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ECONOMIC CONTRIBUTIONS TO THE UNDERSTANDING OF CRIME

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■ **Abstract** The past decade has seen a sharp increase in the application of empirical economic approaches to the study of crime and the criminal justice system. Much of this research has emphasized identifying causal impacts, as opposed to correlations. These studies have generally found that increases in police and greater incarceration lead to reduced crime. The death penalty, as currently used in the United States, does not appear to lower crime. We also review the evidence on three other crime-related debates in which economists have played a central role: racial profiling, concealed weapons laws, and the impact of legalized abortion.

INTRODUCTION

The casual observer might expect that economics has little to contribute to the understanding of criminal activity. Economics is a discipline seemingly concerned with market-based transactions in which parties act purposefully to realize the benefits of exchange. In contrast, many criminal acts, such as homicide and theft, are inherently nonconsensual, even coercive. Moreover, many crimes appear to be acts of impulse or emotion rather than the kind of rational decision making associated with market behavior. Despite this apparent mismatch of methodology and subject matter, the economic approach has made significant contributions to the study of crime, and in the past decade, the pace of these contributions has accelerated. In this review, we examine some of the recent contributions that economics has made to the understanding of crime, and, as much of the recent work by economists on crime is empirical rather than theoretical, we emphasize empirical research over theory. The discussion is organized around two broad categories of influences on criminal behavior: (*a*) the criminal justice system and (*b*) other legal rules that, although distinct from the criminal justice system, nonetheless potentially influence criminal behavior.

As an initial matter, it is worth clarifying what we mean by the phrase economic approach. We believe four characteristics distinguish the economic approach to the study of crime from that of other social sciences. These characteristics are (*a*) an emphasis on the role of incentives in determining the behavior of individuals, whether they are criminals, victims, or those responsible for enforcing the law; (*b*) the use of econometric approaches that seek to differentiate correlation from causality in nonexperimental settings; (*c*) a focus on broad, public policy implications rather than evaluation of specific, small-scale interventions; and (*d*) the use of cost-benefit analysis as the metric for evaluating public policies.[1]

The emphasis on incentives in determining behavior arises from the assumption in economics that individuals maximize their utility subject to constraints. While counterexamples of this assumption surely exist, particularly in the context of criminal behavior, a plausible generalization about human behavior is that most individuals do the best they can with what they have. Becker (1968) presented the first modern economic model of criminal behavior. In it, actors prospectively compare the expected costs and expected benefits of offending. They commit crimes when the expected gains exceed the expected costs, and otherwise refrain from criminal acts. The model incorporates the criminal justice system as part of the offender's expected costs, with policing influencing the probability of punishment and fines representing the criminal sanction. In the model, the criminal justice system therefore reduces crime through deterrence. Since Becker's influential article, a large and active literature on the theory of deterrence has developed, with many elaborations and refinements of the core Becker model [see Kaplow & Shavell (2002) and Polinsky & Shavell (2006) for comprehensive reviews].

The emphasis in economics on identifying true causal impacts of particular crime-control policies also distinguishes it from other modes of research. Questions, such as the impact of the size of police forces or the scale of imprisonment on crime, are vexingly difficult to answer because crime rates influence the formation of public policies that combat crime. For example, if police reduce crime, but crime leads to the hiring of police, standard correlational statistical approaches such as ordinary least squares estimation do not capture the causal impact of these policies. Rather, these estimates suffer from what econometricians term simultaneity bias. Despite this formidable challenge, economists have enjoyed significant progress in recent years in addressing these issues. In so doing, they have found various components of the criminal justice system to be effective, but not always cost-effective, means of reducing crime.

The third distinguishing feature of the economic analysis of crime is the emphasis on broad, public policy implications rather than on the assessment of particular, small-scale policy interventions. Economists typically conduct analyses at the level of geographic aggregates, which serve as a proxy for individual behavior and permit comparisons across jurisdictions with different criminal justice

[1]Not all economic studies of crime are done by economists. A number of researchers who are not economists have done research that fits our broad definition of the economic approach (e.g., Bushway 2004).

policies. This emphasis on a broader level of analysis does not discount the importance of context. Rather, for economists interested in testing Becker's prediction that criminal actors respond to incentives in the form of deterrence, the goal is to understand human behavior in general rather than in particular settings. Similarly, for economists interested in evaluating policy, a frequent objective is to provide analysis that is sufficiently general to assist policy makers in numerous contexts.

The fourth defining characteristic of the economic approach to crime is the use of cost-benefit analysis. In economic theory, a widely used normative criterion is Pareto efficiency, the condition in which no reallocation of resources can make one person better off without leaving another person worse off. In practice, economists typically assess the desirability of a social program by weighing its costs and benefits, relative to other alternatives. These comparisons necessarily involve counterfactuals or predictions of the outcomes that would be achieved under the alternative, and what is especially useful in making these predictions are the arguably causal estimates provided by empirical economic analysis. Moreover, cost-benefit comparisons express the many dimensions of a decision in a single metric of price or money. The idea of a common unit of measurement has its limits; even many economists would resist the notion that all human values can be reduced to monetary equivalents. Despite these limitations, cost-benefit analysis furnishes a coherent normative criterion that is especially appropriate in the context of crime control, where the menu of policy alternatives is extensive and hundreds of millions of dollars are expended.

The remainder of this review is structured as follows. The next section discusses the economic literature regarding the effectiveness of three specific aspects of the criminal justice system: police, prisons, and capital punishment. In addition to examining the impact of the criminal justice system on crime rates, some economists have studied how the criminal justice system itself operates. Although a complete review of economic research on the operation of the criminal justice system is not possible here, a topic that has received much attention recently is racial profiling, and thus the next section also describes the work on racial profiling. The third section reviews the economic contributions of two determinants of crime outside the criminal justice system: the victim precaution of concealed weapons and the influence of legalized abortion. Both of these issues have been active areas of academic debate in recent years.

MEASURING THE IMPACT OF THE CRIMINAL JUSTICE SYSTEM ON CRIME

The Bureau of Justice Statistics estimates that nearly $200 billion is spent each year in the United States to catch, prosecute, and punish offenders (Bauer & Owens 2004). Measuring the effectiveness of this spending is a critical input into the development of good public policy, both in terms of allocating criminal justice dollars and of determining whether money is better spent on punishing today's offenders or on resources to lower the criminality of future cohorts.

As mentioned in the introduction, the key issue in measuring the impact of components of the criminal justice system is the identification of causal relationships. A step in isolating a causal effect of a criminal justice policy is to exclude the influence of other factors bearing on the frequency of criminal activity. To do so, economists often mimic the empirical design of medical studies. They compare outcomes in a set of jurisdictions that adopted a particular policy or law before and after its implementation. Consciously analogizing to medical research, economists typically refer to persons who receive and the jurisdictions that adopt the particular policy or law as the treatment groups. Economists also draw these comparisons relative to a set of jurisdictions that did not adopt the particular treatment, and this second set of jurisdictions acts as a control group. This empirical design draws comparisons across two dimensions—before and after, and treatment and control—and is therefore known as difference-in-differences.

The application of a difference-in-differences framework does not assure the accurate measurement of a causal impact because, unlike treatment groups in medical experiments, the set of jurisdictions adopting a particular crime-control policy is not randomly selected for two reasons. First, the crime-fighting policies that jurisdictions adopt are often themselves products of crime rates. For example, the amount of resources allocated to the criminal justice system is typically greater when crime is higher. Crime-fighting policies set the incentives to engage in crime and at the same time respond to the incidence of crime. Second, the social and historical factors that influence the adoption of specific laws and enforcement policies may also affect the incidence of crime directly. Social norms in the South, for instance, might lead southern states to use more freely capital punishment, and aside from any impact of capital punishment, these norms may directly affect southern crime. Econometricians refer to the simultaneous determination of policies and outcomes as the simultaneity problem, and this problem bedevils measurements of the impact of crime-control policies on crime rates. A failure to control for such biases risks serious mismeasurement of the impact of crime-control policies. We therefore review empirical attempts to deal with these difficult questions topic by topic. We begin with studies estimating the impact of police on crime, before examining the scale of imprisonment and the enforcement of the death penalty.

Measuring the Impact of Police on Crime

The first generation of empirical research on police and crime consisted of cross-sectional studies that compared policing and crime rates across jurisdictions, typically cities or states, at a point in time. In his comprehensive review of the literature, Cameron (1988) reported that a majority of these papers found either no relationship or a positive relationship between the level of police and crime rates. The cross-sectional methodology of these papers provided a poor test of the causal effect of police on crime because it failed to correct for the simultaneity problem. Jurisdictions with higher crime rates typically respond by hiring more police, whereas jurisdictions with lower crime rates need to employ fewer police. The failure of early contributors to this literature to recognize or grapple with the simultaneity problem prompted Fisher & Nagin (1978), as part of a National Academy of Sciences report, to criticize the cross-sectional approach severely.[2]

A second generation of research emerged in the 1990s. Two methodological innovations characterize this generation of scholarship. First, researchers employ larger and richer data sets. Rather than investigating a single year of data from a set of jurisdictions, second-generation researchers typically examine multiple years of data from the set of jurisdictions. These so-called repeated cross-sectional data sets allow economists to track patterns in crime rates within a jurisdiction over time as well as across jurisdictions. Thus, both sources of variation in crime rates—across different jurisdictions and within a single jurisdiction over time—may help identify the impact of police on crime. Estimates from repeated cross sections enjoy greater precision owing to the larger number of observations. They also are less prone to bias from unobserved heterogeneity because repeated cross-sectional data permit the inclusion of year and jurisdiction fixed effects as control variables.

The second feature of this wave of literature was the explicit recognition of the simultaneity problem and the use of relatively sophisticated econometric techniques to overcome it. Economists have primarily used two techniques: Granger causality (Granger 1969) and instrumental variables or natural experiments. The first of these methods, Granger causality, refers to a temporal relationship between variables rather than actual causation. One variable "Granger causes" another when changes in the first variable generally precede changes in the second.[3] The first to apply the technique of Granger causality to the question of police and crime were Marvell & Moody (1996). These authors studied a repeated cross section first of states and then of cities that spanned 20 years. They also controlled for sundry demographic and economic characteristics of these jurisdictions. Their estimates revealed that increases in police preceded declines in crime. In other words, police "Granger cause" crime drops. Their estimates implied that a 10% increase in an urban police force produced a 3% long-term decline in total crime.

Marvell & Moody (1996) used annual observations of police and crime, but annual data may miss short-term fluctuations and produce an understatement of the actual relationship between police and crime. For example, an increase in

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[2] An early quasi-randomized experiment in Kansas City (Kelling et al. 1974) that also found little impact of police was not subject to these critiques, although it has been criticized by Larson (1976) for having inadequate power.

[3] A frequently cited example of the difference between actual causation and Granger causality is Christmas cards (Kennedy 1998). Christmas cards arrive in mailboxes before December 25, and thus they Granger cause Christmas. But the true causation is the reverse: the imminent arrival of Christmas causes individuals to mail Christmas cards. Forward-looking behavior produces a divergence between actual causation and Granger causation.

crime at the beginning of a year may later in the year prompt authorities to hire more police. With annual observations, any increase in crime and police appears contemporaneous rather than sequential, and the short-term causal effect is not observed. The resulting estimate of the impact of police on crime is biased toward zero in that circumstance. Higher frequency data, or data with a finer degree of temporal disaggregation, can remedy this problem. Corman & Mocan (2000) tested for the presence of these shorter-term fluctuations using nearly 30 years of monthly data from New York City. They found evidence that the governmental response to higher crime occurs rapidly. After an increase in crime, the hiring of more police occurs within six months. They also found evidence that offending rates similarly react quickly to greater policing. Employing the Granger causality method, Corman & Mocan (2000) estimated that a 10% increase in police resulted in a 10% drop in crime rates. These estimates suggest a considerably stronger effect of police on crime than the annual data indicated.[4]

Another econometric technique employed by contributors to the second generation of economic research on police and crime was instrumental variables or natural experiments. Instrumental variables help an econometrician break the simultaneity of policies and outcomes. Here, a valid instrument is a variable correlated with the criminal justice policy of interest but otherwise uncorrelated with crime rates. Unlike laboratory scientists, economists cannot assign subjects randomly to treatment and control groups. Instead, economists often look for naturally occurring phenomena that create nearly random exposure to particular public policies. They seek natural rather than laboratory experiments. In effect, economists employ such naturally occurring phenomena as instrumental variables.

Levitt (1997) proposed that the timing of mayoral and gubernatorial elections was a valid instrumental variable. He reported that police forces in major cities grew during election years, presumably because expanding the ranks of police forces provides an electoral advantage to incumbents. Thus, peculiar timing in the growth of police was plausibly exogenous to the incidence of crime because, after controlling for other factors, an election is not otherwise apt to influence the incentives for street crime. The elections were effectively a natural experiment that induced movement in the size of police forces but were otherwise exogenous to crime rates. Applying this methodology to a repeated cross section of cities covering more than 20 years, Levitt (1997) estimated that an increase of 10% in the police force led to a 3% to 10% reduction in crime rates. These estimates are very close to those of Marvell & Moody (1996).[5]

[4]Another dimension of disaggregation is geography. Di Tella & Schargrodsky (2004) pursued this approach and investigated a reallocation of police in Buenos Aires at the level of city blocks. When police were posted to temples and mosques in response to terrorist threats, automobile thefts on these blocks diminished. But beyond the immediate vicinity of these buildings, the incidence of automobile thefts was unchanged.

[5]Economists have continued to investigate the relationship of police and crime. McCrary (2002) disputed that electoral cycles induced sufficient variation in the size of police forces to measure the impact of crime. He argued that when properly measured, the imprecision of

In sum, the second generation of research by economists provides persuasive evidence on the behavioral question of whether police reduce crime. In view of this evidence, economists have also addressed the normative question of whether additional increases are worth it. Using estimates derived from his sample that terminated in 1992, Levitt (1997) claimed that the marginal benefit in reduced crime from employing another city police officer exceeded the costs. Marvell & Moody (1996) noted a similar calculation, but believing that additional costs (such as the attendant court and prison expenditures) could not be quantified, they declined to conduct a full cost-benefit analysis. Despite this hesitation, the estimates from this research indicate that the marginal benefit in reduced crime associated with hiring an additional police officer in large urban environments likely exceeds or equals the marginal cost.

Measuring the Impact of the Scale of Imprisonment on Crime

Research on the effect of the scale of incarceration on crime followed a similar path as research on the impact of policing. The first generation of scholarship gave insufficient attention to the issue of causal direction and drew normative conclusions on the basis of relatively limited data sets, such as single cross sections or national time series. In the 1990s, a second generation of research, conducted by economists, emerged. As with research on policing, the next wave of incarceration scholarship examined larger and richer data sets and applied econometric techniques that attempted to isolate causal direction. With respect to data, repeated cross sections again became standard, and with respect to techniques, Granger causality and instrumental variables again furnished estimates that more persuasively reflected causal effects.

Marvell & Moody (1994) applied the technique of Granger causality to almost 20 years of state-level data on incarceration and crime rates. They found that increases in incarceration precipitated drops in crime. Their estimates indicated that a 10% increase in prison populations led to a 1.5% reduction in crime rates. Evidence from instrumental variables or natural experiments also suggested that the scale of incarceration had a crime-reducing effect. Levitt (1996) argued that lawsuits challenging overcrowded conditions in state prisons constituted a valid instrumental variable. He demonstrated that when these suits resulted in injunctions to alleviate overcrowding, state authorities complied by releasing prisoners or by declining to incarcerate convicts who otherwise would have been imprisoned. The suits were plausibly exogenous to crime rates because, after controlling for other factors, the suit would not affect the incentive to commit crime, except through the channel of prison populations. In other words, prison litigation was arguably

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the instrument precluded inferences about the causal effect of police on crime. In response, Levitt (2002) showed that even with an alternative instrumental variable, the number of firefighters, estimates of the impact of police on crime are negative and sizable. Also, W. Evans and E. Owens (unpublished manuscript) found estimates similar to those of Marvell & Moody (1996) and Levitt (1997, 2002) by examining the impact of recent federal subsidies on the hiring of local police.

a natural experiment that produced nearly random variation in incarceration rates in certain states but left other states unaffected. Applying this technique to over 20 years of annual, state-level data, Levitt (1996) found that increases in crime occurred simultaneously with litigation-induced reductions in state prison populations. His estimates implied that the reduction in crime from incarcerating an additional prisoner was two to three times larger than that predicted by Marvell & Moody (1994). According to Levitt's (1996) analysis, the release of an additional prisoner produced an additional 15 crimes annually.

Another test of the responsiveness of offending rates to the magnitude of penalties was Levitt's (1998) study of the severity of the transition from the juvenile to the adult criminal justice system. The estimation of the causal impact of this transition is complicated by the fact that crime propensities rise and fall with age and by the difficulty in making absolute comparisons between expected punishments in juvenile and adult systems. Levitt (1998), however, exploited the fact that in certain states the criminal justice system was substantially more punitive than the juvenile system, whereas in other states this was less true. States where the transition to the adult system was severe did not appear correlated with other factors influencing crime, and thus the pattern of transitions approximated a natural experiment in the severity of punishments. Applying a difference-in-differences methodology, Levitt (1998) observed that in states where the severity of the adult system is greatest relative to the juvenile system, both violent and property crime fell almost 30% at the age of majority relative to the states where the transition to adulthood engenders the smallest increases in punishment.

Incarceration is an expensive penalty. Although the estimates are already several years old, the most recent comprehensive estimates of the annual cost of incarcerating an offender range from $25,000 (DiIulio & Piehl 1991) to $35,000 (Donohue & Siegelman 1998). Economists who estimated the number of crimes reduced by incarcerating the marginal prisoner used their forecasts, together with these cost figures and estimates of the monetary loss associated with criminal offenses, to conduct cost-benefit analyses of incarceration. In the early to mid-1990s, when the second generation of economic studies of incarceration appeared, a frequent conclusion was that the benefits of incarceration outweighed the costs. However, in the intervening decade, prison populations have nearly doubled again. Economists' now decade-old estimates of the number of crimes reduced by the marginal offender have limited application in conducting a current cost-benefit analysis. If the two-millionth prisoner incarcerated is less dangerous than the one-millionth prisoner, the net social benefits of incarceration today are likely less than those of a decade ago. For the most reasonable set of assumptions, the current scale of incarceration is apparently at or above the socially optimal level.

Measuring the Impact of Capital Punishment on Crime

Academics, policy makers, and the general public have long debated capital punishment. Empirical economists have also contributed to the debate by attempting to determine whether capital punishment reduces crime. Economic research on this topic divides again into two generations. As with the other research questions discussed above, the first generation of economic research on capital punishment consists of studies of national time series and state cross sections, and the second generation examines repeated cross sections. Unique to the study of capital punishment is that a dramatic and brief policy change, the Supreme Court's moratorium on the application of the death penalty, separates the two generations.[6] In addition, economic research on the deterrent effect of the death penalty has not produced a consensus.

The most prominent national time-series study that claimed to find evidence of the death penalty's deterrent effect was Ehrlich (1975). He claimed that over the period 1932–1970, each execution prevented between one and eight homicides, as well as numerous robberies, assaults, and property crimes. Other researchers disputed Ehrlich's claim, and Cameron (1994) reviews the large body of work on the topic.[7] Perhaps the most troubling criticism was that if the sample period is extended to include later dates or truncated to exclude the late 1960s, no effect on crime rates is found (Passell & Taylor 1977, Klein et al. 1978, Leamer 1983).

Studies of cross-sectional data fared no better in persuading researchers. Ehrlich (1977) examined two cross sections of states and reported estimates similar to those of his earlier time-series study. Cameron (1994) reviews the criticisms of this work as well. Perhaps most important is that, as seen in the research on policing and incarceration, cross-sectional estimates may not reliably identify a causal effect because a particular state's criminal justice policies, such as capital punishment, may themselves be functions of a state's crime rate. Moreover, the concentration of executions in southern and western states strongly suggests that the use of the death penalty is nonrandom. In addition, researchers who explored other tests of the hypothesis that the death penalty deters found contrary evidence. Bailey (1982), for example, reports no effect of the death penalty on the frequency with which police officers are killed. Because killers of police officers are much more likely to face capital punishment than murderers of civilians, the absence of an effect there is troubling.

The second generation of economic research on capital punishment began within the past five years, as data on executions in the postmoratorium period

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[6]In *Furman v. Georgia* (1972), the Supreme Court held that Georgia's death penalty statute was constitutionally deficient because it created a substantial risk that it would be imposed in an arbitrary and capricious manner. In *Gregg v. Georgia* (1976), the Court held that the death penalty does not always constitute cruel and unusual punishment and that the revisions to the Georgia statute remedied its constitutional deficiencies.

[7]Even the Supreme Court, in *Gregg v. Georgia* (1976), which held that capital punishment could comport with the Eighth Amendment, commented on the debate. In reference to the estimates on the deterrent effect, the Court observed, "[s]ubstantial attempts to evaluate the worth of the death penalty as a deterrent to crimes by potential offenders have occasioned a great deal of debate. The results have simply been inconclusive" (*Gregg v. Georgia* at 184–85, footnotes omitted).

became available. All these studies employ repeated cross-sectional data, but the range of estimates they present is even wider than that of the earlier studies. Mocan & Gittings (2003) investigated the effect of commutations, as well as of executions, in a monthly, state-level panel spanning 1977–1997. They reported that the marginal execution spared five homicides, and each commutation caused five homicides. Contrary to Ehrlich's (1975) findings, their estimates indicated that executions and commutations had no effect on other categories of crime, such as assaults, robberies, and property offenses. Dezhbakhsh et al. (2003) applied a simultaneous equations methodology to annual, county-level data and reported that, with a margin of error of 10, each execution prevents an astonishing 18 homicides. The lower end of this estimated impact, 8 homicides, matches the highest of Ehrlich's (1975) estimates. Shepherd (2004) used a monthly, state-level panel covering 1977–1999 to estimate the effect of the death penalty on different types of homicides. Her estimates indicated that each execution deterred approximately three murders, including so-called crimes of passion. More recently, Shepherd (2005) reported that the impact of the death penalty varies by state, reducing homicides in some states and increasing it in others. Rather than attribute these differences to the weakness of the empirical test, she hypothesizes that capital punishment deters in some states and brutalizes or fosters brutal violence in others.

In contrast to these authors, Katz et al. (2003) studied state-level panel data covering the 1950–1990 period and detected no effect of the death penalty on crime rates. Katz et al. (2003) found instead that a proxy for prison conditions, the death rate among prisoners, correlated negatively with crime rates. Donohue & Wolfers (2006) present the most exhaustive review of the recent empirical literature on capital punishment and are highly critical of the existing studies. Their reanalysis of recent studies finding a deterrent effect of the death penalty exposes a range of empirical weaknesses and demonstrates the sensitivity of these results to minor changes in specification. Donohue & Wolfers (2006) conclude that there is no compelling evidence of a deterrent effect of executions.

The Donohue & Wolfers conclusion is not particularly surprising given the relatively modest use of the death penalty. In 1999, 98 prisoners were executed, more than any other year since the Supreme Court's 1976 reinstatement of capital punishment. However, relative to the 3540 prisoners under a sentence of death in 1999, the number of executions constituted 3% of death row. Since then, the number of death row inmates has steadily grown while the pace of executions has slowed. For example, the 59 executions in 2004 implied an execution rate of 1.8%. The execution rates on death row are only slightly larger than the risk of accidental and violent death for African American males aged 15 to 34. The new death sentences as a percentage of the homicides in a particular year, a rough proxy for the risk of receiving a death sentence conditional on having committed a homicide, is also low—hovering between 0.8% and 1.8% in recent years. Note also that most criminals sentenced to death will never actually be executed, so this number dramatically overstates the true risk of execution. For a person regularly participating in crime, the risk of violent death is surely higher. Kennedy et al.

(1996) estimate that annual violent death rates among gang members in Boston are between 1% and 2%. Levitt & Venkatesh (2000) find that the death rate among street-level drug dealers in an urban gang was 7%. Individuals who readily bear such risks as part of their criminal activity are unlikely to be influenced by the relatively low risk of capital punishment. Ironically, to the extent the death penalty has any deterrent effect on homicide, it is unlikely to be the rational criminal who is deterred but rather an irrational criminal who mistakenly overstates the expected punishment associated with the death penalty.

Racial Profiling

In addition to testing the economic model of deterrence and assessing the costs and benefits of particular crime-control policies, economists have studied the operation of the criminal justice system itself from a rational choice perspective [see Levitt & Miles (2006) for a review of that literature]. An aspect of the operation of the criminal justice system that has received considerable attention from the public and academics is racial profiling, the practice by police of using the race of the driver as a criterion in choosing whether to stop and search automobiles. Knowles et al. (2001) developed an empirical test of whether racial prejudice or the goal of maximizing the number of successful searches explains the racial disparity in searches. Their test distinguishes two economic accounts of discrimination. Becker's (1957) model describes racial prejudice as a preference or taste for disfavoring members of a particular race. In contrast, Arrow's (1973) model describes actors who statistically discriminate by using race to predict outcomes and then rely on these predictions in their decision making. Arrow's information-based model differs from Becker's in that its actors are not necessarily racially prejudiced. As applied to stops and searches, the Arrowian framework presents police as seeking to maximize the number of searches in which contraband is found. If being black correlates with the likelihood of possessing contraband, police, even if they are not racially prejudiced, in an Arrowian equilibrium will search blacks more often [see Persico (2002) for a model of police searches that includes a notion of fairness and a stigma of search].

Knowles et al. (2001) compare the probabilities that searches of members of each racial group yield contraband. If police are not racially prejudiced and merely maximize the number of successful searches, the rates at which searched persons are found carrying contraband should be equal across racial groups. Knowles et al. (2001) tested their hypothesis using a data set obtained from the ACLU containing all vehicle searches along an interstate highway in Maryland over a four-year period. The authors could not reject equality across racial groups in the rates at which police found contraband during searches. They also could not reject equality in the rates of successful searches across various other dimensions, such as gender, age of the vehicle, whether the vehicle is a luxury model, and whether the search occurred during the day or at night. The estimates of Knowles et al. (2001) suggest that racial disparities in vehicle searches result from a desire to maximize successful searches rather than from racial prejudice.

Several economists have criticized the Knowles et al. (2001) finding. Dharmapala & Ross (2003) argued that in a more generalized model, including varying offense levels and imperfect observation of offenders, the appropriate test requires stratifying the data by offense severity. Using the Knowles et al. (2001) data, Dharmapala & Ross (2003) found that blacks have higher guilty rates for felonies and whites have higher guilty rates for misdemeanors. Antonovics & Knight (2004) also argue that the ability of the Knowles et al. (2001) model to distinguish preference-based discrimination from statistical discrimination is not robust to generalization. Antonovics & Knight (2004) instead predicted that, if officers engaged only in statistical discrimination, search decisions should be independent of an officer's race (assuming police are randomly assigned geographically by race). Using data from the Boston Police Department, they found that they were not. A consensus about the explanation for racial disparities in search rates has not emerged among economists, and the questions of race and policing are likely to remain an active and contentious area of research.

All the papers described in this section focus on the issue of whether or not the police unfairly target minorities. Heaton (2006), in contrast, investigates the impact that racial profiling has on criminal activities. In particular, Heaton exploits the fact that highly publicized cases in both New Jersey and Maryland led to wholesale changes in police practices, resulting in sharp reductions in the relative likelihood that black motorists would be stopped and searched in these states. Heaton finds evidence that, in response to these alterations in police strategy, motor vehicle thefts by blacks rise relative to thefts by whites, although he does not find evidence that other dimensions (motor vehicle crashes, failure to properly restrain children) of black driving behavior changes. And, as would be expected, changes in vehicle searches have no discernible impact on nonvehicle-related crimes such as burglary.

MEASURING THE IMPACT OF FACTORS OTHER THAN THE CRIMINAL JUSTICE SYSTEM ON CRIME

The incidence of criminal activity is a product of many factors, of which the criminal justice system is only one, and economists have also studied many of these other factors. Although a complete review is well beyond the scope of this paper, this section discusses two such factors: concealed weapons laws and abortion legalization. We focus on these topics because they have received considerable attention recently and because they involve laws, albeit not criminal laws.

Measuring the Impact of Concealed Weapons Laws on Crime

In the late 1990s, the debate over gun policy partly shifted away from measures designed to limit the availability of guns to laws that expand the availability of guns, especially concealed guns. This change resulted from a series of studies (Lott & Mustard 1996, Lott 1998, Lott & Landes 1999) that claimed that a greater availability of concealed weapons reduced, rather than increased, crime. Their hypothesis was that a potential victim's surreptitious gun possession has a general deterrent effect because it was an unobservable precaution that raised the risk of criminal activity. Lott and his coauthors employed the difference-in-differences methodology. They compared county-level crime rates before and after a state's passage of concealed weapons laws, relative to other states without these laws. They asserted that once a state enacted a concealed weapons law, the rates of certain types of crimes decline sharply.

At the time of the Lott studies, the existing literature on guns and crime focused on gun control policies. Much of this earlier research by economists casts doubt on the efficacy of these policies and hypothesized that they were unsuccessful because the vast secondary markets in guns implied that gun control measures did not effectively constrain the availability of guns [Cook et al. (2002) provide a thorough review of that literature]. Despite this evidence, the Lott hypothesis assumed the opposite: that concealed weapons laws could reduce crime because they could increase the availability of guns.

A large number of researchers challenged the Lott findings by criticizing the empirical weakness of the estimates and by reporting that additional tests of the unobservable precaution hypothesis yielded contrary evidence. First, Duggan (2000) showed that the basic Lott & Mustard (1996) estimates lack statistical significance when the assumption of the statistical independence of counties within the same state is relaxed. Second, the Lott estimates also appeared to suffer from omitted variable bias. Duggan (2000) demonstrated that crime rates in states that adopted concealed weapons laws began to decline before the passage of the laws. Donohue & Levitt (2001) reported that after controlling for abortion rates, the laws do not correlate with crime rates. Third, Ayres & Donohue (1999) extended the original Lott sample to include more recent enactments of concealed weapons laws, and in this larger sample they found no correlation between the law and crime rates. Fourth, other researchers conducted further tests of the behavioral implications of the concealed weapons hypothesis. For example, a victim's potential possession of a concealed weapon should have the greatest deterrent effect on crimes involving the criminal's direct confrontation of a stranger victim. Street robbery is the prime example of such a crime. However, the original Lott & Mustard (1996) estimates featured large effects on homicide and rape and a much smaller effect on robbery. But, in Ayres & Donohue's extended sample, the concealed weapons laws correlated positively with robbery. Moreover, Ludwig (1998) predicted that the laws should induce criminals to substitute victims who cannot take the precaution. Because the laws do not authorize possession of concealed weapons by juveniles, the laws should prompt criminals to seek younger victims. Ludwig (1998) found evidence to the contrary. He reported that passage of these laws is associated with larger declines in juvenile victimizations than adult victimizations.

Measuring the Impact of Legalized Abortion on Crime

The overwhelming majority of research by economists on crime has focused on the near-term impact of criminal justice and public policy interventions on crime. Donohue & Levitt (2001) presented evidence suggesting that a much earlier change in policy—the legalization of abortion in the 1970s—may also have played an important role in influencing the unprecedented declines in American crime in the 1990s. The theory underlying an abortion-crime link is straightforward. A substantial body of social scientific research suggests that growing up unwanted puts a child at risk for adverse life outcomes, including criminal involvement (Dagg 1991). If the legalization of abortion led to the birth of fewer unwanted children, then two decades later when those cohorts reached peak crime ages, there would be fewer individuals at risk for crime.[8]

Donohue & Levitt (2001) put forth six different types of empirical evidence in support of this conjecture: (*a*) a calibration exercise based on pre-existing estimates of the impact of abortion and the distribution of women seeking abortions; (*b*) the national time-series data; (*c*) a comparison of the crime paths of early legalizing states and those in which abortion only became legal with *Roe v. Wade* (1973); (*d*) differences in crime patterns in states with high and low abortion rates after abortion became legal nationwide; (*e*) isolation of the reductions in crime in high-abortion states to those born after abortion legalization; and (*f*) a correlation between arrest patterns by single year of age and abortion exposure. The findings of each of these analyses were consistent with the proposed theory. Donohue & Levitt estimated that legalized abortion lowered crime by 10%–15% in the 1990s relative to a counterfactual in which abortion had remained illegal.

These findings have generated substantial controversy, inside and outside of academia. A number of papers have confirmed and extended the initial findings. A. Sen (unpublished paper) found a similar relationship between abortion and crime in Canadian data, as did Leigh & Wolfers (2000) using Australian data. Pop-Eleches (2006) demonstrated that a ban on abortion in Romania led to increased criminal activity in that cohort, consistent with the theory. Charles & Stephens (2006) analyzed the Monitoring the Future survey data and uncovered evidence that exposure to legalized abortion was associated with lower rates of illicit drug use as a teenager. Sorenson et al. (2002) argued that legalized abortion led to decreases in homicides of young children.

Joyce (2003) is the only published study to date that directly challenges the link between abortion and crime. Joyce (2003) contended that legalized abortions largely replaced illegal abortions such that one would not expect to see an impact on crime rates. He argued that the national time-series fluctuations in arrests by age were not supportive of the abortion hypothesis, and he showed that when the sample

[8] In addition to this change in the composition of the cohort, it is also the case that the absolute number of children fell by approximately 5% after legalization (Levine et al. 1999), which will further reduce crime in a mechanical way.

was restricted to the 1985–1990 time period, the abortion-crime link disappeared. Donohue & Levitt (2003) responded to Joyce's claims and presented evidence that legalization indeed increased the number of abortions. They noted that the other two results in Joyce (2003) were likely an artifact of the crack epidemic, which hit just as the first cohort exposed to legalized abortion reached peak crime age. Joyce estimated a difference-in-differences approach that compared those born before and after legalization in states that did and did not legalize abortion before *Roe v. Wade* (1973). Joyce reported results using only crime rates for 1985 and 1990. When Donohue & Levitt carried out the same exercise on the same birth cohorts but expanded the sample to include data from all available years, the results were consistent with their initial claims; Joyce's contrary findings appear to be a function of his limiting the sample to the peak crime years.

In a recent and widely publicized working paper, Foote & Goetz (2005) challenged one of the six pieces of evidence in Donohue & Levitt (2001), namely the analysis done on arrests by single year of age. Foote & Goetz correctly noted that the final table in the original Donohue & Levitt paper was mislabeled—the text implies that the analysis included controls for state-year interactions, when none were included. Foote & Goetz proceeded to show that when examining arrests by single year of age, a specification that combined three features—the inclusion of state-year interactions as controls, the use of log of per capita arrests (rather than the log of the number of arrests), and the extension of the data set beyond the original years covered in Donohue & Levitt (2001)—eliminated any effect of legalized abortion. Donohue & Levitt (2006) responded by arguing that the combination of poorly measured abortion rates and the high demands on the data associated with the inclusion of state-year interactions removed virtually all the signal from the abortion rates. They demonstrated that even when the preferred specification of Foote & Goetz (2005) is employed, three adjustments—the use of more precise abortion data (which measures abortions by the state of residence of the women, as opposed to the state in which the procedure is performed), the correction for cross-state mobility between birth and adolescence, and more careful matches of the ages in the arrest data to abortion exposure—led to estimates as large as or larger than those reported in the original Donohue & Levitt (2001) paper.

CONCLUSION

The past decade has seen a renaissance in the application of empirical economic approaches to issues of crime. Substantial progress has been made in understanding the impact of elements of the criminal justice system such as police, prisons, and the death penalty. In addition, new hypotheses related to racial profiling, concealed weapons, and legalized abortion have emerged as contentious and widely publicized debates. In some cases, such as the impact of adding more police, a consensus has developed among economists. For most other questions, substantial uncertainty remains, ensuring that the subject will be a fertile area of study for the foreseeable future.

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