The impact of the death penalty on murder

John J. Donohue, Stanford Law School
Deterrence and executions

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John J. Donohue III, Senior Editor
Yale Law School

Both history and daily crime sheets underscore a depressing capacity for human violence and inhumanity. Some scholars feel that eliminating capital punishment would be a step toward reducing the toll of human suffering, whereas others feel that retaining the death penalty will prevent some murders at least. Kovandzic, Vieraitis, and Boots (2009, this issue) provide a comprehensive ordinary least-squares (OLS) state panel data assessment of the most recent postmoratorium data available and reach a strong conclusion that the death penalty does not deter murder. This article is an important piece in the complex jigsaw puzzle that will illuminate which factors can deter which crimes under which circumstances.

Commenting on the Kovandzic et al. (2009) article are two scholars who have authored major articles that concern the impact of the death penalty on murder. Richard Berk (2009, this issue) speculates whether the deterrent impact of the death penalty is knowable given current data and methods, whereas Paul Rubin (2009, this issue) argues that “the weight of the evidence as well as the theoretical predictions both argue for deterrence, and econometrically flawed studies such as this article are insufficient to overthrow this presumption.” With virtually all positions represented by these three documents, I will discuss three recent new studies that I think address some of Berk’s concern and provide strong evidence to support that Kovandzic et al. are right.

The Chicago School and Three Major New Studies

Many Chicago School economists believe that universal answers for deterrence issues flow inexorably from simple price theory. A growing body of evidence contradicts this view, which is often associated with Gary Becker (perhaps unfairly). Predictions about deterrence need to be far more pointed and nuanced than is possible with mere theoretical musings about the slope of demand curves. Undoubtedly, a well-functioning criminal justice system deters massive amounts of crime. If you took the police away—as we have seen when they go on strike or are otherwise
drastically disrupted—then crime can soar.\(^1\) This pattern shows that the Chicago School views capture an important element of truth. But although we see deterrence in one domain, it does not suggest that we will see similar levels of deterrence everywhere.

For example, Lee and McCrary (2009) show that past juvenile offenders are not particularly responsive to the prospect of the dramatically higher sanctions they will face when they reach the age of majority. Specifically, they suggest that a 230% increase in expected sentence length is associated with only a 1.8% reduction in the probability of arrest.\(^2\) In other words, the class of individuals who have been arrested at least once by age 17 shows stunningly little sensitivity to even enormous increases in the probability that they will go to jail and will face a longer sentence if they do. This finding does not refute Becker, but it does suggest that Beckerian deterrence predictions may not apply to at least one class of offenders as they pass to the age of majority.

A second stunning article by Francesco Drago, Roberto Galbiatti, and Pietro Vertova (2009) continues to deepen our understanding of criminal deterrence in two ways. Italy released a huge portion of its prison population on August 1, 2006, but the early release involved the prospect of a penalty kicker; if the early releasees were arrested again, then they would have to repay the time avoided on top of any new sanction. At least in the first 7 months, the threat of the enhanced sentence had a substantial deterrent effect on the average Italian prisoner. Thus, the initial lesson from the Italian experiment vindicates a Chicago School deterrence prediction. But again, deterrent effects are not uniform but are sensitive to context. The Italian prison case revealed that telling a prisoner who has just been let out 3 years early that he or she will have to serve those remaining 3 years tacked on to any additional sentence if caught has a substantial deterrent effect on rearrest for the first 7 months, which becomes somewhat less potent after 1 year (that is all the data we have at present).\(^3\) Will substantial deterrence persist after more than 1 year? We await the results of the follow-up study to answer that question. It also is worth pondering whether a similar deterrent effect would have occurred if a legislative enactment simply had been implemented that all crimes would now carry an extra 3-year sentence. Having a personalized threat may well be more potent than a more uniform and distant threat of punishment.

But a second key finding that emerges from the Italian study conflicts with a wooden Chicago School view of deterrence. The most serious prior criminals—defined as those individuals with an original sentence longer than 69 months—were not deterred by the prospect of the enhanced sentence. This point brings us to the death penalty. What Rubin (2009) and many Chicago Schoolers think emerges from theory—an assured deterrent effect—is undermined again by the

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1. In October 1969, a police strike in Montreal sparked a brief wave of crime and violence. During the strike, banks were robbed, more than 100 shops were looted, and at least 12 fires occurred (Time Magazine, 1969).
2. Although this point estimate is not significantly different from zero, the large standard error means that the largest level of deterrence that emanates from a confidence interval generated by a one-tailed test could be 9.5%.
3. The elasticity of crime with respect to sentence length was estimated at –0.74 for the first 7 months but only –0.45 at 12 months.
best empiricism. The fact that the most serious criminals were not deterred by the risk of added jail time in the Italian experiment, and the less serious criminals were deterred, shows that as one marches down the path toward more serious criminal propensities (or toward adolescence, as Lee and McCrary [2009] show), the evidence of deterrence weakens or vanishes.

A final noteworthy study by Zimring, Fagan, and Johnson (in press) that compares the near-identical murder paths of Singapore and Hong Kong despite the latter’s renunciation of capital punishment and the former’s wildly enthusiastic embrace of it provides yet another indication that Kovandzic et al. (2009) have it right when they conclude that the death penalty has no net deterrent effect in the postmoratorium United States (Zimring et al., 2009).

The Zimring et al. (2009) piece is so powerful because Singapore made Texas look like a piker when it came to using the death penalty; yet the matched-comparison state of Hong Kong experienced the same time path of murder while jettisoning capital punishment. Perhaps someone will show that Singapore’s massive jump in executions and subsequent massive decline were tightly calibrated to keep the murder rate moving along a similar path to Hong Kong, or that Hong Kong had some unusually good crime experience when things were going south in Singapore, but the likely answer is what we see from the Italian experience—the most serious criminals are not particularly responsive to threats of higher severity, whether they be a longer sentence or capital punishment.

Berk on Kovandzic, Vieraitis, and Boots
I completely agree with Richard Berk’s (2009) assessment that “no credible evidence exists that the death penalty, as implemented in the United States since 1979, has any deterrent value.” At present, however, Berk claims “no credible evidence exists to rule out any deterrent effects.” He worries that OLS panel data techniques are not powerful enough to answer this enduring issue definitively.

I agree that one panel data study alone can never finally resolve such a contentious question—in part, because the one study itself would need to be thoroughly vetted through continued research. I am more optimistic than Berk (2009), however, that the aggregation of several different types of extremely high-quality studies, which all point in the same direction, can generate more convincing answers to important empirical questions than he seems to think possible. For example, the three studies discussed do not share the same potential weaknesses of OLS approaches. From such combined evidence, a pattern is beginning to emerge that the most serious criminals (and juvenile-offending adolescents) simply are not susceptible to distant threats of heightened punishment, perhaps because they fear they will not be caught, they never think about the consequences of their impulsive acts, or they just do not care. I will be interested to hear if Berk is moved by the Zimring et al. (2009) article to conclude that massive increases in the use of capital punishment followed by an elimination of the death penalty had no discernible effect on murder in Singapore. I would take the Singapore experience as credible...
evidence effectively ruling out a deterrent effect of even heavy reliance on executions—at least in Singapore when carried out in the (rather odd) Singaporean way.

Berk (2009) underscores all concerns of trying to tease out causal relationships from observational data, but I think that Kovandzic et al. (2009) do a magnificent job of showing that well-done OLS state panel data analyses on the most current U.S. data simply will not produce evidence of deterrence. This conclusion is an important contribution.

Rubin on Kovandzic, Vieraitis, and Boots
Rubin (2009) also provides some important notes of caution. He observes that the survey evidence amassed by criminologists cannot show that no criminals will be deterred simply because most will not be. He also makes the valid point that perhaps instrumental variable approaches will generate evidence of deterrence when OLS fails. Indeed, the only deterrent effect evidence of capital punishment that survives the Kovandzic et al.’s (2009) march through the land of OLS estimation are some 2SLS (i.e., two-stage least squares) papers by a handful of authors (which include Rubin and his coauthors).

Although Rubin (2009) suggests that most studies have supported the death penalty, this conclusion is unconvincing because Kovandzic et al. (2009) have laid all of the OLS studies that find deterrence to waste. Kovandzic et al.’s article is now state of the art on OLS estimation of the impact of capital punishment, and its predecessor studies on shorter time frames and with more idiosyncratic specifications simply are off the table now. Those earlier studies no longer can be counted. Progress occurs in science.

Rubin (2009) is correct to note, however, that a hint of life in the deterrence hypothesis can be found in one of the seven Kovandzic et al. (2009) estimates when state-specific time trends are dropped (this evidence is in the model that explains state-year murder rates with a raw number of executions). Against the sea of insignificant results, some scholars may dismiss this one lonely sign of deterrence as spurious, but it is worth thinking about the desirability of including state trends and the “number of executions” specification. Certainly, Kovandzic et al. correct the notion that standard specification tests would call for including the state trends, but such tests are not uniformly dispositive of these vexing specification choices.

However, reasons exist to be skeptical of the number of executions model (recall that even this model only shows signs of deterrence if the state trends are dropped). First, the number of executions model assumes that one would expect a bigger deterrent effect in a huge state that has 2,000 murders and executes 20 murderers (1%) than in a small state that has 10 murders and executes all 10. Certainly on a percentage basis, I would think any deterrence would be far more likely to show up in the latter state, where the message is “all murderers are executed,” than in the former (where the message is “it is highly unlikely that you will be executed if you murder someone”).

But a second factor provides even greater reason to doubt the isolated “number of executions” model result (when state trends are dropped). Kovandzic et al. (2009) show that this
“number of executions” model also leads to the ostensible finding that more executions lead to a lower burglary rate. Because the death penalty for capital murders clearly is not influencing burglaries, this “number of executions” model likely is picking up another effect associated more generally with lower crime.

Rubin (2009) makes other criticisms of Kovandzic et al. (2009) that I find less persuasive. He suggests that including some variables found in Dezhbakhsh, Rubin, and Shepherd (2003) might overturn the Kovandzic et al. findings. I think Kovandzic et al. are correct to steer clear of the arrest and death sentence ratios (as well as the controls for aggravated assault and robbery) that Dezhbakhsh et al. employ, as I have written elsewhere (Donohue and Wolters, 2009). Rubin’s concern that including a measure of racial composition might change Kovandzic et al’s results turns out to be unfounded, because adding a control for percent black generates no evidence of deterrence.

Rubin (2009) also questions Kovandzic et al.’s (2009) corrections for serial correlation, but again, I think they follow current best practice in doing so. Rubin’s other primary specification critique is that 2SLS approaches are needed, whereas Kovandzic et al. only provide OLS evidence. Kovandzic et al. show that efforts to control for endogeneity in the death penalty arena (they cite Zimmerman, 2006) at times generate clearly biased results. Indeed, Kovandzic et al. as well as Donohue and Wolters make the case that it is likely that any postmoratorium endogeneity effects would be biased toward finding deterrence. If this view is correct, then it suggests that 2SLS approaches should generate even less evidence of deterrence than OLS approaches. Add to that the highly unconvincing instruments that have been offered thus far in the 2SLS studies, and I think Kovandzic et al. were wise to limit their analysis to OLS.

I disagree with Rubin (2009) that “Many selections and remedies [in the Kovandzic et al’s (2009) article] are ad hoc and at odds with sound econometric practice.” Kovandzic et al. try several different specifications, which may not be preferred, but because some scholars have advocated their use and the results consistently support the no-deterrence finding, it seems reasonable to present the results to clear away any future contention.

Rubin (2009) then states “it would be incredible and a violation of the law of demand if the chance of execution did not deter at least some murders.” The point is not well taken, as illustrated in the discussion of the most severe Italian criminals who were not deterred by increased possible sanctions. Even massively increased executions generated no apparent drop in murder in Singapore. But, of course, even if some criminals were deterred by the prospect of the death penalty, some murders might be induced by it, which leads to no net effect. Indeed, Rubin’s statement ignores the fundamental asymmetry of potentially benign and malign effects that flow from the death penalty. Specifically, the death penalty only can have a possible useful effect on a small number of individuals—essentially, those individuals who commit murder when they face only life without the possibility of parole—because everyone else already is deterred by lesser sanctions.
For example, in New York—a state with no capital punishment (as of 2004), a large population (19,300,000), and a relatively low murder rate (4.77 per 100,000 people)—we find that 921 murders occurred in 2006. Assuming that 921 roughly equals the number of murderers in New York in 2006, then this figure represents the maximum number of individuals whose behavior could have been changed in a socially acceptable manner by the presence of a death penalty law (at least under a rational actor model). But against these 921 murderers who potentially might have been deterred by capital punishment, approximately 19,299,000 individuals in New York were not deterred by the threat of capital punishment (because it was nonexistent and yet they still did not kill). This number is roughly 20,000 times as great as the number of murderers in New York in 2006. If the death penalty has a brutalization or other crime-inducing effect, then we would be concerned that its introduction might have an adverse effect on the 19,299,000 current nonmurderers. If any malign per capita effect were only 1/20,000 as strong as the per capita deterrent effect (potentially influencing only 921 individuals), then the malign effect would offset entirely any deterrent benefit, because it would operate on 20,000 times as many New York citizens. Even if Rubin (2009) was right that some deterrence occurs in the small set of individuals who would murder when confronted by the monumentally severe sentence of life without parole, a tiny per capita crime-inducing effect operating on hundreds of millions could offset or even overwhelm it.4

Rubin (2009) concludes his essay with the admonition that “an element of elitism may be present in academic recommendations for abolishing the death penalty, because others will bear the costs.” But given the increasingly more powerful evidence that the death penalty neither deters crime in the United States (or Singapore) nor is cost efficient, one must ask, what cost is Rubin thinking of? As I sit in Connecticut where two separate DNA exonerations of convicted murderers finally have released two men after decades in prison, and read the case of what seems to have been an innocent father executed for killing his children (who died in a Texas fire), the case for the death penalty becomes ever more problematic (Labossiere, 2009; Grann, 2009; Herbert, 2009).5

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4. Following a bright line rule that we do not kill people except in the most exigent of circumstances may well restrain harsh and brutal behavior in many different contexts.

5. Cameron Todd Willingham was executed in Texas in 2004 for committing arson that killed his children. According to a recent report, however, no scientific basis determined that the fire was arson. Connecticut resident Kenneth Ireland was sentenced in 1989 to 50 years in prison for the rape and murder of Barbara Pelkey. He was released in August 2009, however, after DNA testing showed he could not have committed the crimes for which he was convicted. After 20 years in prison, Miguel Roman (again from Connecticut) also was released in December 2008 after DNA evidence pointed to the true killer of the woman he was convicted of murdering. In both the Willingham and Roman cases, jail-house snitches were employed to garner convictions when ample reasons were present to be dubious about the prosecutions.
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John J. Donohue III is the Leighton Homer Surbeck Professor of Law. He is an economist
and lawyer who has used large-scale statistical studies to estimate the impact of law and public
policy on a wide range of areas from civil rights and employment discrimination law to school
funding and crime control. Before joining Yale Law School, he was a chaired professor at
both Northwestern Law School and Stanford Law School. He recently published Employment
discrimination: Law and theory. Among his major articles are: *Uses and abuses of empirical
evidence in the death penalty debate* (with Justin Wolters), *Shooting down the ‘more guns, less crime’
hypothesis* (with Ian Ayres), and *The impact of legalized abortion on crime* (with Steven Levitt).
Donohue is a graduate of Hamilton College, and he received his J.D. from Harvard and a
Ph.D. in economics from Yale.