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Reviewed work(s):

Source: *Crime and Justice*, Vol. 37, No. 1 (2008), pp. 279-311

Published by: [The University of Chicago Press](#)

Stable URL: <http://www.jstor.org/stable/10.1086/524825>

Accessed: 03/01/2013 22:58

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# Learning from the Limitations of Deterrence Research

## ABSTRACT

Public policy and scientific knowledge concerning deterrence have long been marching in different directions. Despite the proliferation of three-strikes, mandatory minimum, and concealed weapons laws and retention of capital punishment in 37 states, there is little credible evidence that changes in sanctions affect crime rates, and there is no credible evidence that capital punishment deters better than life sentences or that allowing citizens to carry concealed weapons deters at all. There is evidence that changes in enforcement and sanctions can affect some kinds of behavior—for example, tax compliance, speeding, illegal parking—and there are plausible grounds for believing that other deterrable behaviors can be identified. Doing so will require fine-grained studies that take account of offender characteristics and perceptions, offending situations, and whether and how new enforcement strategies and sanctions systems are implemented.

The state of the art of policy-relevant knowledge about the deterrent effects of the criminal justice system is little different in 2008 than it was 30 years ago when the National Academy of Sciences (NAS) panel on deterrence and incapacitation reported that the existence of a criminal justice system has overall deterrent effects, there is a widely shared intuition that penalty increases have marginal deterrent effects but the available evidence is highly ambiguous and contested, and there is no credible evidence that capital punishment deters homicide any more

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0192-3234/2008/0037-0007\$10.00

effectively than penalties that would otherwise be imposed (Blumstein, Cohen, and Nagin 1978). There is some evidence, as Beccaria and Bentham believed, that certainty and promptness of punishment are more important than severity. Because there are differences in order of magnitude in the abilities of police and courts to alter the promptness of their behavior or affect would-be offenders' perceptions of risk, changes in police practices are more likely to achieve deterrent effects than are changes in sentencing policies and practices. The capacity of the police, however, to achieve long-term crime-reductive effects—as opposed to short-term effects from well-publicized crackdowns (Sherman 1990)—is limited by practical constraints on availability of police resources. Finally, there is considerable evidence that police, prosecutors, judges, and juries often alter their behavior to offset the effects of punishment policy changes with which they disagree, thereby undermining the likelihood of achieving marginal deterrent effects.

There is little point in continuing to investigate these subjects in the same old ways for another 30 years. There are, however, some other lessons about future research agendas to be derived from current knowledge. First, macro-level modeling of deterrent effects of changes in sanctions policies by economists and econometricians has reached a dead end, as Ronald Coase in 1978 predicted would happen concerning subjects on which the economist's advantage was primarily one of technique.<sup>1</sup> Results are inevitably fragile and highly sensitive to minor specification changes. Such research is incapable of taking into account whether and to what extent purported policy changes are implemented, whether and to what extent their adoption or implementation is perceived by would-be offenders, and whether and to what extent offenders are susceptible to influence by perceived changes in legal threats. At the very least, macro-level research on deterrent effects should test the null hypothesis of no effect rather than the price theory assumption that offenders' behavior will change in response to changes in legal threats.

Second, useful research on deterrence will have to become much more nuanced than it has mostly been so far. Whether changes in legal threats are implemented in practice and whether would-be offenders

<sup>1</sup> "Once some of these practitioners [social scientists other than economists] have acquired the simple, but valuable, truths which economics has to offer, and this is the natural competitive response, economists who try to work in the other social sciences will have lost their main advantage and will face competitors who know more about the subject matter than they do" (Coase 1978, p. 210).

perceive those changes are important questions, of course, but so are differences in individuals' susceptibility to changes in legal threats. To many, probably most, people, perceived changes will have no effects on behavior either because their personal values and circumstances remove them altogether from the class of would-be offenders or, conversely, because their values and circumstances make them more or less immune from changes in legal threats. Wikström (2007, ms. 28), for example, argues that "inclusion of subjects who would never be motivated to or consider committing the crime/crimes in question" is a major shortcoming of many micro-level deterrence studies.<sup>2</sup>

Third, however, studies of the implementation of penalty changes may, turned around, provide important insights into prevailing norms concerning the seriousness of crimes and acceptable severities of punishment. It is at least as important to study why penalty changes have little or no effect on crime rates as to study whether they have effects. Penalty laws that are seldom applied, such as most habitual offender and three-strikes laws, or that are routinely circumvented by officials, such as many mandatory minimum sentence laws, are patently out of step with prevailing norms; otherwise they would be applied or enforced. Their non- or partial application or enforcement is likely to be among the reasons why such laws have little discernible effect. The correct conclusion to be drawn from studies that show that penalty laws are routinely circumvented is not that officials are misbehaving but that laws are out of step with prevailing norms and should be altered or repealed.

Although readers of this essay are unlikely to be laypeople, it may be useful to say a couple of things that would be said in a talk to laypeople on this subject. The first is that it is natural to suppose that changes in disincentives to crime will change peoples' behavior. Often they do. Human beings are influenced by incentives and disincentives, and offenders are human beings. For relatively minor forms of prohibited behavior such as illegal parking, fast driving, or littering, significant increases in the perceived likelihood of apprehension or severity of penalties influence behavior. When the threat of having an illegally parked car towed goes up, people become more careful parkers. When speeding drivers see marked police cars stopped by the side

<sup>2</sup> Others have made the same argument (e.g., Pogarsky 2002) and tried to take account of it in their research designs (e.g., Bachman, Ward, and Paternoster 1992; Piquero and Tibbetts 1996).

of a highway, they slow down. Similarly, for calculated instrumental crimes such as tax evasion (e.g., Klepper and Nagin 1989), increases in perceived risks of apprehension appear to deter prospective wrongdoers significantly (though much of this literature is based on laboratory research and responses to hypothetical what-if questions, which raises significant external validity issues).

The debates about whether penalties deter at all, or whether increases in penalties produce marginal increases in deterrence, are not mostly about those kinds of behavior. They are about the mass of ordinary property, “morals” (drugs, prostitution, gambling, etc.), and violent crimes. Typically, these crimes are not committed in public, like driving, parking, and littering offenses, and often they are not highly calculated. Most, however, are unambiguously wrongful, which means that many people will not commit them under any but exceptional circumstances. Many of these crimes are impulsive or are committed under the influence of drugs, alcohol, peer influences, powerful emotions, or situational pressures. Many are committed by people who are deeply socialized into deviant values and lifestyles. These characteristics of many would-be offenders do not mean that it is a priori impossible to affect would-be offenders’ criminal choices by means of legal threats. They do mean that doing so is far from being a matter merely of enacting harsher laws, imposing harsher penalties, or adopting more aggressive policing strategies.

Another thing to be said to a lay audience is that thinking and knowledge about deterrence are important. Policy makers would like to believe that penalties and penalty increases deter because those beliefs provide a basis for trying to do something about troubling social problems. The difficulty is that mistaken beliefs in deterrence may lead to adoption of seriously mistaken policies.

Three times in the last 30 years, American state and federal policy makers have adopted major policy changes on the basis of deterrence research findings that were subsequently repudiated.<sup>3</sup> These were the enactment of capital punishment statutes (and the U.S. Supreme Court’s upholding of them) in the context of Isaac Ehrlich’s claim that every execution saves the lives of eight would-have-been victims (Ehr-

<sup>3</sup> Findings on the alleged deterrent effects of capital punishment or “shall-issue” concealed weapons laws were in both cases invoked by proponents of such laws, but support for them was so ideological and the political impetus behind them so strong that it is not unlikely that most would have been enacted when they were irrespective of research results (see, e.g., Tonry and Green 2003).

lich 1975);<sup>4</sup> the enactment in a majority of American states of mandatory arrest policies in misdemeanor domestic violence cases on the basis of Sherman and Berk's finding that arrests deterred future violence against the original victims (Sherman and Berk 1984; later repudiated by Sherman himself [Sherman, Schmidt, and Rogan 1992]); and the enactment in many states in the last decade of "shall-issue" laws mandating issuance to most adults of licenses to carry concealed firearms in public on the basis of Lott's finding that carrying concealed weapons reduces violent crime rates because would-be assailants are deterred by the knowledge that a potential victim may be carrying a gun (Lott 1998).<sup>5</sup> All three findings have since been shown to be incorrect or not generalizable, but most of the capital punishment, mandatory arrest, and concealed weapons laws whose enactment they influenced (or seemed to justify) remain in effect. The implication is that policy makers should set very high evidentiary standards when considering evidence about the deterrent effectiveness of penalties before adopting policies predicated on deterrence rationales.

Although in the last 30 years there has not been a huge amount of research on deterrence, several small literatures have accumulated. First, since the mid-1990s, a handful of American economists have attempted to model the deterrent effects of capital punishment on homicide and concluded that each execution saves, for example, 18 lives (Dezhbakhsh, Rubin, and Shepherd 2003). The theory is that would-be offenders' knowledge that capital punishment is authorized or that some murderers have been executed will make them less likely to kill people. This work has been discredited by other economists (e.g., Donohue and Wolfers 2005) and by noneconomists (e.g., Fagan, Zimring, and Geller 2006). The only credible conclusions that can be drawn are either that capital punishment has no deterrent effects on homicide or that there is no credible evidence that it does. This conclusion accords with a recent review of this literature by Levitt and Miles (2007, pp. 474–76). Some analyses suggest that executions lead to increases in homicide, possibly through a brutalization effect (e.g., Katz, Levitt, and Shustorovich 2003).

Second, since the late 1980s, a small number of economists have

<sup>4</sup> Solicitor general Robert Bork cited Ehrlich's work in arguing before the U.S. Supreme Court in *Gregg v. Georgia*, 428 U.S. 153 (1976), a decision upholding the constitutionality of existing capital punishment laws in the United States.

<sup>5</sup> In 1994, 1995, and 1996, 13 American states adopted shall-issue laws (Lott 2000, p. 169, n. 7).

attempted to model the crime-preventive effects of letting most ordinary citizens (usually except children, some former felons, and the mentally ill) carry concealed weapons in public places (e.g., Lott and Mustard 1997; Lott 1998, 2000). The hypothesis is that would-be offenders will be deterred by the knowledge that prospective victims may be armed and able to use firearms to defend themselves. Although Lott and his colleagues have concluded that allowing citizens to carry concealed weapons has substantial crime prevention effects, his results have been discredited by other economists on both technical (e.g., Ayres and Donohue 2003*b*; Cook and Ludwig 2003) and ethical (Ayres and Donohue 2003*a*; Donohue 2004)<sup>6</sup> grounds and by noneconomists (e.g., Black and Nagin 1998). A recent U.S. NAS review of that research concludes that there is no credible evidence that enactment of shall-issue laws has measurable deterrent effects: “The committee concludes that with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates. It is also the committee’s view that additional analysis along the lines of the current literature is unlikely to yield results that will persuasively demonstrate a causal link between right-to-carry laws and crime rates” (Wellford, Pepper, and Petrie 2005, pp. 150–51).

Third, since the 1980s, a small number of researchers have attempted to demonstrate that private possession of firearms reduces criminal victimization and more than offsets harmful consequences of private ownership of firearms including accidental injuries and deaths and suicides (e.g., Kleck and Kates 2001). The hypotheses are that would-be offenders are deterred by knowledge that prospective victims have guns and that offenders desist from initiated crimes when they learn victims are armed. The most authoritative reviews of this literature conclude that the claims are unconvincing (e.g., Cook 1991; Ludwig 2000). The recent NAS panel was agnostic: “Ultimately, researchers may conclude that it is impossible to effectively measure many aspects of defensive gun use” (Wellford, Pepper, and Petrie 2005, p. 108; also see pp. 110–11, 117).

Fourth, from the mid-1970s through the late 1980s, evaluators investigated the effects of new laws expressly aimed at reducing crime rates through the deterrent effects of increased penalties. Mostly these

<sup>6</sup> Research ethics, that is. Serious questions have been raised about repeated systematic errors in Lott’s data and about other matters. Details and references to a supporting literature can be found in Ayres and Donohue (2003*a*) and Donohue (2004).

laws prescribed mandatory minimum sentences for offenders convicted of particular crimes. The hypothesis is that knowing that use of a gun in a robbery will result in a minimum 5-year sentence, or an extra 2 years on top of the otherwise appropriate sentence, for two examples, makes would-be offenders less likely to carry or use a gun. The evaluations concluded either that there were no measurable deterrent effects or that there were short-term effects that quickly wasted away (Tonry 1996, chap. 5). There were two primary reasons: there was little evidence that offenders were aware of the harsher potential penalties; more important, most evaluations showed that the increased penalties were seldom imposed. In some cases, the prescribed “harsher” punishments were less severe than offenders would otherwise have received. In others, judges and lawyers altered their charging, plea negotiation, and fact-finding practices to nullify the law’s effects. There has been little recent research on implementation of harsh mandatory punishments, but the two latest major studies produced the same pattern of findings as earlier studies (McCoy and McManimon 2004; Merritt, Fain, and Turner 2006).

Fifth, during the 1990s, American policy makers implemented a number of tough-on-crime initiatives, most notably zero-tolerance policing in New York City and a broadly framed “three-strikes-and-you’re-out” law in California, and claimed dramatic crime reductions through deterrence. Rudolph Giuliani, the New York mayor; William Bratton, his police chief at the time; and their representatives claimed credit for the happy correlation, as did governor Pete Wilson of California. However, no sophisticated study has shown that the policy changes substantially affected the crime rate changes. American crime rates peaked in 1990–91 in the United States, three to four years before zero-tolerance policing was put into place and California’s three-strikes law was enacted, both in 2004. Preexisting downward trends in homicide and robbery rates continued after the initiatives were announced, but neither the timing of the downturns nor the pitches of the slopes were significantly different in California and New York than in other populous states or large cities, respectively. The most exhaustive examinations of the evidence conclude that there is no credible basis for believing that the policy changes substantially influenced declines in crime rates (Harcourt 2001 [New York City]; Zimring, Hawkins, and Kamin 2001 [California]; Tonry 2004, chap. 5 [both]; Har-



court and Ludwig 2006 [New York City]; Taylor 2006 [New York City]).

Sixth, since the early 1980s researchers have been investigating threat communications and perceptions. A sizable literature demonstrates that ordinary citizens are largely uninformed about the operation of the justice system, the content of the criminal law, and the severity of punishments (Roberts et al. 2002). For a hypothesis that a change in practice or policy will affect behavior to be plausible, there must be some basis for believing that the people whose behavior is being targeted will know about the change.<sup>7</sup> A small but growing literature examines whether and how experience with the criminal justice system, or peers' experiences of the justice system, affect perceptions of risk and how those perceptions affect behavior (e.g., Pogarsky, Piquero, and Paternoster 2004; Matsueda, Kreager, and Huizinga 2006; Lochner 2007). A largely psychological literature has investigated what people know and provided additional knowledge and then tried to determine how that knowledge might affect behavior. This literature is afflicted by two common problems of laboratory research: its subjects are usually college students (rather than offenders), and its outcome measure is how subjects say their behavior would be affected rather than how their behavior changes. A related subliterate asks offenders whether in retrospect they would have behaved differently had they known what the penalties would be or if they had known that the penalties would be even greater. Both the main and the narrower literature report that people say they would have altered their behavior (the major works are cited in Pratt et al. [2006]).

Those literatures are not all examined in detail here. There is little point in reprising discussion of the literatures on deterrent effects of mandatory minimum sentence laws, major broad-based policy changes (e.g., zero-tolerance policing, three strikes), possession of weapons for self-defense, or threat perception. These have been exhaustively reviewed before, and no important original research findings have recently been published. Section III briefly discusses the less often reviewed research on capital punishment and concealed weapons. Before that, perhaps surprisingly, Section I discusses Robert Ellickson's *Order without Law* (1991), an examination of the influence of civil law,

<sup>7</sup> And, separately, that they will be influenced by that knowledge. There are two other important issues: whether the change is implemented as announced and whether changes in practice are sustained.

and knowledge about it, on cattlemen's behavior. Ellickson is a law and economics specialist, and his work raises fundamental issues germane to thinking about deterrence and offers arguments that may explain why econometric research based on aggregate data is seldom instructive concerning deterrence. Section II is an overview of the major reviews and meta-analyses of deterrence research. Section IV sets out conclusions and a short agenda for research that might sharpen understanding of the deterrent effects of penalties.

Alex Piquero, who read an earlier draft of this essay, asked why another review of the literatures on deterrence is needed, given that relatively little, if anything, has been learned about deterrent effects since Daniel Nagin reviewed the literature for *Crime and Justice* in 1998. There are three answers. First, the economics literatures on concealed weapons and capital punishment largely postdate Nagin's essay. Second, those literatures make it clear that such studies of sanction effects—aggregate, macro-level studies relying on official crime data—are unlikely ever to yield credible results. Third, however, more promisingly, since 1998, micro-level studies have begun to emerge on how offenders' (and their acquaintances') criminal justice system experiences affect their perceptions of legal threats and their behavior.

It is important to stress, however, that I believe it is a Good Thing that work by economists on the criminal justice system has increased greatly in the past 10 years (see, e.g., Bushway and Reuter, in this volume), even if I'm exceedingly skeptical about the likely value of additional econometric studies of deterrence. Economists' distinctive disciplinary frame and analytical techniques are likely to yield important new insights into a wide range of subjects (e.g., the effects of imprisonment on labor force participation [Bushway, Stoll, and Weiman 2007] and the costs and consequences of imprisonment [Raphael and Stoll, forthcoming]).

### I. Order without Law

Ellickson's *Order without Law* (1991) examines compliance with civil laws affecting cattle farmers in rural Shasta County, California. This may not appear to be an obvious place to begin an examination of research on the deterrent effects of criminal penalties. It is a useful place to start, however, for two reasons. First, both Ellickson's discussions of how cattle farmers deal with recurring potential civil law prob-

lems and a five-level model he develops for understanding what kinds of knowledge and other considerations influence their behavior are apposite to thinking about how criminal laws and punishments influence behavior. Second, the book is in some ways a report on an economist's (more specifically, a law and economics specialist's) gradual realization that economic models sometimes grossly oversimplify how humans make choices. This may be part of the explanation for why some of the economists whose work is discussed in Section III (capital punishment and concealed weapons) so often get things so badly wrong.

*A. How Cattle Farmers Deal with Legal Problems*

Ellickson's discovery was that cattle farmers in Shasta County had little knowledge of the law, confidently believed things to be true about the law that were false, and resolved almost all their conflicts without consulting the law or lawyers. In addition, local officials, police, and insurance adjusters also often were unaware of relevant laws and shared the farmers' views on dispute settlement. This surprised Ellickson as a scholar of law and economics because that school of thought presupposes that people order their affairs on the basis of calculations of self-interest in relation to the strengths of their legal positions.

Shasta County is a rural area in Northern California that, important for Ellickson's interests, had "open" and "closed ranges" in the early 1980s when he carried out his field work. In open ranges, usually on land owned by the American federal government or timber companies and leased to the cattlemen, cattle are allowed to wander at will. In closed ranges, cattle must be confined within fences. What happens when cattle damage another farmer's crops illustrates the difference. In open range, the crop grower must bear the loss unless his crops were fenced in. In closed range, the cattle farmer is liable. Put differently, in open range, a grower must build fences to keep the cattle out. In closed range, a cattle farmer must build fences to keep his cattle in.

Ellickson examined three kinds of disputes: damage to crops by wandering cattle; obligations to repair, or contribute to repair, fences along joint boundary lines; and auto accidents involving cattle. An old and elaborate body of California law governs damages by different kinds of animals under different kinds of circumstances (negligently roaming versus purposely roaming animals; cattle and horses versus goats and swine; circumstances in which owners are liable without fault versus

circumstances in which they are liable only if negligent or reckless), but farmers never resorted to legal proceedings or invoked legal remedies to sort out these cases. Instead they resolved things by agreement, by self-help, and by threatening or seeking informal community disapproval of the cattle's owner. Often, though, they did nothing at all, knowing that other minor disputes and differences would inevitably later arise and that both parties would realize that the responsible party had some reciprocating to do. The farmers believed, and acted on the belief, that their obligations for damage by animals depended in large part on whether they were kept in open or closed range. Sometimes as a strictly legal matter they were right about that and sometimes they were wrong, but that did not make any difference to how the disputes were resolved.

California statutes provided detailed and precise rules on responsibility for fence building and maintenance. Shasta County farmers were mostly unaware of these laws but instead observed well-established conventions that largely but not entirely paralleled the statutory provisions. They were mostly based on "community welfare" norms rather than stakeholders' immediate self-interest. This apparently surprised Ellickson, as a law and economics scholar, since individuals operating outside systems of legal control are expected primarily to engage in self-interested competition.

Automobile accidents raised more complicated issues. Cattle farmers believed that the farmer should and would win in collisions with cattle in open range, but that the driver should and would win in collisions in closed range. Because of this, many open-range farmers did not insure themselves adequately or at all for liability for accidents in which their cattle were involved. California law made no such distinction. As a legal matter, the question in either kind of case was who if anyone is negligent, and how much of the responsibility for the accident is attributable to that negligence?<sup>8</sup> In practice, Shasta County farmers and residents dealt with accidents informally, between themselves, usually observing the (legally nonexistent) open- and closed-range distinction. They never resorted to courts or lawyers. The only court cases involved plaintiffs who were not locals.

What Ellickson learned was that the farmers often did not know or

<sup>8</sup> California is a "comparative negligence state," which means that responsibility for the loss and for making it good is apportioned on the basis of relative fault. In other U.S. states, "contributory negligence" by a plaintiff bars any recovery.

much care what the law was concerning matters of interest to themselves, preferred to and tended to resolve disputes informally, and observed social norms that could be described as based on community welfare premises.

*B. How Economists Understand Legal Problems*

As his work progressed, Ellickson felt obliged to read works by law and society specialists, who attempt to understand legal problems in their social contexts and who explore implications of social structures, functions, and norms for understanding institutions and practices. In the end, he concluded that “I must confess my suspicion that law-and-society scholars, because they better understand the importance of informal social controls, would better be able than the law-and-economics scholars to predict the essentials of what was found in Shasta County” (Ellickson 1991, p. 8).

Ellickson explains that most law and economics scholars adopt a view of Ronald Coase (1960), one of the movement’s founders, that “the state functions as the sole creator of operative rules of entitlement” (p. 4) and that many economists “rarely shrink from applying in every context the model of rational, self-interested, human behavior that they borrow from economics proper” (p. 7). Elsewhere, concerning deterrent effects, Coase wrote, “Punishment, for example, can be regarded as the price of crime. *An economist will not debate whether increased punishment will reduce crime*; he will merely try to answer the question, by how much?” (1978, p. 210; emphasis added). This may explain why economists, especially politically conservative ones, tend to conclude that increased penalties must in the nature of things have marginal deterrent effects and that capital punishment must deter homicide better than other penalties do.

John Donohue, one of the handful of widely respected senior American economists specializing in studies of the criminal justice system, observes that deterrence studies may implicitly challenge fundamental economic presuppositions. In responding to an article by Gary Becker, the “founder” of modern economic studies of the criminal law and punishment (Becker 1968), and judge Richard Posner, law and economics’ most famous expositor, Donohue observes that Becker analogizes punishment analyses to price theory: “Becker suggests that price theory can fill in where empirical evidence is lacking: capital punishment is akin to a rise in the price of murder and hence might be

expected to lessen the number of murders” (2006, p. 4). He asks whether many economists’ vigorous defense of traditional economic models of choice in relation to criminals’ behavioral choices is in effect a defense of price theory itself.

### *C. Conceptual Models of Punishment Effects*

Ellickson described a world that is more complicated than many economists assume it to be, one in which community welfare norms sometimes trump self-interest, and in which legal threats only occasionally influence behavior. He developed a five-level model of social control (Ellickson 1991, p. 131):

1. First party controls (self-control)
2. Second party controls (other persons in direct contractual relations)
3. Social controls (informal social controls through norms)
4. Organizational controls (enforcement of organizational rules)
5. Governmental (legal) controls (state enforcement through law).

This model is important for law and economics specialists because it makes clear that the fifth level, usually their focus, is but a small part of the story.<sup>9</sup> That would surprise few social scientists. Durkheim ([1893] 1933), for example, argued that the criminal law’s direct effects (levels 4 and 5) through deterrence are modest at best and not especially important; their indirect effects through their interactions with social norms (level 3) and social norms’ effects on private behavior (levels 1 and 2) are what matters.

Ellickson’s model is also relevant, in a loose way, for thinking about deterrence of criminal behavior by means of legal threats. Whether individuals do or do not engage in criminal behavior is determined by a mixture of personal, situational, social, and organizational factors in addition to the criminal law’s legal threats.

Many sociological and psychological models of criminality resemble Ellickson’s but are considerably more complex as they attempt to iden-

<sup>9</sup> The fourth level, organizational behavior and controls, which economists studying punishment generally ignore, is where laws are and are not applied. Typical studies of deterrent effects of increased penalties nearly always treat statutory changes as the indicators of changes in sanction threats, without attending to the questions whether officials have the capacity or the will to implement the increased threats. As the literature on mandatory minimum sentence laws demonstrates (Tonry 1996, chap. 5), one or both are typically lacking.

tify and measure the myriad personal, social, situational, and environmental factors associated with criminal behavior. A recent meta-analysis by Pratt et al. (2006) attempts to measure the much more nuanced deterrence effects that such research investigates.

Wikström's (2007) theoretical overview of deterrence tries to explore interactions between moral socialization and legal threats. He concludes that

law abidance is largely a question of an individual's *moral education* (of which their deterrence experiences are a part) through which they have developed moral rules and moral habits that preclude them from engaging in crime. I have also argued that individuals breach the law either out of *habit*, or by making a *deliberate choice*, and that only in the latter case is there a question of whether or not *deterrence* may influence their choice to abide by or breach the law. I have also maintained that *rationality* and an individual's *capability to exercise self-control* come into play as factors only when an individual deliberates over action alternatives . . . Crime is fundamentally a question of morality and moral habits. (2007, ms. p. 37; emphasis. in original)

Ellickson's book, though it is not about criminal law, punishment, or deterrence, nonetheless provides a platform for considering those subjects. Shasta County farmers have little knowledge of the law relevant to the disputes they most commonly encounter. Their disputes are resolved primarily through interactions in Ellickson's first three levels (personal morality, contractual relationships, and local social norms). There is little reason to suppose that most would-be offenders most of the time operate in Ellickson's fifth level.

## II. Reviews of Deterrence Research

Knowledge of the deterrent effects of criminal punishments is little different in 2007 than it was in 1978 when the U.S. National Academy of Sciences Panel on Deterrence and Incapacitation issued its report (Blumstein, Cohen, and Nagin 1978). A substantial body of work has since accumulated, some of it on new subjects, but the main substantive conclusions to be drawn have changed little.

The panel was convened to evaluate the evidence underlying Isaac Ehrlich's claim that each execution in the United States prevented eight murders through its deterrent effects (Ehrlich 1975). More gen-

erally, it assessed research on deterrence and incapacitation that had accumulated in the aftermath of publication of Gary Becker's influential early article "Crime and Punishment: An Economic Approach" (Becker 1968). There were three principal conclusions on deterrence:

1. Taken as a whole, the criminal justice system has a general deterrent effect.
2. No conclusions can be reached on whether capital punishment deters homicides.
3. Though the evidence is unclear, it is likely that marginal changes in punishments have marginal deterrent effects.

Were a similar panel convened today, it would discuss additional subjects, notably research on threat perception, natural experiments, and the deterrent effects of concealed weapons laws. It would, however, affirm two of the 1978 conclusions: the overall system deters and no evidence-based conclusions can be reached about capital punishment. On the third question—marginal deterrence—it would discuss stronger support for the existence of marginal deterrent effects for some crimes under some circumstances but conclude, as a practical matter, that few policy changes can reasonably be expected to achieve those effects. On the fourth substantive question—whether allowing private citizens to carry concealed firearms in public places reduces crime—the panel would decide that no conclusion can be reached (as an NAS panel convened partly to address that question did conclude [Wellford, Pepper, and Petrie 2005]).

The critical question for policy makers is whether marginal increases in penalties can reasonably be expected to reduce the incidence of crime. Outside the United States, for example, in other English-speaking countries or in most European countries, the other three questions have little policy relevance. Death penalty statutes and laws allowing citizens to carry concealed firearms in public are not in the offing. No one seriously doubts that the system as a whole has some deterrent effects, compared with a hypothetical situation in which there were no criminal penalties; but in any case, no real-world policy makers would consider doing away with criminal penalties.

A considerable number of exhaustive reviews of deterrence research have been commissioned and published since the NAS panel issued its report in 1978. Most have reached pretty much the same conclusions. The Home Office of England and Wales commissioned a multiyear



review of the evidence (von Hirsch et al. 1999). Three widely cited and influential reviews have been published in *Crime and Justice* (Cook 1980; Nagin 1998; Doob and Webster 2003), and other reviews have been published elsewhere (e.g., Pratt et al. 2006) or soon will be (e.g., Bushway, forthcoming). Three reviews by economists (Lewis 1986; Levitt 2002; Levitt and Miles 2007), to the contrary, concluded that increased penalties produce lower crime rates through deterrence; these reviews draw almost entirely on analyses by economists, and neither cite nor discuss the larger deterrence literature produced by non-economists.<sup>10</sup> The most recent major review, a meta-analysis by Pratt et al. (2006), examines a wide range of multivariate studies in sociology, psychology, and criminology that test deterrence effects (broadly defined, to include certainty and severity of punishments, effects of different kinds of punishments, and effects of nonlegal social and shaming consequences of crime).

As a practical matter of criminal justice policy, the critical question is whether marginal changes in sanctions have measurable deterrent effects. The major broad-based reviews reach similar conclusions that no credible evidence demonstrates that increasing penalties reliably achieves marginal deterrent effects.

In 1980, Cook concluded that existing studies showed that “there exist feasible actions on the part of the criminal justice system that may be effective in deterring [certain] crimes . . . . [But the] studies do *not* demonstrate that all types of crimes are potentially deterrable, and certainly they provide little help in predicting the effects of any specific governmental action” (1980, p. 215; emphasis in original).

In 1998, Nagin observed that he “was convinced that a number of studies have credibly demonstrated marginal deterrent effects,” but concluded that it was “difficult to generalize from the findings of a specific study because knowledge about the factors that affect the efficacy of policy is so limited” (1998, p. 4). He highlighted four major factors: the relation between short- and long-term effects, the relation between risk perceptions and sanctions policies, the methods of implementation, and the extent of implementation.

Von Hirsch et al. conclude that “there is as yet no firm evidence regarding the extent to which raising the severity of punishment would enhance deterrence of crime” (1999, p. 52).

<sup>10</sup> The reviews by social scientists, to the contrary, almost always discuss the economics literature. I comment on this curious pattern in the conclusion.

Doob and Webster, adopting the null hypothesis approach, noted in 2003 some inconclusive or weak evidence of marginal deterrence but concluded that “there is no plausible body of evidence that supports policies based on this premise [that increased penalties reduce crime]. On the contrary, standard social scientific norms governing the acceptance of the null hypothesis justify the present (always rebuttable) conclusion that sentence severity does not affect levels of crime” (2003, p. 146).

The three works by economists, summarizing work principally by economists, find that increases in punishment achieve marginal deterrent effects. Lewis describes “a substantial body of evidence which is largely consistent with the existence of a deterrent effect from longer sentences” (1986, p. 60). Levitt, relying principally on data from two of his own analyses, describes them as evidence “for a deterrent effect of increases in expected punishment” (2002, p. 445). Levitt and Miles conclude that “the new empirical evidence [produced by economists] generally supports the deterrence model. . . . Evidence of the crime-reducing effects of the scale of policing and incarceration is consistent across different methodological approaches” (2007, p. 456).

The Levitt and Miles (2007) review is the most recent. In discussion of research on shall-issue laws and deterrent effects of capital punishment above, I have, using quotations from this article, indicated the authors’ skepticism about deterrent claims based on those literatures. In addition, they discuss evidence on the effects of increasing police numbers; they conclude that increased numbers are associated with declines in crime rates but are unable to conclude whether this is for deterrent (more visible policing provides disincentives to offending) or incapacitative reasons (more high-rate offenders are apprehended and incarcerated (pp. 468–70).

Relatively little attention is paid to the marginal deterrence hypothesis. Most of the discussion of sanctions other than the death penalty (pp. 470–74) considers whether increases in the scale of imprisonment have reduced crime rates and, concluding that it has, to what degree that effect has been achieved through deterrence and to what degree through incapacitation. Levitt (1996) himself conducted one such study in which, using aggregate state-level police arrest data, he attempted to learn whether states in which courts ordered prisons to reduce their populations experienced higher crime rate increases than states that were not subject to such orders (he concluded that each released pris-

oner produces an additional 15 crimes annually). Marvell and Moody (1994) looked at the effects of increased imprisonment rates on crime rates and concluded that increased imprisonment yielded lower crime rates though the estimates were considerably lower than Levitt's. A major difficulty with aggregate research of this kind is that it does not address the marginal deterrence hypothesis, and thus provides no guidance to policy makers wondering whether increased incarceration of bicycle thieves or street robbers will reduce bicycle theft or robbery. A second difficulty is that it provides no insight into whether asserted crime rate reduction effects result from incapacitation or deterrence.

One article by Kessler and Levitt (1999) gets closer to testing the marginal deterrence hypothesis. It attempts to identify reductions in crime rates resulting from the passage in 1982 of California's Proposition 8 (which provided sentence enhancements for designated crimes) and, concluding that there was a crime reduction effect, to disentangle its deterrent and incapacitative elements. However, as Webster, Doob, and Zimring (2006) demonstrate, Kessler and Levitt fell prey to a classic mistake: by examining data at 2-year intervals, the analysis missed a longer-term downward trend in crime rates for all five crimes examined (homicide, rape, robbery, aggravated assault with a firearm, and residential burglary), which adequately explained the reduction in crime rates. For four of the five offenses, crime rates peaked 2 years before passage of the referendum and continued afterward, making the continuing decline as likely to be the continuation of preexisting trends as the result of the policy change.<sup>11</sup>

The meta-analysis by Pratt et al., by contrast citing no economists, produced a main finding, one "noted by previous narrative reviews of the deterrence literature," that "the effects of severity estimates and deterrence/sanctions composites, even when statistically significant, are too weak to be of substantive significance (consistently below  $-.1$ )" (2006, p. 379).

<sup>11</sup> This is a common pattern. California crime rates began to fall 3 years before its three-strikes law was enacted, providing a rich opportunity to politicians to compare rates the year before the new law was enacted with rates afterward and to claim that the law caused the decline (Zimring, Hawkins, and Kamin 2001). In the 1970s, the initial evaluations of California's Uniform Determinate Sentencing Law (1976) concluded that compared with the year before enactment the law produced higher prison commitment rates and reduced sentence lengths. Subsequent evaluations that looked at longer time series showed that both patterns began several years before the law was enacted, making the subsequent patterns merely the continuation of preexisting trends (Blumstein et al. 1983, chap. 4).

That article, however, potentially illuminates a useful role deterrence theory might play in understanding individual, social, and structural influences on behavior. That would be to integrate deterrence ideas and theories into much more nuanced accounts of human behavior that allow for the contingent influences on behavior of individual, social, and structural conditions; variations in threat perceptions; and variations in the nature and degree of implementation of crime prevention strategies. They note, for example, four major theoretical developments that have influenced much recent research on deterrence by social scientists. First, routine activities theories and their policy progeny, situational crime prevention programs, assume predisposed offenders and seek to prevent crime through manipulation of material opportunity structures. Second, a considerable body of research in the past 15 years has emphasized the importance of risk perceptions of the social costs (e.g., shame, loss of others' respect) associated with punishment. Third, a variety of efforts have been made to embed deterrence analyses in other theoretical frames such as self-control and experiential learning (through one's own and others' experiences) theories. Fourth, punishment interaction theories stress the important influences on individuals' threat perceptions of their own experiences with the justice system (that experience might enhance or reduce the effects of perceived legal threats). Analyses of these kinds are much more in keeping with the analyses by Ellickson (1991) and Wikström (2007) that were discussed above than they are with traditional analyses by economists that attempt to relate changes in behavior directly to changes in sanctions while ignoring all the intervening stages and processes. Levitt and Miles acknowledge, by contrast, that "the economic model of crime differs from the major branches of criminology in that it abstracts from the social processes and psychological aspects of offending and emphasizes individual choices. A cost of the economic approach is thus a loss of the social context of offending" (2007, p. 462).

I mention these developments because they point to the need for much more precise delineation of deterrence questions than to ask globally whether changes in sanctions lead to changes in criminal behavior. Whether people engage in particular actions in particular places at particular times depends on their circumstances, characteristics, and predispositions; the criminal opportunities and precipitants they face; and the perceived consequences to themselves and others. The emerg-

ing literature that focuses on and attempts to measure the effects of events (e.g., arrests) that might alter perceptions is illustrative (Matsueda, Kreager, and Huizinga 2006; Lochner 2007).

### III. Economists on Concealed Weapons and Capital Punishment

Two new deterrence literatures emerged in economics beginning in the mid-1990s. One, associated with economist John Lott, examined the effects of states' enactment of shall-issue laws that required state officials to issue permits allowing citizens to carry concealed firearms in public. Although state laws varied in detail, the only circumstances in which permits could generally be denied involved criminal convictions for designated crimes and certain diagnosed mental conditions. The other literature investigated the effects on homicide rates of states' enactment of death penalty laws in the aftermath of the U.S. Supreme Court's decision in *Gregg v. Georgia*, 428 U.S. 153 (1976), upholding capital punishment laws meeting specified conditions.

Both literatures followed trajectories resembling that of Ehrlich's (1975) finding that each execution would prevent eight murders. Each attracted substantial attention, influenced policy making in important ways, or was invoked by policy makers to justify their support for new penalty laws, and was subsequently shown by other scholars to be unsound. In Lott's case the repudiation included allegations of "manufacturing data" (Donohue 2004, p. 623).<sup>12</sup>

Both literatures are based on econometric models using Uniform Crime Reports data on state and county arrest rates for serious offenses. They typically use time-series data to compare arrest rate trends for states enacting the laws hypothesized to have deterrent effects before and after the legal change and to compare arrest rate trends in those states with trends in states not enacting such laws. Both literatures are confounded by America's declining crime rates for 20 of the 25 years between 1981 and 2005 (declines in every year except the period 1986–91). In principle that should have been relatively easily soluble, but in practice it was not. The models in both literatures were highly sensitive to specification problems, with effects disappearing or

<sup>12</sup> The editor of *Science* (Kennedy 2003) suggested that a committee of scholars be appointed to investigate Lott's behavior.

signs reversing with inclusion or exclusion of particular states and years.

#### *A. Concealed Weapons*

Lott and Mustard's (1997) original analysis used annual cross-sectional time-series county-level arrest data for all 3,054 counties in the United States for the years 1977–92.<sup>13</sup> Concealed weapons laws were in effect in eight states in 1977, and 10 other states enacted them between 1977 and 1992.

The findings were strong:

1. "When state concealed handgun laws went into effect in a county, murders fell by 7.65 percent, and rapes and aggravated assaults by 5 and 7 percent" (Lott and Mustard 1997, p. 19).
2. "If the rest of the country had adopted right-to-carry concealed handgun provisions in 1992, at least 1,414 murders and over 4,177 rapes would have been avoided" (Lott and Mustard 1997, p. 64).
3. "The annual declines in crime from right-to-carry laws are greater for murder (2.2 percent), rape (3.9 percent), and robbery rates (4.9 percent), while the impact on aggravated assaults (0.8 percent) and the property crime rates (0.9 percent) is smaller" (Lott 2000, p. 172).
4. "For each additional year that the laws are in effect, murders fell by an additional 1.5 percent, while rape, robbery, and aggravated assaults all fell by about 3 percent each year" (Lott 2000, p. 170).

The most exhaustive surveys by economists of research on the deterrent effects of enactment of concealed weapons laws conclude that Lott's analyses are unpersuasive (Ayres and Donohue 2003*a*, 2003*b*; Cook and Ludwig 2003; Donohue 2004). Cook and Ludwig conclude that "the best empirical evidence does not support" Lott's conclusions (2003, p. 595). Ayres and Donohue, having tested more than 700 alternate regressions, concluded that there is "no credible statistical evidence that the adoption of concealed-carry (or 'shall issue') laws reduced crime" (2003*a*, p. 1372). The leading reanalysis of Lott's data by quantitatively sophisticated noneconomists reached the same conclusion (Black and Nagin 1998).

<sup>13</sup> The first edition of Lott (1998) updated the data through 1994; the second (2000), through 1996.

Levitt and Miles (2007) review the literature on shall-issue laws and express skepticism. They note that other analyses, including one that Levitt coauthored (Donohue and Levitt 2001), identify a long series of problems that “raised questions” about the validity of the concealed-weapons hypothesis.<sup>14</sup>

### *B. Capital Punishment*

The disjuncture between conservative economists and everyone else concerning the deterrent effects of capital punishment parallels that concerning carrying concealed firearms. The most cited recent econometric 50-state analysis (partly using Lott’s data) concluded that each execution saves 18 lives (Dezhbakhsh, Rubin, and Shepherd 2003). The most exhaustive critique of this literature by economists, which tested the robustness of existing studies to alternative sample periods, comparison groups, control variables, functional forms, and estimators, concluded that “our key insight is that the death penalty—at least as it has been implemented in the United States since *Gregg* ended the moratorium on executions—is applied so rarely that the number of homicides it can plausibly have caused or deterred cannot be reliably disentangled from the large year-to-year changes in the homicide rate caused by other factors” (Donohue and Wolfers 2005, p. 794). An exhaustive critique of the recent economic literature by quantitatively sophisticated noneconomists reached the same conclusion (Fagin, Zimring, and Geller 2006). Levitt and Miles concurred. After discussing a long list of critiques of recent death penalty research, they observed that “a large deterrent effect is surprising given the relatively abstemious application of the death penalty. . . . Individuals who regularly participate in criminal activities with such hazards [high death rates among street gang members and narcotics traffickers] are unlikely to be influenced by the relatively low risk of capital punishment” (2007, p. 476).

<sup>14</sup> These concluded that estimates “lack statistical significance when the assumption of the statistical independence of counties within the same state is relaxed”; that “passage of a concealed weapons law did not correlate with a proxy for the rate of gun ownership”; and that “after controlling for abortion rates, the laws did not correlate with crime rates.” They also observed that “further tests of the behavioural implications of the concealed-weapons hypothesis have also raised questions about its validity” (Levitt and Miles 2007, p. 477).

#### IV. Whither Deterrence Research?

Three overriding themes emerge. First, economic and econometric studies of deterrence effects using aggregate data are unlikely to shed useful insights into the deterrent effects of punishment. They are conducted at the wrong level of analysis and are incapable of taking into account vagaries in implementation of sentencing laws, situational and circumstantial influences on offender decision making, or offenders' perceptions of risk. Second, advances in understanding of offender decision making are likely instead to emerge from work that takes account of offender characteristics (e.g., values, self-control), offender interactions with other people, and informal social control in organizational and legal contexts. Third, closer and richer studies of implementation of sentencing laws and punishment practices may increase understanding of cultural acceptance of punishment options and hence of socially optimal levels and kinds of punishment.

A recent publication of the National Bureau of Economic Research provides another reason to be skeptical of the findings of the economics and econometrics literatures on deterrence. It is understandably difficult for scholars to know much about legal systems other than their own. American economists generally analyze only American data and only data for recent decades. As Dills, Miron, and Summers (2008) demonstrate, when analyses are extended to incorporate data from other countries or are extended within the United States to cover longer periods, few of the findings of recent economic research on crime and punishment appear to be substantiated.

Only nuanced behavioral and social science analyses of the types exemplified by Ellickson (1991) and Wikström (2007) and included in the meta-analysis of Pratt et al. (2006) are likely to add significantly to current understanding of the influence of changes in law enforcement practices and sentencing policies on criminal behavior. The policy implications of such knowledge as emerges will lie primarily at the lowest three levels (self-control, second-party influences, and social control) of Ellickson's five-level model. The implications of new knowledge are unlikely to have much relevance to development of policy at governmental (legal) or criminal justice operational (organizational) levels. New knowledge may have implications at organizational levels for non-criminal justice agencies involved in formulation and implementation of public health, educational, and social welfare policies.

It is unclear to me which is more surprising: that so little credible



evidence exists that criminal behavior is much affected by changes in punishment policies or that policy makers continue to believe that policy changes significantly affect behavior and that research continues to test for their crime-preventive effects. Although I have observed in this essay that understanding of deterrent effects has changed little over the past 30 years, it can also be said that understanding has changed little over the past two centuries.

Two images from eighteenth-century England when the number of offenses punishable by death greatly increased make the point: pickpockets actively at work among the crowds assembled for the executions of pickpockets (Teeters 1967); and English juries regularly nullifying the criminal law by refusing to convict obviously guilty defendants of crimes punishable by death (Hay et al. 1975). The literature on mandatory penalties mentioned in the introduction reaches similar findings: prospective penalties are seldom determinative of what would-be offenders do, and officials regularly alter their practices in order to circumvent application of penalties they consider too severe (e.g., Tonry [1996, chap. 5] presents lots of examples).

There are good reasons why little research focused primarily on the deterrent effects of changes in laws setting or changing authorized or mandated criminal punishments has recently been carried out. The research findings are so robust and so long-standing that most specialists believe we now know most of what we are likely ever to know about the deterrent effects of sanction changes. Implementation effects may be another matter. Research on local legal cultures and courtroom workgroups as organizations may shed more nuanced insights into how practitioners adjust their operations and policies to react to new or altered sanctioning policies.

Only economists seem to conclude regularly that research will demonstrate that penalty changes in general significantly affect crime patterns and rates or that capital punishment in particular is a more effective deterrent than other punishments that might be imposed. For example, Joanna Shepherd, an author of several studies finding a deterrent effect, in 2004 testified before the U.S. Congress that there was a “strong consensus among economists that capital punishment deters crime” and that “the studies are unanimous” (U.S. House 2004, pp. 10–11). Setting aside the problem that it is true neither that the studies are unanimous nor that there is a consensus among economists, why

might Shepherd believe her statements to be true? There are a number of possible explanations.

Economists writing about deterrence do not seem much to read work by noneconomists on the same subject. The surveys by economists of knowledge about deterrence seldom discuss work by noneconomists (Lewis 1986; Levitt 2002; Levitt and Miles 2007). This might be seen as the result of disciplinary insularity or of disciplinary hubris; but whatever the explanation, among the consequences are apparent lack of understanding of fundamental problems with the sources of aggregate national data that are analyzed and lack of knowledge of processes of implementation and of offender ratiocination and motivation.

As works cited above by Gary Becker (1968), Ronald Coase (1978), and Isaac Ehrlich (1996), all quoted earlier in this essay, make clear, many economists assume something to be true that social scientists regard as merely a hypothesis: that offenders' choices to commit particular crimes are the products of rational calculation of the likely economic gains of particular crimes offset by the likely risks of punishment compared with likely net economic gains of available lawful employment. Other social scientists' models of offender (and nonoffender) decision making are considerably more complex and contingent.

Ronald Coase, in an essay discussing economists' incursions into substantive realms more commonly inhabited by other kinds of social scientists, and their retreats, may have explained why many economists' work on deterrence at macro levels has the characteristics it has. He makes two relevant arguments. The first is that economists' contributions will depend on whether their comparative advantage is in technique, theory, or substance. Theory he sees as derivative primarily from substance, and technique is not enough: "to the extent that [economists'] movement [into a substantive area] is based on technique or approach, we can expect a gradual displacement of economists from their newly-won ground" (Coase 1978, p. 205).<sup>15</sup>

The second is that "the great advantage that economics has possessed is that economists are able to use the 'measuring rod of money,'" which gives precision to their analyses (Coase 1978, p. 209). Since money is an important determinant of human economic behavior, the resulting analyses have plausible explanatory power. "It by no means

<sup>15</sup> See the quotation from this article in n. 1.

follows [however] that an approach developed to explain behaviour in the economic system will be equally successful in the other social sciences. In these different fields, the purposes which men seek to achieve will not be the same, the degree of consistency of behaviour need not be the same and, in particular, the institutional framework within which the choices are made are quite different. . . . [Understanding all this] will require specialized knowledge not likely to be acquired by those who work in some other discipline" (p. 208).

Coase's two points may explain why, after the flurry of work by economists on capital punishment in the 1970s, little was published until the 1990s. Ehrlich's work could not withstand close scrutiny by people possessed of comparable technical skills but vastly greater substantive knowledge (Blumstein, Cohen, and Nagin 1978). The work by economists since the mid-1990s on capital punishment and shall-issue laws has also followed the trajectory Coase predicted.

Finally, some or much of the work on deterrence by economists may be conscious or unconscious products of ideological, as opposed to merely disciplinary, ways of thinking. Dan M. Kahan (1999) offered an informative analysis of deterrence arguments in which he suggests that they are generally normative arguments in disguise. Americans hold widely divergent intuitions about the purposes of punishment, including whether for moral reasons killers should be killed. Disagreements based on deeply held moral intuition are seldom resolvable by resort to argument. Kahan suggests that seemingly technical and empirical arguments about deterrence are really camouflaged normative arguments: "The rhetoric of deterrence displaces an alternative expressive idiom . . . . Ultimately the deterrence idiom takes the political charge out of contentious issues and deflects expressive contention away from the criminal law" (1999, pp. 416–17). Put differently, many people who believe that capital punishment is morally permissible and in some cases is morally required—who believe that the state should kill people—are often uncomfortable saying that explicitly. Invocation of empirical evidence that capital punishment deters homicides provides a more comfortable rationale for laws they support for other reasons. The same analysis applies to shall-issue laws. Many of their proponents are Second Amendment ideologues. Lott's research provides an empirical fig leaf to cover what is often an ideological commitment.

Many of the economists who have written on the deterrent effects of punishments are well-known political conservatives—Gary Becker,

Richard Posner, Isaac Ehrlich, John Lott—and others such as Joanna Shepherd are less well-known conservatives. It is merely human to be deeply attached to one's intuitions. Robert Nozick (1981, pp. 2–3), writing of philosophers, observed, “When a philosopher sees that premisses he accepts logically imply a conclusion he has rejected until now, he has a choice: he may accept this conclusion or reject one of the previously accepted premisses . . . . His choice will depend upon which is greater, the degree of his commitment to the various premisses, or the degree of his commitment to denying the conclusion. It is implausible that these are independent of how strongly he wants certain things to be true.” So it may be with economists. John Donohue (2006) argued that his fellow economists are committed to price theory and a model of rational self-interested behavior, and this may make it exceedingly difficult to accept that price theory is less apposite to many forms of offending than it is to some other forms of human behavior.

This review of the deterrence literature supports three main conclusions concerning future research. First, good research designs for measuring the marginal deterrent effects of sanctioning changes on would-be offenders' behavior must be much more fine-grained in the questions they attempt to answer and in the ways they try to answer them. Models or designs aimed at investigating effects of changes in laws and policies or organizational practices (Ellickson's top two levels), but that do not take account of social norms, personal obligations, social contexts, and offenders' characteristics (his bottom three levels), will not teach us very much.

There are some realms in which enforcement strategies and sanctioning changes affect behavior and in which research on the effects of legal threats may usefully influence policy. Examples include tax compliance and evasion, illegal parking, and speeding. No doubt there are others. In the Netherlands, for example, where there are more bicycles than people, policy makers may need to develop effective ways to respond to increases in bicycle theft. Or in any country, increased levels of theft or robbery of new electronic gadgets create policy needs for new preventive approaches. Often the best responses will be technological and situational, as, for example, when development of thief-resistant automobile locks led to reduced auto theft rates in many countries, but sometimes they may include changes in police practice or sanctions policies. Well-designed micro-level studies that take ac-

count of implementation patterns, offense circumstances, and offenders' perceptions may well be able to provide important policy guidance (Tonry 2008).

Second, following Doob and Webster (2003), research on the deterrent effects of sanctions and sanctions changes, especially by economists, should test the null hypothesis that sanctions changes have no effects on offenders' behavior. Isaac Ehrlich, by contrast, has observed that the

“market model” . . . builds on the assumption that offenders, as members of the human race, respond to incentives. . . . This has been the justification for applying economic analysis to all illegal activities, from speeding and tax evasion to murder. Indeed, the distinguishing feature of the major contributions by economists has been the attempt to explain the various aspects of crime through the tools of organization and equilibrium analysis, rather than by reliance on deterministic social and environmental factors that are independent of the human will. *At least in the economic literature*, there has been little controversy concerning this approach. (1996, pp. 43–44; emphasis added)

There has, however, been substantial controversy among noneconomists about that approach. No one argues that (sane) offenders lack rationality, but many people do argue that credible research must take account of offenders' personal values and norms, of their knowledge of sanctions and sanctions changes, of the considerations and circumstances that shape offenders' choices, and of the nature and extent of implementation of legal or policy changes. So far none of the major economic literatures on the effects of sanctioning changes has withstood scrutiny by social scientists or by other economists.

Third, considerably more money and energy should be invested in studies of the implementation of legal changes intended to alter patterns of criminal punishment. A research finding that a change in sanctions policies had no significant effects on offending should come as no surprise if an implementation study showed that the change had no effects on sanctions imposed. Put another way, implementation studies examine a new policy's effects on officials' behavior. Sometimes new policies have little effect because they overload organizational systems or have resource implications that cannot be managed. Other times they have little effect because they call for dispositions that officials believe to be unjust or inappropriate and as a result do not impose.

Not so long ago, researchers developing decision tools for judges and parole boards viewed official noncompliance with punishment guidelines as indication that the guidelines were out of step with prevailing notions of justice, and thus as a sign that the guidelines themselves needed changing (Gottfredson, Wilkins, and Hoffman 1978). So it may be with many of the punishment innovations of the past 30 years. We may learn more from studies on the effects of changes in punishment laws and practices if we more often investigate why they do not operate as intended rather than whether they do.

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