed for the analysis.

After collecting and checking the information, then coding and checking the coding, we identified the set of traits of particular cases on which we had information in most or all cases, and as to which there was enough variance to analyze.[62] 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. 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 was present in each case: (a) 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;[63] (b) 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; and (c) the types of evidence introduced to prove guilt.[64]

Information on statistical significance, effect size (exponentiated B's) and fit is reported here. 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 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 thus 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. Some scores (*e.g.*, fit) also are sensitive to the number of explanatory factors being analyzed, requiring that number to be considered in assessing the significance of changes in scores from one analysis to another or in calculating the score itself.

B. Results

1. Descriptive Statistics

Descriptive statistics for this dataset are shown in Appendix B. The overall reversal rate for federal habeas cases is 40%. Table B.1 in Appendix B shows the means and standard deviations for the predictors used in this analysis. The number of cases available for analysis varies from 596 to 543, so the number of cases in each of the iterative analyses shown below vary as well. A few of the measures are skewed, meaning that the standard deviations are greater than the means for those variables. Table B.2 shows the correlation matrix for the individual-level variables included in the analysis of habeas review.

2. Model Estimates

Results of the first analysis of the individual-level data are shown in Table 20. This model examines the explanatory power of five conditions related to federal habeas reversals of capital verdicts.[65] The p-value indicates significance.

Table 20. Logistic Regression of Probability of Reversal in Federal Habeas Cases(595 Cases)

Factor	p-value	Effect Size
State Evidentiary Hearing Held	.008	.57
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.010	1.63
# of Statutory Aggravating Factors - # of Mitigating Factors	.017	.85
Index of 7 Other Aggravating Factors	.000	.76
Federal Evidentiary Hearing Held	.012	1.73

All the predictors in Table 20 are statistically significant. Federal habeas relief is predicted by two factors that measure the quality of the proceedings that determined the validity of the defendant's capital verdict, and two additional factors that express the extent of aggravation in the crime. A final factor – whether a federal evidentiary hearing is held –tests the legal strength of the defendant's challenges to the verdict. We then iterated this model two more times, adding additional factors that are potentially important factors in habeas decision making.

In the first iteration, we added one factor to the model in Table 20: the year the death verdict was imposed (SENTYR). We lost three cases from the analysis because the published decisions did not say when the verdict was imposed. The results are shown in Table 21.

Table 21. Logistic Regression of Probability of Reversal in Federal Habeas Cases with Sentence Year

Explanatory Factor	p-value	Effect Size
State Evidentiary Hearing Held	.020	.60
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.023	1.56
# of Statutory Aggravating Factors - # of Mitigating Factors	.011	.84
Index of 7 Other Aggravating Factors	.008	.81
Federal Evidentiary Hearing Held	.025	1.65
Year Death Verdict Was Imposed	.000	.86

Adding sentence-year does not alter the initial results. All previously identified factors remain significant, with similar effect size. The addition of sentence year confirms what we previously understood from the state-level analyses: 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. This occurs in part because of atypically long delays in deciding federal habeas appeals involving flawed verdicts.[66] Whether declines in reversals over time is also due to less error-prone verdicts or better state-court review procedures over time is unclear, and cannot be determined from these analyses. The fact that the state supreme courts found substantially more error over time on direct appeal, controlling for other factors, tends to refute the "less error" thesis and to support the "better state court review" thesis.

We next added the number of claims raised at the final federal habeas stage (CLAIMNO), a rough measure of the strength of the claims raised. This measure is important legally because the stronger any given claim is, the more pages lawyers will devote to the claim in their briefs, and in turn will have less space and inclination for adding additional 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). And, skilled lawyers are more likely to effectively increase the number of claims made.

Although there is no indication of how many claims were raised at that stage in 34 cases, leaving 559 to be studied, the factor is worth considering as another means of controlling for the strength of the claim. The results are shown in Table 22. As expected, the "number of claims" factor is significant and negatively related to reversal. The more claims raised, the lower the probability of reversal. The significance and magnitude of the effects of measures in the previous iterations of this model remain unchanged.

Table 22. Logistic Regression of Probability of Reversal in Federal Habeas Cases

Explanatory Factor	p-value	Effect Size
State Evidentiary Hearing Held	.051	.51
Defense Lawyer at Habeas Stage Is Not from Sentencing State	.016	1.63
# of Statutory Aggravating Factors - # of Mitigating Factors	.014	.84
Index of 7 Other Aggravating Factors	.033	.84
Federal Evidentiary Hearing Held	.014	1.77
Year Death Verdict Was Imposed	.000	.87
Number of Claims Raised	.000	.86

3. Summary and Discussion of Individual-Level Effects

These analyses suggest that federal habeas relief is predicted by two factors that measure the quality of the proceedings that determined the validity of the defendant's capital verdict, two factors that measure the strength of the case for a death sentence, and other factors that test the strength of the defendant's challenges to the verdict. We discuss the significance and implications of each below.

a. Quality of Review Proceedings

The first two factors—that a state evidentiary hearing was not held to test the validity of challenges to a capital verdict, and the quality and financial resources available to the defendant's federal habeas lawyer (as indicated by the lawyer's status as an out of state volunteer)— indicate that the quality of proceedings at each court stage is crucial: Just as low quality trials generate serious errors; low quality review procedures keep flaws from being found and cured.

If the state court review process after trial and before federal habeas review was of relatively high quality, given that before approving the verdict, the state reviewing court held a hearing to receive evidence about its validity, the probability that the verdict will be reversed on federal habeas is *lower*. 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.[67] Such hearings are not always required by state law, however, and state courts not infrequently fail to hold them, lowering the quality of the state court review process and 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. When viewed in the context of the state-level analyses, these results suggest that lower quality state

court proceedings—as indicated by state courts' low funding, high caseloads of capital and other cases, and unreliable methods of finding out what happened— increase the probability that death verdicts will later be reversed due to serious error.

Quality of defense counsel also predicts the probability of relief. If the capital defendant's federal habeas lawyer probably provided high quality, well funded representation, it is more likely that a federal habeas court will find serious error and reverse the verdict. Out-of-state federal habeas lawyers[68] tend to have substantially more resources at their command and come from a firm where lawyers are more highly trained or experienced capital appellate lawyers, than is true of in-state lawyers, When this condition is present and other factors are held at their averages, the probability of federal habeas reversal increases by about *two-thirds*, holding other factors constant at their averages.

Despite the indirectness of this measure of the quality of representation—which certainly misses some high quality, well-funded in-state lawyers[69] and may include some low quality or poorly funded out-of-state lawyers[70]—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 good deal.

b. Weak case for death

Tables 20-22 also shows 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. That is, when the number of statutory aggravating factors minus the number of mitigating factors is low, reversal rates are high. 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. 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 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.[71] Common mitigating circumstances are that the defendant was a juvenile at the time of the killing, acted under duress or extreme emotional disturbance, had no criminal record, or can be rehabilitated.

The level of aggravation is a strong predictor of the probability of reversal. Exponentiated coefficients for both the number of aggravating circumstances and the imbalance of aggravators over mitigators are .84. In other words, for each group of aggravating circumstances that we tested, the addition of one additional factor decreases the likelihood of reversal by 16%. Conversely, one additional mitigating factor increases the likelihood of reversal by a similar amount. This finding confirms what our state-level analyses showed: political pressures to impose death sentences that are not based on the seriousness of the crime increase the likelihood of reversal. In contrast, capital sentencing officials who limit death verdicts to highly aggravated cases have lower probability of error.

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 and mitigating factor 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 "aggravating net of mitigation."[72]

c. Strength of legal claims

When legal challenges to a capital verdict are stronger, the probability of reversal is greater. Controlling for this factor is critical, however, in order to be sure that the other important factors are not simply proxies for this condition. We find 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. In other words, the "federal evidentiary hearing" factor appears to provide a rough estimate of the strength of the claims raised. 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 do and do 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.

Accordingly, an important reason hearings are granted—and a condition linked to a greater chance of reversal—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 thus the more reliable the proceeding is, and the more likely it is that reversal will occur.

We also find that the more claims a capital prisoner raises in challenge to his or her death verdict in federal habeas appeals, the less likely it is that this verdict will be overturned. This factor is another measure of the weakness of the petitioner's legal claims.

d. Conclusion

After accounting for the strength of legal claims, death verdicts which 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. Also, the less aggravated the offense, the more likely it is that reversible capital error was found on federal habeas review. 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.

The effects discovered in the state 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.[73] The finding here that habeas reversals tend to occur in weak cases for a death verdict suggests that overall death-penalty policy prompts officials in some jurisdictions to use the penalty aggressively, even in marginal cases. And the factually weaker the case for death is, the greater the need to overreach and commit errors so that police can convince the district attorney to seek the penalty, prosecutors 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.

We found that neither the race of the particular defendant, nor the race of the victim nor a combination of the two is significantly related to the probability of federal habeas reversal. Thus, it is not when individual cases are being tried, but when overall death-sentencing policy is set, that the two racial factors discussed earlier 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 and white victims.

SUMMARY AND DISCUSSION

A. The Overproduction of Death

We begin with the 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, the broader the array of homicides to which they apply it, and the more frequently they apply it to homicides that are not highly aggravated—the greater the risk that any death verdict they impose will be seriously flawed.

We also reach supporting conclusions that emerge from the data analyses and 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: (a) the risk of homicide to the community, especially the risk to politically influential citizens approaches or exceeds that to other citizens, which we measure as the ratio of the homicide risk to whites relative to the homicide risk to blacks; (b) 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; (c) the interaction of (a) and (b), (d) well-founded doubts about the ability of the state's law-enforcement system to deal effectively with crime through arrest, conviction and incarceration; (e) state trial judges' susceptibility to being harmed politically if their capital rulings do not conform to popular sentiment; and (f) overtaxed and poor quality trial proceedings[74]—which are in part a function of heavy use of the death penalty—also appear to increase the risk of serious, reversible error. We discuss each of these factors in more detail below.

B. Pressures to Aggressively and Erroneously Apply the Death Penalty

Of these factors listed above, the first five are potential indicators 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, or both. Together with our principal finding linking heavy use of the death penalty to high rates of serious capital error, the strong association between these conditions and high capital-error rates leads us to conclude that each factor is an indicator of pressure capital jurisdictions and officials feel 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 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.

1. Inefficient and Ineffective Criminal Justice Systems

The less effective law enforcement is at capturing, prosecuting and punishing criminals, the more pressure affected individuals and communities are likely to put on officials to do more to fight crime. This is especially so when the crime people and neighborhoods fear is homicide, and when those in fear have the political influence to translate their concerns into effective public action. A response such political pressure

invites is expanded use of the death penalty as a visible demonstration of officials' intolerance for crime and commitment to punishing it severely. Because expanding the death penalty costs little at first—although eventually it requires very expensive 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 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. 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 reenforcing effect is the one our study documents: Higher rates and amounts of serious capital error.[75]

2. Political Pressures on State Judges

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. 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. 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 if they want to retain their seats or be elevated to a higher court—more often produce seriously flawed capital verdicts than courts whose judges are insulated from direct political influence by voters and contributors.

This finding is important because it reveals a way in which politically influential members of the public who are threatened by serious crime and doubtful about their state's response to it can pressure policy makers to publicly demonstrate that they care about and are aggressively responding to crime—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. 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. Thus, the sizeable effect of judicial selection techniques on capital error rates—a doubling or tripling of predicted error rates when selection methods placing the least and most political pressure on their judges are compared (other factors held constant)[76]— probably underestimates the effect of all types of political pressures on all capital officials.

3. 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 higher 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.[77] We consistently found that the greater the share of the homicide risk that is borne by whites relative to black, the higher the state's rate of serious capital error. Holding other factors at their averages, predicted reversal rates double or triple across the spectrum of conditions among states and years in our companion study.[78]

In some 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.[79] In 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 all but one analysis of state-level effects, the homicide rate exclusively among whites was not as powerful a predictor of error rates as the homicide threat to whites compared to blacks. This suggests 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 among whites and blacks—more specifically, whether the homicide risk to whites approaches or surpasses that to blacks, or on the other hand, whether blacks bear the brunt of the homicide risk.

The central finding of these prior analyses and the focus of their explanations is that, after controlling for other relevant variables, death verdicts are substantially more likely for homicides against white victims than against black victims.[80] 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?

The answer to this question comes from this study's principal finding: Jurisdictions that use the death penalty more often per homicide have higher capital error rates. The strong association between high error rates and policies leading to greater use of the death penalty in certain situations predicts that particular conditions prompting aggressive use of the death penalty may also be associated with high error rates. 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 that is borne by whites or other politically influential communities, the more pressure officials may feel to broaden the death penalty as a strong public demonstration of resolve to deal forcefully with homicides. That 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, resolve 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 cases than extremely aggravated ones through which to demonstrate that resolve. Expanded capital sentencing in response to crime fears thus almost inevitably invites capital verdicts in relatively weakly aggravated cases where the probability of serious error is the greatest.

Ironically, this helps to explain why capital error occurs just as often in black-victim as in white-victim cases. This is part of a pattern of results indicating that high capital-error rates are mainly associated with across-the-board capital-sentencing policies, not individual

decisions in particular (*e.g.*, white-victim, or especially aggravated) cases. 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.[81]

The results discussed here and in the previous section have a further implication. As a matter of first principles, law enforcement officials must do everything the law permits to lessen the threat of homicide to all residents of the jurisdiction. The results in this study reveal, however, 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 up in net and perpetrators go free), nor is it a strategy the law permits (because it multiplies reversible capital error), nor is it a strategy designed to protect all communities (because it responds to concentrations of homicide in only the white community).

4. Large Numbers of African-Americans and Welfare Recipients

We found, in two stages of review and over time, that the larger the proportion of a state's population that is African-American, the larger the state's rate of serious capital error. At the federal habeas stage, we found instead that the proportion of the state's population receiving welfare and its per capita cost predict higher rates of capital reversals. Like the race of homicide victims discussed just above, these racial factors are indicators of the pressure officials face to respond forcefully to crime. Problems in the administration of the death penalty arise when officials use the death penalty to respond to fears about crime. As we have seen, expanded and indiscriminate use of the death penalty is ineffective. When it occurs, the result is not more successful law enforcement, but instead a greatly increased risk of serious capital mistake, reversal and costly retrials.

We reach this conclusion sadly, given what it suggests about race relations. But we reach it with confidence. 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 high. 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 to demonstrate their firm resolve to fight crime: (1) low rates of apprehension, conviction and incarceration of serious criminals, and (2) a high risk of homicide concentrated on whites relative to blacks. Given consistent empirical evidence of a strong link between the size of the black (and the poor) population and the perceived threat of crime,[82] 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.

We also find that 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. 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, 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, resulting pressures on officials to broaden the availability of the death penalty and higher rates of serious capital error.

C. Heavy Court Congestion and Delay

We 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. This finding is predictable: Capital verdicts stuck in the review process can't serve the purpose for which they were imposed—and those that are flawed can't be corrected. But the findings have added significance in conjunction with the analyses' 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.[83] 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.

Delayed appeals also limit the amount of completed review, generating lower numbers of reversals. Indeed, delayed appeals also lead to lower rates of reversal for three separate reasons. 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. Although this rate is not the true error rate, which is the number of reversals as a proportion of reviewed, not imposed, verdicts, observers and the public are sometimes fooled into thinking that fewer reversals per imposed verdicts means fewer errors. 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.[84] Third, there is evidence that pileups of death verdicts awaiting review are disturbing to the public, generating pressure to move cases more quickly through the overburdened review process, with the result that fewer errors may be detected.

D. Overburdened and Underfunded Courts Have a High Risk of Capital Error

The analyses of state-level sources of error show that 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. At the same time, we also show that low per capita funding on the courts is also related to high capital error rates. 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 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.

At the two phases of review where individual-level 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.[85] 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.[86] 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 analyses of federal habeas outcomes also reveal 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. One reason why needed hearings are not held is state courts' inability to afford the costs of evidentiary hearings—e.g., counsel, witness and court reporter fees and expenses, 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 there are. 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 tightly this second supporting conclusion, like the first, is tied to our central theme and conclusion: More capital prosecutions and sentences means more strain on the system, more delay and more serious error.

E. The Review Process Probably Does Not Catch All Serious Mistakes

The results of the state-level analyses provide evidence that the appellate review process (like the trial process) responds to political pressures, rasing the risk that state-level review may not catch and cure all the serious error in the verdicts it inspects. Thus in addition to the political pressures on judges to impose death verdicts at trial in cases that do not meet the standards of high aggravation, the analyses suggest that the same political pressures associated with high error rates at trials supervised by elected judges may keep the same judges from correcting errors during subsequent state post-conviction review proceedings, and may discourage (without entirely stopping) elected state direct appeal judges from reversing verdicts based on the same errors. In particular, we find evidence in these analyses that state judges are moved to affirm flawed verdicts that (1) were themselves imposed as a result of political pressures generated by state judicial selection methods, (2) have piled up on appeal, and (3) are from rural areas where any given reversal is more likely to be controversial than in urban areas.

Analyses of federal habeas decisions similarly suggest that evidence that federal review judges are influenced by national political pressures associated with the process by

which they are appointed and promoted. Thus, the probability that a capital verdict will be reversed on final federal review seems to be related to whether review is by judges mainly appointed by Republican Presidents or by judges mainly appointed by Democratic Presidents.

F. Conclusion: the Probability That Innocent People Have Been Executed Is Unacceptably High

This study shows that the capital review process has not achieved the other goal of any inspection process: catching flawed products before they harm innocent people. 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. [87] Of the 101 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.[88]

As we discuss elsewhere,[89] it is impossible to know how many innocent people have been capitally convicted, sentenced and executed. The best researchers and policy makers can do, therefore, is to use available evidence to estimate the risk that innocent people have been executed. Our conclusion is the same as the conclusion of 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."[90]

The evidence in this study points in the same direction. Specifically, we found that rates of reversible error of 50% or more across nearly all states and years; deep-seated racial, political and other factors associated with that error; reviewing judges' inability to catch serious error even when it has caused an innocent person to be convicted and condemned; political pressures on reviewing judges to approve flawed capital verdicts; and high reversal rates persisting through the final review stage, not the steadily shrinking rates of discovered error needed to instill confidence in the efficacy of the review process.

Analyses by other researchers 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.[91] Moreover, among the events helping to save innocent inmates before being executed were a film maker's interest in one case, an investigation by college students in another, 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 case—fortuities that cannot be trusted to keep miscarriages from occurring.[92]

Together, these findings convince us, like Justice O'Connor, that the probability that an innocent person has been executed during the modern death-sentencing era is unacceptably high. The findings also convince us that lesser but still serious harms are widespread in the capital system, including the execution of individuals who were guilty of an offense but not one for which the law allows the death penalty.

POLICY OPTIONS

The study identifies a series of conditions associated with high rates of capital error. Each such error is serious. Serious errors cast substantial doubt on the reliability of a death verdict, cause delay, and inflate the cost of the capital system. Serious errors frustrate the expectations of victims, survivors, and the citizens and taxpayers who support and fund the capital system. Serious error the ability of the system of capital punishment to achieve its deterrent and retributive purposes. 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 being executed unless major policy changes are made.

A central goal of this study was to identify policy options for responding to serious capital error and the resulting risk that innocent people will be executed. We divide the discussion of these options into three sections. Section A discusses two options that can reasonably assure 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 outright. The next section discusses options that cannot promise to solve the problem of chronic error and 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 would probably magnify the problem of serious capital error.

We present the reforms discussed below as options among which death penalty states 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 discussing each option, we note the risks our analyses document that each reform option avoids or moderates. In a few cases, we identify options as close to a policy imperative in states to which they apply.

A. Attacking the Problem by Severely Curtailing the Death Penalty

Serious capital error is an ongoing problem with potentially severe effects and monumental costs, including executing the innocent. Worse, rigorous examination reveals that the conditions most strongly associated with capital error are not easily changed. For example, underlying most of the important explanatory conditions are fears about serious crime that pressure politically vulnerable 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. One source of such fears are well-founded doubts about jurisdictions' ineffective response to serious crime. These doubts evidently tempt policy makers to broaden the use of the death penalty as a visible demonstration of a will to combat crime. But attempting to cure across-the-board deficiencies in a state's law enforcement capacity by extending the death penalty to additional, marginally aggravated homicides is likely to divert tens of millions of dollars per each executed inmate from effective crime control into additional trials, multi-stage appeals and retrials that in the vast majority of cases will end in *non-capital* outcomes.

One way to moderate harmful conditions is to remove factors associated with them

that may be their cause. For the most part, however, this is not a viable solution to the problem of serious capital error. 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,[93] 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 are weakening.[94] It also is difficult to avoid officials' susceptibility to political pressures generated by fears about crime, the racial make-up of communities, the distribution of homicides among whites and blacks, or the distribution of the entire population among urban and rural areas. Even potentially causal conditions our analyses identify that *could* be changed—*e.g.*, by amending state constitutions to replace judicial elections with appointment for life, insulating judges from direct political pressure, or by increasing funding for overburdened courts—are unlikely to be changed any time soon.

We conclude, therefore, that the best that can be done is to try to limit the influences on capital-sentencing outlets of crime fears and political pressures from highly aggravated cases. The uncertain prospects for this approach are indicated by another study finding: The conditions that most strongly predict capital error operate mainly at the level of state death-sentencing policy, not at the level at which policy is applied to individual cases. The actions most associated with capital error are ones 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 ones, and open-ended interpretations of those definitions and factors on a statewide basis by state supreme courts and state's attorneys and by local sentencing courts and district attorneys.

B. Moderating the Problem Through Targeted Changes in Capital Policy and Practice

Some states may 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 sections lists policy options aimed at entire sets of cases, and applicable case by case.

1. Policy Goal: Limit the Death Penalty to Very Highly Aggravated Cases

The goal and basic design of any set of reforms are dictated by our principal finding: The more states 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. Therefore, the central goal of any set of carefully targeted reforms 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 calls for firm policies that (1) remove the death penalty from consideration in cases where the evidence or aggravation level 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.

2. Ten Ways to Moderate Error by Modifying Capital Policy and Practice

We begin with six options for categorically moderating the death penalty's use.

a. Require proof beyond any doubt that the defendant committed the capital crime.

The most effective way to bar death verdicts where the risk of error is great is to eliminate cases where there is doubt about the defendant's guilt. This would end the practice in most states requiring jurors to convict defendants of capital murder where they find guilt "beyond a *reasonable* doubt" but still harbor *some* doubt about the defendant's guilt.

The "beyond a reasonable doubt" formulation invites error because courts have been unable to narrow the kinds of doubts jurors can 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.[95] 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 know the defendant is the strongest suspect, may conclude that the "reasonable" thing to do is convict the defendant despite doubts about guilt.

When lengthy or permanent imprisonment is the result—allowing mistakes to be corrected whenever they are discovered—the risks from using the undefinable "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 chance 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.

Death-sentencing jurisdictions are split over how aggravated a first-degree murder must be, after accounting for mitigating circumstances, before a death sentence may be imposed. A small number of jurisdictions bar the death penalty unless aggravating circumstances *substantially* outweigh the mitigating circumstances, and unless the jury, in addition, is convinced that the death penalty is required by the amount of aggravation that remains after considering mitigation. These jurisdictions 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 calculated to avoid the high rates of unreliable error that our 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 death penalty by any amount, however minor or minuscule.[96] And still other states, such as Arizona, require the death penalty when aggravating and mitigating factors are *evenly balanced*, *i.e.*, unless mitigating factors actually outweigh the aggravating ones to some or even a significant extent—a

practice also followed in Kansas, until its Supreme Court recently objected, concluding that "fundamental fairness" demands that ties go the defendant when life is at stake.[97] It is hard to imagine policies more likely than these to inflate capital-sentencing rates—and thus rates of serious capital error—through imposition of death verdicts in marginal cases. A simple way to moderate the risks associated with serious capital error is to limit death verdicts to clear cases—ones where the jury finds that aggravation so far exceeds mitigation that only a death sentence will suffice—and to adopt model jury instructions that clearly inform jurors about the findings needed to permit a death verdict.[98]

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.

- ***Mentally retarded persons***. Because mentally retarded defendants are inherently weak candidates for the death penalty, they are prime candidates for serious capital error, reversal and retrial, and they are especially at risk of being convicted and condemned despite being innocent.

- ***Juveniles***. The Constitution bars executions for offenses committed by children 15 years old or younger,[99] and over a dozen states and the federal government ban the death penalty for offenses committed by teenagers below the age of 18. 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.[100]

- ***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.[101] 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 questions that determine the appropriate legal disposition of such cases.[102] A survey of cases in which death verdicts were reversed due to egregiously incompetent lawyering reveal a substantial number in which the problem was the lawyer's failure to develop evidence demonstrating that a defendant's severe mental disability is a defense to a capital conviction or sentence.[103] States that bar capital prosecutions when there is an expert consensus on the presence of psychosis or other severe mental disorders thus stand to avoid 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 [with sentence of life without parole] if they are so inclined."[104] Former federal judge and FBI Director William Sessions also recently advocated that jurors be "able to impose sentences, short of death, that they believe will protect society from the criminal."[105] 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.[106]

These findings identify two steps that together can effectively discourage death verdicts in cases where our analyses show the risk of capital error is 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 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.

e. No judge overrides of jury life verdicts.

If there is a single capital policy that most embodies the problems our analyses identify, 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*.[107] The policy is especially dangerous in states—Alabama, Florida and Indiana—that directly elect judges, and *most* especially in Alabama, which gives elected judges total discretion to overturn jury verdicts for any reason, without explanation.[108] 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 so susceptible to political pressures on judges whose reelection prospects are tied to their override records,[109] the resulting death verdicts fall simultaneously in several categories in which the risk of serious capital error is the greatest.

It is not surprising, therefore, 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,"[110] is much 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.[111] Also, the more political pressure a state's judicial selection methods 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.[112] Finally, researchers have found evidence that death verdicts imposed by judges in cases

where the jury voted for life are especially likely to be overturned on appeal.[113]

f. Use comparative review of murder sentences to identify what counts as "the worst of the worst" in the state, and overturn outlying death verdicts.

Our analyses indicate that over-broad capital charging, convicting and sentencing policies force capital appellate judges to function as substitute capital sentencers to winnow down the many capital verdicts imposed at trial to the few the evidence and aggravating circumstances clearly warrant.[114] Regrettably, almost no state appellate courts actually compare murder cases in which the death penalty is imposed to ones in which it is not imposed to assure sentencing consistency in like cases.[115] 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—ones for which the evidence is strong enough and the offense aggravated enough that death nearly always is imposed—and to distinguish verdicts imposed in those cases from "outlier" verdicts 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: (1) It 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 that identify the quality of evidence and level of aggravation that warrants capital treatment in the state. (2) It moderates high death-sentencing rates, and thus the high error rates associated with them, by quickly screening out the weak cases in which serious error is most likely. And, (3) the standards it sets gives prosecutors and trial judges a mandate and a way to resist political pressures to over-charge or to conform rulings to popular sentiment, further lowering the risk of error that those pressures otherwise create.

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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. The five options set out below take a different tack. Instead of placing entire sets of marginal cases off limits to capital outcomes, these options aim to improve the quality of decisions in *each case* by prosecutors, judges, jurors and defense lawyers, so they can more reliably separate marginal from core candidates for capital verdicts. Each option aims to increase the capacity of particular actors in the death penalty process to serve as a check on excessive capital charging and 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 all aspects of the system.[116]

g. Base charging decisions in potentially capital cases on full and informed deliberations.

Capital-charging decisions by a single prosecutor before all the evidence is in often commit jurisdictions to pursuing capital verdicts in cases where the evidence of guilt is not strong, or evident aggravating circumstances are substantially or completely offset by later-discovered extenuating factors.[117] Once a case is charged capitally, however, substituting non-capital charges, or the jury's imposition of a non-capital conviction or sentence, is 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 weak evidence and aggravation and strong mitigation.[118]

Capital statutes adopted recently by Congress and New York, and local capital-sentencing practices in places like Austin, Texas and Jacksonville, Florida,[119] have found a useful way to limit this problem. In those jurisdictions, a decision to proceed capitally in cases in which murder charges have been filed may not be made until three things have occurred: (1)
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; (2) defense lawyers, following their own investigation, have been invited to meet with prosecutors, review the state's evidence and their own and explain why capital charges are not warranted; and (3) 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.[120]

These steps can facilitate four goals to reduce the likelihood of serious error in capital cases: (1) replace the indiscriminate capital-charging policies that our study strongly associates with increases in capital error; (2) target capital charges on the strongest cases for a death verdict where serious error is least likely; (3) help local professionals use their own standards—and any added to 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 (4) foster improved law-enforcement, which also 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 about the strength of the evidence of guilt and about the amount of aggravation net of mitigation is the police and prosecution file in the case. Often, however, potentially important evidence in that file never reaches the jury. On the contrary:
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. The failure of police and prosecutors to disclose evidence before trial is an important reason why post-trial litigation over the reliability of capital verdicts takes an

average of 12 years from death sentence to execution. And, prosecutors' charging decisions are especially likely to ignore the weakness of the evidence of guilt and aggravation, and the strength of mitigation, when evidence in their own files that reveals those problems may be kept from public view.

One reason official suppression of important evidence is so common before trial, and so costly and contentious to litigate on appeal, is that the legal rule saying when prosecutors must turn over evidence is ambiguous and difficult to apply before trial. Under that rule, whether a police officer or prosecutor must turn over evidence indicating that the defendant may be innocent turns on a guess about how the evidence might or might not change events at trial that have not yet occurred.[121] 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 can decide whether to present it to the jury as evidence of weaknesses in the state's case or the strength of the accused's defense.

Any jurisdiction that relies on fully informed and responsible capital juries or judges to identify the "worst of the worst" cases, and to winnow out the rest at the conviction and sentencing stages, and yet does not insist that those decisions be informed by all the available evidence, takes a huge risk of producing serious error. 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, and keeps defense lawyers from doing their jobs at trial, open-files policies in capital cases are a policy imperative.

i. Insulate capital-sentencing and reviewing judges from political pressure, or use jurors to impose capital verdicts.

The results in this study show a clear connection between political pressures elections put on state judges and high rates of serious capital error. 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 each of which is associated with higher capital error rates. Other findings also reveal political pressure on elected state *appellate* judges to affirm seriously flawed death verdicts. Given these findings, each of the changes 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 review judges.
- Lengthen those judges' terms, whether they are appointed or elected.
- If judges are elected, use non-partisan elections.
- If judges are elected, use recall or retention elections, not contested, elections.

j. Identify, appoint and compensate capital defense counsel in ways that attract an adequate number of well-qualified lawyers to do the work.

Although competent and properly funded counsel can help avoid serious capital error, this crucial adversarial check on flawed capital trials has broken down in many capital jurisdictions:

- Egregiously incompetent lawyering—the only kind for which reversal is permitted—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.

- An important predictor of high error rates at the direct appeal review stage is poorly funded courts, which lead to inadequately funded capital defense, because funding for criminal defense comes out of local court budgets, and because states that resist spending money on their criminal courts are also likely to skimp on criminal defense.

- A link between low-quality lawyering and high rates of capital-sentencing [122] and resulting higher rates of serious error and conviction and condemnation of the innocent has also frequently been drawn.[123]

A recent and careful study of improved standards for appointing, compensating and providing support services to capital defense lawyers in Indiana (and by preliminary data on Oklahoma) shows that the routine provision of qualified and adequately funded capital trial lawyers leads to sharply lower death-sentencing rates, which our analyses, in turn, link to sharply lower capital error rates.[124] The Indiana study also documents that 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 that *are* imposed are confined to the kinds of cases in which the probability of serious error is the lowest: highly aggravated killings where the evidence of guilt is strong. Skilled lawyers are more likely to: (1)insist that the resources states need to spend on capital trials to avoid serious error are spent, (2) dissuade politically vulnerable judges from making the kinds of erroneous rulings that political, racial and other pressures otherwise tend to trigger, and (3) 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.[125]

The main cause of the break-down of the adversarial check in capital cases is a dangerous combination of extremely heavy demands on capital defense lawyers and minimal compensation.[126] The amounts of lawyer time and expert and investigative resources needed for an adequate defense in a capital cases are extremely high—many times those needed for the typical non-capital defense.[127] 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 a common burglary case.[128] Some states cap lawyer compensation for an entire capital trial at $5000, or even $1000.[129] 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, typically requires tens of thousands of dollars in support services.[130]

If jurisdictions rely on case-by-case responses to the problem of serious capital error and the consequent break down of the capital system, it is imperative that those responses include (1) standards assuring that only well-qualified lawyers represent capital defendants; (2) methods of appointing capital lawyers that avoid patronage considerations and rewards

to financial contributors to judicial campaigns; and (3) 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

Our study results not only suggest reforms that can help alleviate chronically high rates of serious capital error but also identify changes in existing practice that will not work, 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 may well have gotten worse over time. At the first, direct appeal review stage—the only stage that reviews all capital verdicts, and the stage responsible for 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. There is *no* reliable evidence that chronically high error rates declined over time—or that they will decline in the future in high-error jurisdictions that resist reform and stand pat.

2. Cutting Back on Review of Capital Verdicts May Increase the Ill-effects of Chronic Error and Lead to More Error

We noted earlier that the main changes in capital practice since the study period ended are sharp cut backs in the breadth of appellate review in places like Texas at the state post-conviction stage, and nationwide at the federal habeas stage. But, as New Mexico Governor and long-time death penalty supporter Gary Johnson recently said in withdrawing a proposal to limit capital appeals, this shoot-the-messenger strategy is an invitation to disaster—a change, in Johnson's words, that could "lead to innocent people being executed."[131] 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 poorly as a *substitute sentencer*, the review process nonetheless has come to serve a crucial role in screening out large numbers of unreliable death verdicts.

Especially if capital error rates continue to occur at anything like the rates during the 23-years 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. Moreover, limiting inspections could cause error rates to *rise*, by removing the only existing, if weak, deterrent to the conditions associated with error— 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 bad crime to which no existing aggravating factor applies.[132] New aggravating circumstances should be resisted on principle. Strong candidates for removal, given their tendency to vastly expand the reach of capital statutes and sweep cases that are not highly aggravated:

- 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 are part of the definition of murder and thus do nothing to assure that killings for which the death penalty is available are more aggravated than most— *e.g.*, that the killing occurred in the course of a robbery or other felony, or that the killing was premeditated—conditions that are present in *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."[133]

4. Large-scale Underwriting by the State of the Costs of Local Capital Prosecutions Invites Higher Capital Error Rates

The final category of reforms that our results strongly caution against are ones that largely replace local with state financing of capital prosecutions. We strongly link capital error to policies that motivate local officials to use the death penalty broadly, while displacing the post-trial cost of the errors these policies trigger onto taxpayers across the state and nation. If local officials can avoid most of the costs to themselves and their constituents of the initial trial—and, worse, of the retrials their errors require—local officials will have even less reason to use the penalty judiciously. And open-ended state subsidies will give local officials even more reason 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.

We agree with Illinois state's attorney Joe Birkett that "voters should review the record of county prosecutors in aggravated-murder cases and decide whether they're too aggressive—or not aggressive enough. Politics shouldn't enter into individual case decisions, nor should the race of the defendants or victims."[134] 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 five options in the prior section) while sharing their costs with local jurisdictions.

CONCLUSION

The death penalty has recently become a matter of rising concern among ordinary Americans. The public places great demands on the death penalty and yet has become increasingly aware that, as currently imposed, it is a costly failure that does not serve the purposes for which it was established and risks taking the lives of innocent people.

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 delays and huge court review and retrial costs. But it is more fundamentally true because by its very nature, and given the strong pressures on reviewing judges to approve even admittedly flawed verdicts, and given the strong bias of the rules governing review towards approving verdicts, reversible error is serious error. Such mistakes nearly always undermine the reliability of the verdict that the defendant committed the crime and that it was aggravated enough to warrant death as a punishment. Such mistakes often risk the execution of people who are innocent of the crime or at least of the death penalty. And such mistakes always frustrate the demands and expectations of the public who adopted the death penalty, the taxpayers who pay for it and those victims who most directly rely on it. We have taken as a research imperative, therefore, to identify the conditions and practices that are significantly linked to, and predict the occurrence of, serious capital error.

The central goal of this research 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? The most important conditions that predict sizeable differences in rates and amounts of serious capital error are capital-sentencing policies, not traits of particular officials, jurors, lawyers, defendants or victims. **The principal conclusion of this research is that heavy use of the death penalty, especially where it sweeps in cases where the evidence of guilt and the level of aggravation are not substantial, is a leading predictor of serious capital error.** Several other social, political, racial and systemic conditions strongly predict high rates of serious capital error, most of which are part of a system of policies that drive the capital sentencing system toward the overproduction of death sentences.

What responses do these findings suggest to the disturbingly high amounts and rates of that error that have characterized the capital system for decades? Policy changes can potentially be used to 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. Toward this end, we listed 10 specific policy options for limiting the overuse of the death penalty and moderating the resulting levels of error.

Such strategies require vigilant oversight, however. This is because the same state and local policy makers who developed the aggressive death-sentencing policies that so strongly predict serious error would have be relied upon to adopt 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 the officials to adopt the risky policies in the first place.

In some states, costs and frustration levels associated with the death penalty may be

so high that a more reliable solution to the problem of chronic capital error and its attendant costs and risks may be demanded. In those places, one option is to stop using the death penalty altogether. Another option is to limit its use to a small number of offenses as to which there is close to a social consensus that only the death penalty will serve.

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."[135] 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."[136] The state- 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. Now that a range of options are available to respond to the high levels of error in capital sentencing, it is time to either fix it or end it.

NOTES AND REFERENCES

1. Bureau of Justice Statistics, *Capital Punishment 2000*, NCJ 190598 (2001) (hereafter, BJSCP 2000).

2. 408 U.S. 238 (1972).

3. The Anti-Terrorism and Effective Death Penalty Act of 1996 ("AEDPA"), Pub. L. 104-132, 110 Stat. 1214 (April 24, 1996), which made a number of changes in preexisting federal habeas review, all designed to cut back on the amount of review and relief.

4. James S. Liebman, et al., A Broken System, Part II: Why There is So Much Capital Error and What Can Be Done About It, http://www.law.columbia.edu/brokensystem2; James Liebman, Jeffrey Fagan and Valerie West, A Broken System (Part I): Error Rates in Capital Cases, 1973-95,http://www.law.columbia.edu/instructionalservices/liebman/; Columbia Law School; James S. Liebman, Jeffrey Fagan, Valerie West, and Jonathan Lloyd,. "Capital Attrition: Error Rates in Capital Cases, 1973-1995." 78 *Texas Law Review*: 1839 (2000).

5. Bureau of Justice Statistics, Capital Punishment 2000, NCJ 190598 (2001), surpa n.1.

6. The reversal rate in some states, including ones with substantial death row populations such as North Carolina and Mississippi, were substantially higher. State habeas and other state post-conviction courts reversed some additional number of capital judgments. Data on those decisions is difficult to obtain, however, and is not included in this study.

7. The federal exhaustion of state remedies and procedural default doctrines assure that no capital judgment is reviewed in federal habeas corpus proceedings before first having been reviewed in a state direct appeal and/or post-conviction proceeding.

8. By way of contrast, consider Virginia death sentences. Because the Virginia Supreme Court has the lowest capital reversal rate of any state direct appeal court (8% compared to the national average of 41%), and the Fourth Circuit Court of Appeals has the lowest capital reversal rate of any federal habeas corpus court (7% compared to 40% nationally), the combined reversal rate for Virginia capital judgments was 14% compared to 63% nationally. Given the apparently low levels of scrutiny given Virginia capital cases, it is perhaps not surprising that the Supreme Court has found it necessary to review 10 Virginia capital judgments over the last five years—far more than either the number or ratio of capital cases reviewed from any other jurisdiction during the period. See, Liebman et al. (2002) supra n.4.

9. Although the state court and federal court reversals rates are nearly identical (40% and 39%, respectively), the fact that state courts review 100% of capital judgments, while federal courts only review something less than 60%, means that state courts account for many more actual reversals than federal courts.

10. John Sutton, Imprisonment and Social Classification in Five Common-Law Democracies, 99 *American Journal of Sociology* 350 (2000); David Jacobs and Ronald Helms, Toward a Political Model of Incarceration 102 *American Journal of Sociology* 103 (1996); Hubert Blalock, TOWARD A THEORY OF MINORITY GROUP RELATIONS (1967); Allen E. Liska, Mitchell B. Chamblin and Mark D. Reed, Testing the Economic Productivity and Conflict Models of Crime

Control, 64 *Social Forces* 119 (1984).

11. John Blume & Theodore Eisenberg, Judicial Politics, Death Penalty Appeals and Case Selection: An Empirical Study, *72 Southern California Law Review* 465 (1999); David Jacobs and Jason T. Carmichael, The Political Sociology of the Death Penalty: A Pooled-Time Series Analysis, 67 *American Sociological Review* 109 (2002); John Blume, Theodore Eisenberg, and Martin T. Wells, Explaining Death Row's Population and Racial Composition, *Cornell Law Review* (in press).

12. See, Liebman et al. (2002), supra n. 4. See, also, John Blume & Theodore Eisenberg, Judicial Politics, Death Penalty Appeals and Case Selection: An Empirical Study, 72 S. Cal. L. Rev. 465, 469, 485-86 (1999).

13. Jacobs and Carmichael, supra n. 11.; Sutton, supra n. 10.

14. Jacobs and Carmichael, supra n. 11

15. 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.

16. Supra n. 3.

17. See. Liebman et al., A Broken System (Part II), supra n. 4, 37-42.

18. James S. Liebman, et al., A Broken System, Part II: Why There is So Much Capital Error and What Can Be Done About It, http://www.law.columbia.edu/brokensystem2; James Liebman, Jeffrey Fagan and Valerie West, A Broken System (Part I): Error Rates in Capital Cases, 1973-95,http://www.law.columbia.edu/instructionalservices/liebman/; Columbia Law School; James S. Liebman, Jeffrey Fagan, Valerie West, and Jonathan Lloyd,. "Capital Attrition: Error Rates in Capital Cases, 1973-1995." 78 *Texas Law Review*: 1839 (2000).

19. If the reasons for reversal at those stages—with the cleanest cases —are clearly serious, there is reason to think that many or most errors caught at the first review stage also are serious. 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. Liebman et al. (2000), supra n.4, showed that 17% 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 not enough evidence to allow a reasonable person to conclude that the defendant was guilty. Exonerations following state post-conviction and federal habeas review were almost never for this most serious 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.

20. See the Wallace case discussed in Liebman et al. (2002), supra n.4 Appendix D, p. D-6.

21. See, Liebman, Fagan and West (2002), supra n. 4. 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	22%
Lawyer denied altogether	.25	.1%
(All denials of competent lawyer)	*(50.42)*	*(23%)*
Prosecutorial suppression of evidence	12.33	6%
Prosecutors' intentional exclusion of African-American jurors	3.33	1.5%
Other police, prosecutor misconduct	11.28	5%
(All police, prosecutor misconduct)	*(26.94)*	*(13%)*
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%
(All invalid instructions)	*(72.03)*	*(33%)*
Juror bias caused by third parties during trial	1.50	.7%
Juror bias due to pretrial influences	5.00	2%
Illegal exclusion of African-Americans from jury pool	8.50	4%
(All biased decision makers)	*(15.00)*	*(7%)*
Exclusion of exculpatory evidence	2.00	9%
Indigent defendants denied funds needed 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.

22. Liebman, Fagan and West (2000), supra n.4. 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 U.S. 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).

23. For full information and summary tables showing the outcomes of reversed cases, see: Liebman, Fagan and West (2000), and Liebman et al. (2002), supra n. 4.

24. During the study period, verdicts finally reviewed on federal habeas much more time under review in state and federal court in later years than in earlier years. The average review time from sentence to final habeas review rose from *5½ years* for verdicts finally reviewed in 1981, to *12 years* for verdicts finally reviewed in 1995. Also, federal habeas review of cases in which relief was granted averaged about 13 years, compared to 11 years for cases where relief was denied (Liebman et al., 2002),supra n.4.

25. See, Liebman et al (2002), supra n.4, for full details of the consideration of time trend.

26. Neither do the states maintain "snapshot" lists of persons on death row at given moments during the period.

27. See, Liebman et al. (2002), supra n. 4.

28. See, Liebman et al., (2002), supra n. 4.

29. Ronald Kessler and David Greenberg, LINEAR PANEL ANALYSIS: MODELS OF QUANTITATIVE CHANGE 158 (1981). See, also, Lois Sayrs, POOLED TIME SERIES ANALYSIS, (989).

30. The number of states having executed criminal defendants as of the end of data collection in 1995.

31. P. McCullagh and J. Nelder, GENERALIZED LINEAR MODELS (1989); William H. Greene, ECONOMETRIC ANALYSIS (2nd edition) (1993); Peter Kennedy, A GUIDE TO ECONOMETRICS (4th edition) (1998).

32. The procedure is PROC MIXED, applying the *Glimmix* macro for generalized linear models with mixed effects. SAS, Inc., Cary, NC. See, for example, Judith Singer, "Using SAS PROC MIXED to Fit Multilevel Models, Hierarchical Models, and Individual Growth Models," *Journal of Educational and Behavioral Statistics*, 24(4). 322-354. (1998)

33.SAS, Inc., id.

34. SAS, Inc., id.

35. See, Liebman et al. (2002).

36. 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 substantive or policy 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). 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.

37. The formula used to calculate effect size in the graphs of factors in the binomial analyses is (all values for the factor of interest) x (that factor's parameter estimate (coded "Estimate" in the tables)) + (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.

Assume that the homicide rate increases from *5* to *6* per 100,000 residents, and that the predicted reversal rate where the homicide rate os *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 x 1.4), or .46 (46%). Where the homicide rate is *7* per 100, 000 residents, the predicted reversal rate is (.33 x 1.4 x 1.4), or .65 (65%). And so on.

38. 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 x 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 x 1.4 x 1.4) to 2, or 1.96 to 2, which is equivalent to a 49% probability.

39. 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 $10 = 10^2$.) 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 x 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 tables). The method we used to calculate the "newestestimate" 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_clsd" 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.

40. 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) x (that factor's parameter estimate (coded "Estimate" in the tables)) + (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 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.

41. 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 "newestestimate") 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 x 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 tables). 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_clsd" 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.

42. For additional, confirmatory state-level analyses, and additional analyses of country-level reversal rates, see Liebman et al. (2002), supra n. 4.

43. All of our binomial analyses are over-dispersed and logistic, even where we omit those descriptions for ease of exposition.

44. In Tennessee, for example, where a local newspaper collected full state post-conviction data on reversals as a proportion of the number of fully reviewed verdicts, our reversal rate estimate (16%) was less than one third the actual reversal rate (51%) reported by the newspaper. See, Liebman et al. (2002), supra n.4.

45. 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.

46. Details of parameter estimates from the baseline analyses are available from the authors upon request.

47. In order to account for the short duration of many appointments, a second scale of politicial pressure (PPINDX2) was constructed that included the length of judges' first elected term, or the longer of retention terms. For example, an appointed first term of 1 year followed by an election term of 15 years is considered a 15 year first term, and scaled as a 1 to reflect lower political pressure.

48. See, Liebman et al. (2002), supra n. 4, Analysis 1B.

49. 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.

50. These results were confirmed by additional state-level analyses of reversal rates at single-stage of review, including where reversal rates were measured as proportions of reviewed (instead of imposed) verdicts, and also in a variety of county-level analyses of reversal rates at all three stages combined and at single stages of review. See, Liebman et al. (2002), supra n. 4.

51. See, Jacobs and Carmichael, supra n. 11, and Sutton, supra n.10.

52. The highest proportion of a state's population that was African-American in any state and year in our study is 36%.

53. Strong empirical support for this proposition is found in existing social scientific research. See Steven 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); 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"; finding link between racial stereotypes associating blacks with past and assumed future violent crimes and support for harsher punishments).

In general, the social scientific literature here is voluminous and consistent in regard to then tendency on the part of white individuals, including actors in the criminal justice and trial system, to, in Professor Randall Kennedy's phrase, "use race as a proxy for an increased risk of criminality." Randall Kennedy, RACE AND CRIMINAL LAW (1995) at 136-67(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. See, also, 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); 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); Brit 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).

54. 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.

55. See Liebman et al. (2002), supra n.4, Analysis 5A.

56. This factor replaces the effect of the race of the general population but the two factors may function similarly in terms of the threat of crime perceived by politically influential communities. Put differently, state court review for error appears to pick up flaws generated by racial pressures, leaving flaws associated with more subtle welfare-related pressures to be corrected at the final, federal habeas review stage.

57. This again is predictable. Because federal habeas judges are appointed pursuant to a process attuned to national political influences on the President and U.S. Senate, they should be more willing and able than state judges to overturn verdicts that come to be flawed as a result of political pressures generated by the methods of selecting state judges.

58. In two analyses, states with higher general homicides rates have higher capital error rates. This supports the finding that relatively high homicides rate among whites compared to blacks (the fifth of the seven key factors) is 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 high 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. See supporting references supra n.10, 11, and 53.

59. 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. Baldus, Pulaski & Woodworth, Comparative Review of Death Sentences: An Empirical Study of the Georgia Experience, 74 *J. Criminal Law & Criminology* 661 (1983); Baldus, Pulaski, Woodworth & Kyle, Identifying Comparatively Excessive Sentences of Death: A Quantitative Approach,33 *Stanford Law Review* 1 (1980); Baldus, Woodworth & Pulaski, Arbitrariness and Discrimination in the Administration of the Death Penalty: A Challenge to State Supreme Courts, 15 *Stetson Law Review* 133, 158 (1986); and, Baldus, Woodworth & Pulaski, Monitoring and Evaluating Contemporary Death Sentencing Systems: Lessons From Georgia, 18 *U.C. Davis Law Review* 1375, 1404 (1985). No similar study, however, has reached across state boundaries and decades.

60. 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. See Liebman, Fagan and West (2000), supra n. 4.

61. See 28 U.S.C. §§ 2254(b), (c).

62. 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 this 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.

63. Aggravating and mitigating factors are treated somewhat differently by law, which accounts for our somewhat non-parallel treatment of them in this index. See, for example, Jeffrey L. Kirchmeier, Aggravating and Mitigating Factors: The Paradox of Today's Arbitrary and Mandatory Capital Punishment Scheme, 6 *William and Mary Bill of Rights Journal* 345 (1998). 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.

64. Scores on this index were not significantly related to the probability of federal habeas reversal.

65. The relevant factors are coded as follows in the detailed results in Liebman et al. (2002), supra n.4, 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").

66. YEAR OF VERDICT is not, however, a reliable indicator of declining amounts of error, after controlling for other factors. During most of the study period, and all of its later years, federal habeas reversals of verdicts imposed in a given year took about one and one-half years longer to occur than federal habeas affirmances of verdicts imposed in the same year. 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 the data reflect to decline over time. 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 higher quality death verdicts over time.

67. 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.

68. These lawyers almost always are teams of partners and associates from large corporate firms in big cities. See, Liebman et al. (2002), supra n.4, for a full discussion of the types of counsel most effective in federal capital appellate hearings.

69. 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.

70. 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.

71. See, for example, discussion of state statutes in Kirchmeier, supra n. 63.

72. 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.

73. See, Liebman et al. (2002), surpa n. 4.

74. Overtaxed and poor quality trial proceedings result from low levels of financial support and the combination of high capital and non-capital cases.

75. 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.

76. See Liebman et al. (2002), supra n.4, Figures 29A-D, 37A, 37B, 40C-1, 40C-2, 41H, 43I, 43J, 44E.

77. 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 pp. xxxx and n.xxx, the homicide rate among blacks is usually higher than among whites. In most states, that is, this factors compares states based on how much lower the white homicide rate is than the black homicide rate—or how closely the white homicide rate approaches the black homicide rate.

78. See Liebman et al. (2002), supra n.4, Figures 34A, 34B.

79. See Liebman et al. (2002), supra n.4, Analysis 7, 15 and 18. Data not shown here, available from authors.

80. 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."); 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); 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).

See, e.g., McCleskey v. Kemp, 481 U.S. 279 (1987) (opinion of Stevens, J., dissenting) (summarizing relevant studies); David C. Baldus, George Woodworth & Charles A. Pulaski, Jr., Reflections on the 'Inevitability' of Racial Disrimination 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).

81. 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. See, Liebman et al. (2002), showing that no *county-level* factors were significantly related to the countywide distribution of the homicide risk between whites and blacks and county reversal rates.

82. See, for example, Jacobs and Carmichael, supra n. 11; Blalock, supra n. 11; Sutton, supra n. 10; Liska, et al., supra n.10.

83. Liebman et al. (2002), supra n.4, estimate that 15-20 death verdicts within a court system can drastically slow down the processing of capital appeals.

84. See Liebman et al. (2002), supra n.4.

85. See Liebman et al. (2002), supra n.4; and Liebman, Fagan and West (2000), supra n.4.

86. 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.

87. Liebman et al. (2002), 25-35. See, also, James Dwyer, Peter Neufeld and Barry C. Scheck, ACTUAL INNOCENCE (2000); C. Ronald Huff, Huff, CONVICTED BUT INNOCENT : WRONGFUL CONVICTION AND PUBLIC POLICY (1996); Hugo Beadau and Michael Radelet, IN SPITE OF INNOCENCE : ERRONEOUS CONVICTIONS IN CAPITAL CASES (1992).

88. See Dwyer et al., id.

89. See, Liebman et al. (2002), supra n.4.

90. Brian Bakst, O'Connor Questions Death Penalty, Associated Press, July 2, 2001. See, also, 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; 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)).

91. See, Jo Thomas, id.

92. See James S. Liebman, Overproduction of Death, 100 Colum. L. Rev. at 2050-51 n.84 (collecting sources); Dwyer et al., supra n. 87.

93. See, Alfred Blumstein and Joel Wallman (eds.) THE CRIME DROP IN AMERICA (2000). See, also, Jeffrey Fagan, Franklin Zimring and June Kim, "Declining Homicide in New York: A Tale of Two Trends." 88 *Journal of Criminal Law and Criminology* 1277 (1998); Franklin Zimring and Jeffrey Fagan, "The Search for Causes in an Era of Crime Declines: Some Lessons from the Study of New York City Homicide." 46 *Crime and Delinquency* 446 (2000).

94. 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 xx ("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.").

95. 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.

96. See, e.g., Boyde v. California, 494 U.S. 370 (1990); Blystone v. Pennsylvania, 494 U.S. 299 (1990).

97. 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); See Walton v. Arizona, 497 U.S. 639 (1990).

98. 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 Dll (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); see also Luginbuhl & Howe, supra note 29, at 1177 (finding, based on North Carolina data, "considerable lack of comprehension" regarding standards of proof for aggravating circumstances and that close to one-half or more of the capital jurors interviewed mistakenly believed that judicial instructions had authorized them to rely on *any* aggravating circumstance, whether or not it was enumerated in the statute, even though state law forbade reliance on nonstatutory aggravating circumstances).

99. See Thompson v. Oklahoma, 487 U.S. 815 (1988).

100. See, e.g., Johnson v. Texas, 509 U.S. 350 (1993); Eddings v. Oklahoma, 455 U.S. 104 (1982).

101. See, e.g., the Wallace case, cited in Liebman et al. (2002), supra n.4, 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.

102. Typically, such questions include: : 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?

103. Such cases are also unusually expensive to review on appeal, given the need for more costly examinations and testimony by qualified physicians and mental health professionals. And given the wide range of legal challenges to death verdicts that arise solely because the defendant is severally mentally disordered, such cases present many more reasons why death verdicts may be overturned—*e.g.*, that the defendant was coerced into confessing, 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 or expertise in dealing with the issues severely disordered defendants present.

104. James Q. Wilson, What Death Penalty Errors?, N. Y. Times, July 10, 2000, at A19.

105. 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).

106. 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 alternative to death); 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).

107. 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).

108. 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.

109. 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.(investigative report documenting influence of politics on judicial overrides in Alabama).

110. See Dobbert v. Florida, 432 U.S. 282, 296 (1977).

111. 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, August 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, to Death Penalty Dialogue Project, 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, 1of 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 to Death Penalty Dialogue Project, 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, to Death Penalty Dialogue Project, 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).

112. As the data in n. 111, id., reveal, the disproportionate use of jury override 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. electoral discipline. In the first regard, compare Florida before 1995, and Alabama throughout the modern death-sentencing era, which gave trial judges relatively free reign to override jury verdicts, to Florida since 1995, 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 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, not death; 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 n.111, at 1049 (emphasis added). 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 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 their scores on our two indexex of the political pressure judicial selection techniques place on judges (with higher scores indicating more political pressure). As over analyses would predict, judges in states where judicial selection methods make judges more politically 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).

113. The Florida experience documents 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) (reporting 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).

114. The 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 a *procedural* error such as incompetent lawyer, suppressed evidence or a bad jury instruction. Absent a procedural error, appellate courts usually are not authorized to cure even very serious mistakes in the bottom-line outcome of the trial.

115. See, e.g., John Shipman, Tennessee article on foregone review, supra n. 44.

116. This point was succinctly made recently by Indiana University Law Processor Henry C. Karlson, "a staunch supporter of capital punishment" and frequent advisor to state lawyers defending capital cases. 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). "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." According to Karlson, financial savings are not the only reason to spending 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."

117. See, e.g., sources catalogued in 100 Colum. L. Rev. 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 review lengthy documents detailing the defendant's upbringing, criminal history and mental health")

118. See, e.g., sources catalogued in *id*

119. See, e.g., sources catalogued in *id.*

120. These proposals track the advice of Joe Birkett, President of the Association of Government Attorneys in Capital Litigation and a Chicago-area prosecutor. Birkett recommends (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. See, also, Lise Olsen, 1 Killer, 2 Standards, Sidebar: Solutions, Seattle Post-Intelligencer, Aug. 7, 2001 (listing reforms proposed by State's Attorney Birkett).

121. The standard invites prosecutors to withhold evidence of innocence or mitigation if, in their judgment, there is no "reasonable probability" that but for their suppression of the evidence, "the outcome of the trial"—which of course has not yet taken place—"would be different." See xxxx.

122. 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); Rosenberg, supra note 57, 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 that elite office is permitted to handle, 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 Ken Armstrong & Steve Mills, Inept Defenses, supra note 64 (quoting University of Michigan Law Professor and accomplished capital defense attorney Andrea Lyon: "Who your lawyer is is the single most important fact deciding whether you get the death penalty or not."); Bright, Counsel for the Poor, supra note 52, at 1841 (May 1994) ("Whether death is imposed frequently turns on the quality of counsel assigned to the accused."); 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.").

123. 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., Armstrong & Mills, Inept Defenses, supra note 64 (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.

124. 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 state commission's guidelines for appointment of qualified counsel in capital cases, and 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 [ditto]" 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.").

125. 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 under-funded." 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. xx (1996) (hereinafter "ABA Report"). For other sources who have comprehensively documented the limited resources available for capital defense, see, e.g., Coyle, supra note 52; Norman Lefstein, Reform of Defense Representation in Capital Cases: the Indiana Experience and its Implications for the Nation, 29 Ind. L. Rev. 495 (1996); 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, August 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); other sources cited supra notes 52, 57.

126. 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." Bruce A. Green, Lethal Fiction: The Meaning of "Counsel" in the Sixth Amendment, 78 Iowa L. Rev. 433, 491-92 (1993); see also Bright, Counsel for the Poor, supra note 52, at 1853-55 (comparing wages of state appointed attorneys in capital cases to their vastly greater earning potential when working on other types of litigation); Rimer, supra note 57 (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").

127. See, Liebman et al. (2002), supra n.4, for authorities. 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 murders, usually often 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. Accordingly, capital investigations and trials are more complicated. Also, 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 given to the judge by a court official in a "presentence report." Both phases of capital trials are governed by highly complex sets of specialized statutory and federal constitutional rules that have no analogue in noncapital cases, and require litigation specialization. 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.

128. 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").

129. See, generally, Gary Goodpaster, The Trial for Life: Effective Assistance of Counsel in Death Penalty Cases, 58 N.Y.U. L. Rev. 299 (1983). See details of economic dimensions of capital defense, e.g., Marcia Coyle, et al., Fatal Defense: Trial and Error in the Nation's Death Belt, Nat'l L.J., June 11, 1990, at 30 ("Wholly unrealistic statutory fee limits on defense representation—such as Mississippi's flat, unwaiveable $1,000 cap [on compensation for capital defense lawyers], equivalent to a fee of about $5 per hour for many lawyers [a provision that remained 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."); 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) (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); Dirk Johnson, Shoddy Defense by Lawyers Puts Innocents on Death Row, N.Y. Times, Feb. 5, 2000, at A1 (providing evidence 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; Tina Rosenberg, Deadliest D.A., N.Y. Times, July 16, 1995 (Magazine), 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).

130. 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).").

131. See, 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.'"

132. But long lists of statutory aggravating factors—an attribute of Illinois's capital statute that some have linked to its high rates of capital error— establish just the kind of broad death-sentencing policy that are associated with high error rates. See Editorial, Ryan's Tough Call on Executions, Chi. Trib., Aug. 28, 2001 (supporting 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

APPENDICES

A. Descriptive Statistics for Variables in State-Level Analyses

B. Descriptive Statistics for Variables in Individual-Level Analyses

APPENDIX A.

Table A.1. Means and Standard Deviations for Predictors of State-Level Error Rates, All Years

	N	Minimum	Maximum	Mean	Std. Deviation
BLTOT	657	0	49	4.38	7.173
FAC_CSLD	657	-1.03255	3.09439	.0000000	1.00000000
FAC_WELF	657	-1.59507	5.06914	.0000000	1.00000000
LDOR	657	-1.0000	2.3188	.735572	.9733110
LDR_EXRT	657	3.3010	5.8922	5.101190	.5627723
LPCTBLK	657	-2.6272	-.4412	-1.127237	.5175050
LPNINDX	657	-.0251	1.1134	.608066	.2162066
LWBRTST	657	-3.0000	.0534	-.834409	.5803048
PCBL	657	0	27	3.18	4.875
PPINDX	657	2	9	6.59	1.725
PPINDX2	657	2	8	5.70	1.443
PSST	657	-2.49135	1.75363	.0000000	1.00000000
YEARN	657	0	22	12.71	5.787
Valid N (listwise)	657				

Table A.2. Means and Standard Deviations for Predictors of State-Level Error Rates, Years With Death Verdicts Only

	N	Minimum	Maximum	Mean	Std. Deviation
BLTOT	657	0	49	4.38	7.173
FAC_CSLD	657	-1.03255	3.09439	.0000000	1.00000000
FAC_WELF	657	-1.59507	5.06914	.0000000	1.00000000
LDOR	657	-1.0000	2.3188	.735572	.9733110
LDR_EXRT	657	3.3010	5.8922	5.101190	.5627723
LPCTBLK	657	-2.6272	-.4412	-1.127237	.5175050
LPNINDX	657	-.0251	1.1134	.608066	.2162066
LWBRTST	657	-3.0000	.0534	-.834409	.5803048
PCBL	657	0	27	3.18	4.875
PPINDX	657	2	9	6.59	1.725
PPINDX2	657	2	8	5.70	1.443
PSST	657	-2.49135	1.75363	.0000000	1.00000000
YEARN	657	0	22	12.71	5.787
Valid N (listwise)	657				

Table A.3. Pearson Zero-Order Correlations of Predictors of State-Level Error Rates, All Years

Correlations

		BLTOT	FAC_CSLD	FAC_WELF	LDOR	LDR_EXRT	LPCTBLK	LPNINDX	LWBRTST	PCBL	PPINDX	PPINDX2	PSST	WBRTST	YEARN
BLTOT	Pearson Correlation	1	.591**	.331**	.358**	.245**	.207**	.171**	.184**	.586**	-.053	.127**	.454**	.143**	.257**
	Sig. (2-tailed)	.	.000	.000	.000	.000	.000	.000	.000	.000	.177	.001	.000	.000	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
FAC_CSLD	Pearson Correlation	.591**	1	.322**	.235**	.000	.385**	-.009	.226**	.570**	-.223**	-.068	.712**	.090*	-.010
	Sig. (2-tailed)	.000	.	.000	.000	.994	.000	.809	.000	.000	.000	.083	.000	.021	.799
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
FAC_WELF	Pearson Correlation	.331**	.322**	1	-.124**	.270**	.154**	.106**	.143**	-.027	-.249**	-.190**	.503**	-.025	.311**
	Sig. (2-tailed)	.000	.000	.	.001	.000	.000	.006	.000	.483	.000	.000	.000	.530	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LDOR	Pearson Correlation	.358**	.235**	-.124**	1	-.049	.319**	.162**	.163**	.410**	.004	.153**	.221**	.093*	-.084*
	Sig. (2-tailed)	.000	.000	.001	.	.208	.000	.000	.000	.000	.912	.000	.000	.017	.031
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LDR_EXRT	Pearson Correlation	.245**	.000	.270**	-.049	1	.013	.536**	.028	-.035	-.071	-.088*	.122**	.132**	.866**
	Sig. (2-tailed)	.000	.994	.000	.208	.	.735	.000	.472	.366	.068	.025	.002	.001	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LPCTBLK	Pearson Correlation	.207**	.385**	.154**	.319**	.013	1	.493**	.579**	.260**	-.452**	-.305**	.669**	.350**	.024
	Sig. (2-tailed)	.000	.000	.000	.000	.735	.	.000	.000	.000	.000	.000	.000	.000	.539
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LPNINDX	Pearson Correlation	.171**	-.009	.106**	.162**	.536**	.493**	1	.208**	-.034	-.279**	-.155**	.199**	.245**	.649**
	Sig. (2-tailed)	.000	.809	.006	.000	.000	.000	.	.000	.387	.000	.000	.000	.000	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LWBRTST	Pearson Correlation	.184**	.226**	.143**	.163**	.028	.579**	.208**	1	.178**	-.191**	-.100*	.446**	.690**	.040
	Sig. (2-tailed)	.000	.000	.000	.000	.472	.000	.000	.	.000	.000	.010	.000	.000	.305
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PCBL	Pearson Correlation	.586**	.570**	-.027	.410**	-.035	.260**	-.034	.178**	1	-.033	.156**	.384**	.133**	-.135**
	Sig. (2-tailed)	.000	.000	.483	.000	.366	.000	.387	.000	.	.402	.000	.000	.001	.001
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PPINDX	Pearson Correlation	-.053	-.223**	-.249**	.004	-.071	-.452**	-.279**	-.191**	-.033	1	.831**	-.373**	-.093*	-.035
	Sig. (2-tailed)	.177	.000	.000	.912	.068	.000	.000	.000	.402	.	.000	.000	.017	.375
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PPINDX2	Pearson Correlation	.127**	-.068	-.190**	.153**	-.088*	-.305**	-.155**	-.100*	.156**	.831**	1	-.261**	-.025	-.043
	Sig. (2-tailed)	.001	.083	.000	.000	.025	.000	.000	.010	.000	.000	.	.000	.520	.275
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PSST	Pearson Correlation	.454**	.712**	.503**	.221**	.122**	.669**	.199**	.446**	.384**	-.373**	-.261**	1	.148**	.114**
	Sig. (2-tailed)	.000	.000	.000	.000	.002	.000	.000	.000	.000	.000	.000	.	.000	.003
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
WBRTST	Pearson Correlation	.143**	.090*	-.025	.093*	.132**	.350**	.245**	.690**	.133**	-.093*	-.025	.148**	1	.090*
	Sig. (2-tailed)	.000	.021	.530	.017	.001	.000	.000	.000	.001	.017	.520	.000	.	.021
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
YEARN	Pearson Correlation	.257**	-.010	.311**	-.084*	.866**	.024	.649**	.040	-.135**	-.035	-.043	.114**	.090*	1
	Sig. (2-tailed)	.000	.799	.000	.031	.000	.539	.000	.305	.001	.375	.275	.003	.021	.
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657

**. Correlation is significant at the 0.01 level (2-tailed).

*. Correlation is significant at the 0.05 level (2-tailed).

Table A.4. Pearson Zero-Order Correlations of Predictors of State-Level Error Rates, Years with Death Verdicts Only

		BLTOT	FAC_CSLD	FAC_WELF	LDOR	LDR_EXRT	LPCTBLK	LPNINDX	LWBRTST	PCBL	PPINDX	PPINDX2	PSST	WBRTST	YEARN
BLTOT	Pearson Correlation	1	.591**	.331**	.358**	.245**	.207**	.171**	.184**	.586**	-.053	.127**	.454**	.143**	.257**
	Sig. (2-tailed)	.	.000	.000	.000	.000	.000	.000	.000	.000	.177	.001	.000	.000	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
FAC_CSLD	Pearson Correlation	.591**	1	.322**	.235**	.000	.385**	-.009	.226**	.570**	-.223**	-.068	.712**	.090*	-.010
	Sig. (2-tailed)	.000	.	.000	.000	.994	.000	.809	.000	.000	.000	.083	.000	.021	.799
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
FAC_WELF	Pearson Correlation	.331**	.322**	1	-.124**	.270**	.154**	.106**	.143**	-.027	-.249**	-.190**	.503**	-.025	.311**
	Sig. (2-tailed)	.000	.000	.	.001	.000	.000	.006	.000	.483	.000	.000	.000	.530	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LDOR	Pearson Correlation	.358**	.235**	-.124**	1	-.049	.319**	.162**	.163**	.410**	.004	.153**	.221**	.093*	-.084*
	Sig. (2-tailed)	.000	.000	.001	.	.208	.000	.000	.000	.000	.912	.000	.000	.017	.031
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LDR_EXRT	Pearson Correlation	.245**	.000	.270**	-.049	1	.013	.536**	.028	-.035	-.071	-.088*	.122**	.132**	.866**
	Sig. (2-tailed)	.000	.994	.000	.208	.	.735	.000	.472	.366	.068	.025	.002	.001	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LPCTBLK	Pearson Correlation	.207**	.385**	.154**	.319**	.013	1	.493**	.579**	.260**	-.452**	-.305**	.669**	.350**	.024
	Sig. (2-tailed)	.000	.000	.000	.000	.735	.	.000	.000	.000	.000	.000	.000	.000	.539
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LPNINDX	Pearson Correlation	.171**	-.009	.106**	.162**	.536**	.493**	1	.208**	-.034	-.279**	-.155**	.199**	.245**	.649**
	Sig. (2-tailed)	.000	.809	.006	.000	.000	.000	.	.000	.387	.000	.000	.000	.000	.000
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
LWBRTST	Pearson Correlation	.184**	.226**	.143**	.163**	.028	.579**	.208**	1	.178**	-.191**	-.100*	.446**	.690**	.040
	Sig. (2-tailed)	.000	.000	.000	.000	.472	.000	.000	.	.000	.000	.010	.000	.000	.305
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PCBL	Pearson Correlation	.586**	.570**	-.027	.410**	-.035	.260**	-.034	.178**	1	-.033	.156**	.384**	.133**	-.135**
	Sig. (2-tailed)	.000	.000	.483	.000	.366	.000	.387	.000	.	.402	.000	.000	.001	.001
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PPINDX	Pearson Correlation	-.053	-.223**	-.249**	.004	-.071	-.452**	-.279**	-.191**	-.033	1	.831**	-.373**	-.093*	-.035
	Sig. (2-tailed)	.177	.000	.000	.912	.068	.000	.000	.000	.402	.	.000	.000	.017	.375
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PPINDX2	Pearson Correlation	.127**	-.068	-.190**	.153**	-.088*	-.305**	-.155**	-.100*	.156**	.831**	1	-.261**	-.025	-.043
	Sig. (2-tailed)	.001	.083	.000	.000	.025	.000	.000	.010	.000	.000	.	.000	.520	.275
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
PSST	Pearson Correlation	.454**	.712**	.503**	.221**	.122**	.669**	.199**	.446**	.384**	-.373**	-.261**	1	.148**	.114**
	Sig. (2-tailed)	.000	.000	.000	.000	.002	.000	.000	.000	.000	.000	.000	.	.000	.003
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
WBRTST	Pearson Correlation	.143**	.090*	-.025	.093*	.132**	.350**	.245**	.690**	.133**	-.093*	-.025	.148**	1	.090*
	Sig. (2-tailed)	.000	.021	.530	.017	.001	.000	.000	.000	.001	.017	.520	.000	.	.021
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657
YEARN	Pearson Correlation	.257**	-.010	.311**	-.084*	.866**	.024	.649**	.040	-.135**	-.035	-.043	.114**	.090*	1
	Sig. (2-tailed)	.000	.799	.000	.031	.000	.539	.000	.305	.001	.375	.275	.003	.021	.
	N	657	657	657	657	657	657	657	657	657	657	657	657	657	657

**. Correlation is significant at the 0.01 level (2-tailed).

*. Correlation is significant at the 0.05 level (2-tailed).

APPENDIX B.

Table B1. Descriptive Statistics for Individual-Level Variables in Habeas Analysis

Variable	N	Mean	Std Dev	Minimum	Maximum
SEH2	596	0.25	0.43	0	1
DLOS_FFD	594	0.30	0.46	0	1
AGG_MIT	596	1.87	1.34	-5	6
OFVCINDX	596	2.01	1.23	0	7
FEH2	596	0.19	0.39	0	1
SENTYR	596	1,980.02	3.37	1973	1990
CLAIMNO	563	4.50	3.57	1	29
REPMAJ	543	0.58	0.49	0	1

Table B2. Correlation Matrix for Individual-Level Variables in Habeas Analysis

Correlations

		SEH2	DLOS_FFD	AGG_MIT	OFVCINDX	FEH2	SENTYR	CLAIMNO	REPMAJ
SEH2	Pearson Correlation	1	.023	.043	.181	-.022	.096	.114	.043
	Sig. (2-tailed)	.	.584	.295	.000	.598	.019	.007	.320
	N	596	594	596	596	596	595	563	543
DLOS_FFD	Pearson Correlation	.023	1	-.041	.098	.051	-.053	-.039	-.067
	Sig. (2-tailed)	.584	.	.317	.017	.214	.201	.353	.118
	N	594	594	594	594	594	593	561	542
AGG_MIT	Pearson Correlation	.043	-.041	1	.030	-.004	-.016	.065	.018
	Sig. (2-tailed)	.295	.317	.	.462	.928	.700	.124	.681
	N	596	594	596	596	596	595	563	543
OFVCINDX	Pearson Correlation	.181	.098	.030	1	.063	.171	.125	.075
	Sig. (2-tailed)	.000	.017	.462	.	.124	.000	.003	.082
	N	596	594	596	596	596	595	563	543
FEH2	Pearson Correlation	-.022	.051	-.004	.063	1	-.049	.001	.065
	Sig. (2-tailed)	.598	.214	.928	.124	.	.229	.989	.131
	N	596	594	596	596	596	595	563	543
SENTYR	Pearson Correlation	.096	-.053	-.016	171	-.049	1	.183	.181
	Sig. (2-tailed)	.019	.201	.700	000	.229	.	.000	.000
	N	595	593	595	595	595	596	562	542
CLAIMNO	Pearson Correlation	.114	-.039	.065	.125	.001	.183	1	-.003
	Sig. (2-tailed)	.007	.353	.124	.003	.989	.000	.	.938
	N	563	561	563	563	563	562	563	538
REPMAJ	Pearson Correlation	.043	-.067	.018	.075	.065	.181	-.003	1
	Sig. (2-tailed)	.320	.118	.681	.082	.131	.000	.938	.
	N	543	542	543	543	543	542	538	543