
6 Police, prisons, and punishment: the empirical evidence on crime deterrence¹

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INTRODUCTION

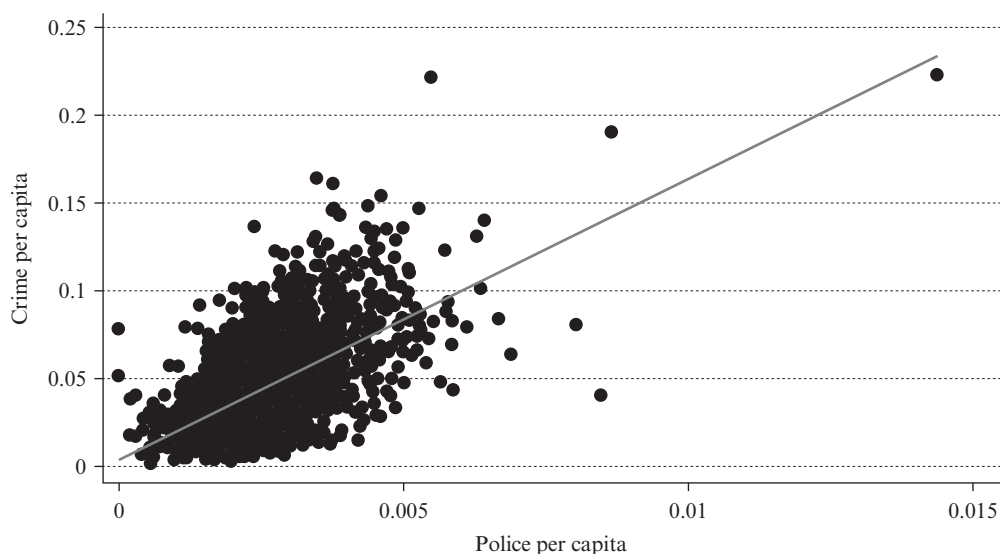
Spending on the criminal justice system in the USA is enormous. In 2007, spending on police and prisons amounted to more than \$175 billion. In terms of local public finance, expenditures in these areas trailed only education and healthcare, according to estimates by the US Census Bureau. Additionally, the direct and indirect costs of crime may amount to a total annual burden that exceeds \$1 trillion (Anderson, 1999). Considering the recent decline in state and local revenue, understanding the determinants of crime and quantifying their effects are important tasks for empirical researchers simply because the public policy implications are huge.

Over the last two decades, economists, using the tools of modern microeconometrics, have provided a number of insights into the factors that affect crime rates. Although there have certainly been a number of missteps over this period, innovative econometric identification strategies, combined with a host of new data sets, have provided a fresh understanding of what factors are important in explaining variation in crime both cross-sectionally and over time.

In this chapter, we review the best recent attempts to evaluate the deterrence effects of police and prisons on crime. Although work on the effects of capital punishment and gun laws has received substantial, perhaps disproportionate, attention in crime discussions in both the academic community and general public, researchers have more consistently shown that increases in police and prisons reduce crime. In fact, results from many studies suggest that spending in these areas may be too low, with increases in the size of local police forces and increased reliance on imprisonment representing investments with positive returns.

THE EFFECT OF POLICE ON CRIME

In the Beckerian model of crime (Becker, 1968), policy-makers can affect crime rates by increasing either the likelihood of punishment or the cost of the punishment. The primary avenue through which the USA increases the probability of punishment is through raising the level of police protection. If there are more police patrolling the street, the likelihood that the crime will be observed by a police officer increases. Further, if the supply of police goes up, the cost of victims and third parties reporting the crime goes down, which will also increase the likelihood of arrest. Lastly, more police should lead to more detective work, which improves the chances of apprehension and subsequent prosecution.



Note: Police and crime per capita from 2494 US towns and cities with population greater than 10000.

Source: FBI Uniform Crime Reports, 1999.

Figure 6.1 Do police cause crime?

Identifying the effect of police on crime statistically, however, is surprisingly difficult. Figure 6.1 graphs crime per capita against police per capita from a large sample of towns and cities with populations greater than 10000. The positive relationship between police and crime is dramatic. Should we conclude that police cause crime? Probably not. It is more likely that crime causes police. That is, greater crime rates lead to more hiring of police. We thus have two chains of potential causality – more police reduce crime and more crime increases police. Since there are two chains of potential causality, the correlation between police and crime cannot be interpreted as an indication of either causal relationship. How then are we to disentangle these chains?²

We could look at a single jurisdiction, focusing on the relationship between changes in the size of the police force and changes in crime over time. But even in this case it is likely that changes in the police force size will not be independent of the crime rate or the expected crime rate, again leading to biased estimation.

In fact, until recently, economists and criminologists were not able to address these problems in a convincing manner. As a result, it is not surprising that a survey of the literature prior to the 1990s found that a majority of the studies reported that increases in the number of police are associated with either no change or an increase in crime rates (Cameron, 1988). Indeed, one criminologist went so far as to argue that ‘The police do not prevent crime. This is one of the best kept secrets of modern life. Experts know it, the police know it, but the public does not know it’ (Bayley, 1994, p. 3). Since that time, however, a large body of research using improved research designs has established a substantial effect of police on crime.

An ideal research design could solve the causality problem by randomly increasing (or reducing) the police force across a large sample of jurisdictions. Thus, imagine taking 1000 roughly similar cities and randomly flipping a coin dividing the cities into two groups. In the first group of cities, double the police force; in the second group, do nothing. If the cities with an increase in police have lower rates of crime over the next few years then (with a large enough sample), we can confidently ascribe this difference to the effect of police on crime. What confuses the correlation evidence in Figure 6.1 is that increases in crime sometimes cause increases in police. If we increase police randomly, however, then we remove the possibility of 'reverse causality'. So if crime falls in the cities that have random increases in police, the cause is most plausibly the increase in police.

Unfortunately, randomized experiments of this kind are very expensive and for political or other reasons they are often not feasible. Although criminologists have pushed for more randomized experiments, their use is still limited (Petrosino et al., 2003a). Further, no randomized experiment involving police levels exists in the literature.

Thus, to solve the causality problem, economists have looked for 'natural' or 'quasi-experiments' in which the size of the police force was varied for reasons unrelated to crime. In 1969, for example, police in Montreal, Canada went on strike and during that time there were 50 times more bank robberies than normal.³ If the strike was a random event with respect to crime, i.e. not tied in any direct way to increases or decreases in the crime rate, then we can be reasonably certain that the increase in bank robberies was caused by the decrease in police.

The Montreal experiment tells us that it is probably not a good idea to eliminate all police, but it does not tell us whether we should increase or reduce police on the street by a more reasonable amount, say 10 percent to 20 percent. Other studies have looked at more common reasons why the size of the police force might vary quasi-randomly. Perhaps the most famous of these papers is Levitt (1997). The central insight of Levitt's analysis is that increases in the size of police forces tend to be concentrated in mayoral or gubernatorial election years. The intuition is that politicians want low crime rates in election years (or at least they want to be seen as tough on crime) because this increases the likelihood of re-election. Because the timing of elections is generally predetermined by factors wholly unrelated to crime rates, Levitt hypothesizes that these election-induced increases represent a kind of random positive shock to police force size that can be used to isolate the causal effect of police on crime.

Using panel data for 59 large US cities for the period 1970–92, Levitt reports that this identification strategy yields elasticities between crime rates and the number of police officers around -1.0 for violent crime and -0.3 for property crime.⁴ The magnitude of the effects estimated by this 'quasi-experiment' are larger by a factor of approximately 5 than the effects estimated through a standard ordinary least squares (OLS) regression (i.e. a statistical procedure that does not exploit election timing as a source of randomization). Thus Levitt's results can explain why earlier studies – studies that did not exploit a source of randomization – found low or even positive effects of police on crime (e.g. Cameron, 1988).

Levitt's identification strategy was ingenious and his paper has had an enormous influence on the field of crime research.⁵ Unfortunately, the specific results must be discounted. McCrary (2002) presents evidence that Levitt's results suffer from one major problem and a more minor problem. As for the former, because Levitt estimated the

effect of police on crime rates in different crime categories jointly, he intended to weight each crime category's contribution to the regression estimation inversely to the year-to-year variability observed within the crime category. However, due to a computer programming error, Levitt actually weighted observations from high-variability categories more heavily; that is, he did the exact opposite of what he aimed to do. McCrary shows that Levitt's mistake gave undue weight to the murder observations, and when this is remedied, none of the estimated coefficients for the effect of police on crime is statistically significant. Further, the difference between these estimates and the OLS estimates shrinks, and McCrary observes that the sign of the coefficient in many instances actually flips.⁶

McCrary (2002, p. 1242) concluded his paper by stating, 'In the absence of stronger research designs, or perhaps heroic data collection, a precise estimate of the causal effect of police on crime will remain at large.'

Levitt (2002) proposed another novel strategy to isolate the causal effect of police on crime, using the number of firefighters per capita as an instrument to isolate the portion of changes in police levels that is uninfluenced by crime.⁷ Levitt's new results suggest that the elasticity of violent crime rates with respect to police levels is about half as large as previously estimated (-0.44) and is only marginally statistically significant. The property crime elasticity actually increases in size to (-0.50), is statistically significant, and exhibits less sensitivity to specification changes than do the violent crime estimates.

Taking a different approach, Di Tella and Schargrodsy (2004) exploit the increase in police presence induced by a terrorist bombing of a Jewish center that occurred in Argentina in 1994. After the attack, federal authorities deployed police protection to every Jewish and Muslim building in the country. The researchers collected data on the number of motor vehicle thefts in Buenos Aires for the 3.5 months before the attack and the 5.5 months after the attack. Because the location of the protected buildings is presumably unrelated to changes in crime levels, this setting provides a potentially powerful natural experiment to examine the effect of police on crime. They find that car thefts decline (relative to pre-bombing levels and relative to areas not receiving additional protection) by about 75 percent when the extra police are deployed. This effect is statistically significant and corresponds to an elasticity of -0.33 . However, they find that the effect is very localized, with the statistically significant decline present only in the city blocks with the extra protection. In areas that are one block removed from the additional police presence, the estimated effect is about one-fifth as large and it is not statistically significant.

While certainly creative, the Di Tella and Schargrodsy (2004) design suffers from a few problems. First, there is the general concern of inference in cases where there is a single shock, raised in Abadie and Gardeazabal (2003) and Abadie et al. (2007). A related concern is raised by Bertrand et al. (2004). The concern here is that a single shock does not generally allow a researcher to use critical values from standard asymptotic theory to judge the statistical significance of observing a given test statistic. Further, Donohue and Ho (2005) suggest that the primary effect of the police deployment studied by Di Tella and Schargrodsy was to displace crime, as they found that auto thefts in blocks far removed from police protection actually increased more than the decline observed in the blocks with greater protection. The greater increase might imply that the redeployment created a suboptimal allocation of police resources.

Using a similar intuition to motivate their design, Klick and Tabarrok (2005) exploit

variation in the terror alert system used by the Department of Homeland Security as a random shock to the size of the police force in Washington, DC. Specifically, when the terror alert level rises from 'elevated' (yellow) to 'high' (orange) due to intelligence reports regarding the current threat posed by terrorist organizations, the Washington DC Metropolitan Police Department reacts by increasing the number of hours each officer must work. Because the change in the terror alert system is unrelated to any observed or expected changes in DC crime patterns, this provides a useful quasi-experiment wherein the effective police presence is randomly increased.

Klick and Tabarrok show that the change in the terror alert level is associated with a decline in daily DC crime on the order of about 7 percent, and this result is statistically significant. Exploiting the fact that most of the increased presence is focused around the National Mall, White House and Congress areas (District 1), they estimate an elasticity between crime and the number of police officers of about -0.3 . To rule out the possibility that this decline comes about due to a decrease in the number of tourists in Washington, perhaps due to fear generated by the change in the terror alert, they control for mid-day subway ridership (a proxy for tourism volume) and find the results largely unaffected. Klick and Tabarrok show that the bulk of the effect they identify comes from reductions in opportunistic crimes – stolen automobiles,⁸ thefts from automobiles, and burglaries⁹ – with little change occurring in violent crimes.

This aspect of their results is notable, especially in contrast with Levitt's original estimates that found the largest deterrence effect for homicides. Although Klick and Tabarrok do not report results from the specific crimes within the violent crime category, they find no deterrence effect for homicide in unreported results. Given that most murders occur in non-public places, it seems questionable that police would have their strongest marginal effect on homicide rates, as suggested by Levitt and a number of earlier studies. On the other hand, it is more difficult to estimate the effect of police on violent crimes, especially homicides, when these are significantly rarer than property crimes.

In distinction to the Donohue and Ho critique of Di Tella and Schargrodsky (2004), Klick and Tabarrok find little evidence of crime displacement, as daily crime does not appear to increase in the other districts of Washington when the terror alert level goes up. While they cannot rule out the possibility of displacement to Prince George's County in Maryland or across the Potomac River in Virginia, it would seem that the most direct displacement effect is ruled out. Further, because Klick and Tabarrok's design exploits four shocks, their results may be more representative than those observed in Di Tella and Schargrodsky (2004)'s single shock application.¹⁰

A significant shortcoming of both the Di Tella and Schargrodsky (2004) and Klick and Tabarrok (2005) research is its reliance on a single jurisdiction. While the strong designs require this and provide a better identification strategy than had been available in the literature previously, external validity is a concern. For example, it is unclear whether the effect observed in Washington DC could be expected in New York City if a similar increase in police staffing were observed. Because the NYPD is generally viewed as a better police force than the DC Metropolitan Police Department, perhaps we might expect to see a larger crime reduction in New York. In particular, none of these studies examines how police are allocated to reduce crime, which would presumably have a large effect on the results. In some ways, the general consensus in the point estimates across these studies and others (for property crime at least) mitigates this concern. However, use

of this design in other cities and more detailed information on police allocation has the potential to increase confidence in these results substantially.¹¹

Evans and Owens (2007) use a different approach to isolate the effect of police on crime. They use the funding shock provided by the Community Oriented Policing Services (COPS) program, established under the Violent Crime Control and Law Enforcement Act of 1994, to instrument for the size of the police force in 2074 cities. They then use this instrumental variables approach to examine annual crime rates in those cities for the period 1990–2001. This approach also yields statistically significant crime reductions related to increases in the police force for burglaries and auto thefts, while the evidence for a deterrence effect with respect to violent crime is much more limited.

One concern with the Evans and Owens approach is that application for and approval of COPS grants was not orthogonal to underlying crime expectations. That is, perhaps only very forward-looking police departments, which may be higher-quality police departments along unobservable dimensions, sought or were awarded the federal money, leading to a spurious negative relationship between the employment of additional police and crime. To counter this concern, Evans and Owens examine a number of specifications that attempt to control for this possibility. In their most demanding specifications they include police-department-specific time trends, generally finding comparable results. Interestingly, in these specifications that control for heterogeneity in background crime trends, they find that the effect of police is most pronounced in property crimes, especially the theft of automobiles, whereas the effects on murder and rape are quite small and not statistically significant. This again accords with the intuition that police are most effective in deterring opportunistic street crimes, as opposed to those crimes that generally occur ‘behind closed doors’ in contrast with some of the earlier, less plausible estimates.

While most econometric studies of the relationship between police and crime focus on changes in the level of police staffing, Mas (2006) examines the relationship between policing effort and crime. Specifically, he examines what happens in New Jersey during periods after a police force’s wages are subject to arbitration. The data set includes almost 400 arbitrations. The implicit story is that when the police win their arbitration (i.e. get paid at or above their wage request), they will be relatively more diligent, whereas they will shirk in periods after they lose in arbitration. Mas shows that clearances increase after an arbitration win and decline after a loss.¹²

Mas also provides evidence suggesting that the likelihood of a criminal being incarcerated and the criminal’s sentence length are both affected by this arbitration-induced change in diligence. Namely, when the police win their arbitration, the number of clearances increases (as does the likelihood of incarceration). This translates into a relative increase in crime, in both the property and violent crime categories, when the police are acting less diligently. Mas estimates an elasticity of crime with respect to clearances of -0.3 , although the results are not precisely estimated.¹³ Mas’s work is especially interesting because it suggests that the deterrence effect of police does not entirely come about simply by having more bodies on the street. The quality of the work performed by those officers appears to matter. Additionally, the Mas results contribute to the finding that homicide and rape seem to be less affected by policing than other more opportunistic crimes. Although one can question whether Mas really identifies the effect of diligence since there is no attempt to control for the possibility that arbitration decisions are

influenced by underlying crime trends and expectations, the work is notable in its unique focus. Further, Mas does find that the effects grow with the magnitude of the arbitration loss (i.e. how much lower the police were paid relative to the union's request), providing some additional confidence in the causal interpretation.

ELASTICITY OF CRIME WITH RESPECT TO POLICE: REVIEW AND DISCUSSION

Table 6.1 reviews the estimates of the elasticity of crime with respect to police. A few regularities are worth discussing. First, note that the estimates for property crimes are similar across papers and within papers. These are typically the more precisely estimated elasticities – this is a good sign that these estimates are identifying something of value. The estimates for violent crime are more widely dispersed across papers, which is also as expected given that these estimates are typically less precisely estimated within each paper. Since violent crime is less common than property crime, it is not surprising that the elasticities of police on violent crime are more difficult to estimate precisely.

The estimates across violent crime are also less different than they might appear. Klick and Tabarrok (2005) estimated how daily crime rates responded to temporary shifts in the number of officers on the street. Given that violent crime often occurs off the street and is highly symptomatic of long-term problems such as gangs and drug violence, their results are plausible. Violent crime may respond more in the long run, however, to a greater number of detectives, higher clearance rates, more gang units and so forth. Thus the higher numbers suggested by Evans and Owens (2007) and Levitt (2002) are also plausible.

Rather than estimating elasticities for violent and property crime separately, it may make sense to think about how these elasticities relate to one other. Violent crime and property crime are distinct categories only after the fact. A burglary, for example, becomes a robbery when the criminal breaks and enters a home and discovers an occupant. Drug dealing becomes homicide when turf needs to be protected and so forth. Thus we would expect that in the long run what deters violent crime will also deter property crime and vice versa, at least at the margin. The correlation between the violent crime rate and the property crime rate over the period (1973–2005) is 0.85. Of course, many things influence these rates, but the fact that the combined effect is similar across violent and property crime lends some credence to the idea that policing might influence these rates similarly,

Table 6.1 Elasticities of crime with respect to police

Paper	Property	Violent
Levitt (2002)	-0.50	-0.44
Di Tella and Schargrodsy (2004)	-0.33	n.a.
Klick and Tabarrok (2005)	-0.3	0
Evans and Owens (2007)	-0.26	-0.99
Average	-0.35	-0.48

again at least on a reasonable margin. Given this, an elasticity of police with respect to crime of 0.35 is consistent with the data and does not seem unreasonable.

What policy lessons can we draw from this estimate?

COST–BENEFIT ANALYSIS OF MORE POLICE

Some back-of-the-envelope calculations suggest that hiring more police would be a significant net benefit. Suppose that a 10 percent increase in the number of police reduced crime by 3.5 percent. Is this a big number? In 2007 there were 17 508 500 property crimes and 5 177 100 violent crimes; thus a 10 percent increase in the number of police would result in 612 798 fewer property crimes and 181 199 fewer violent crimes.

As of 2007, there were just under 700 000 full-time police officers in the USA (US Department of Justice, 2008); thus a 10 percent increase is an increase of 70 000 officers. The average annual cost of a police officer is on the order of \$54 000 (Evans and Owens, 2007; Bureau of Labor Statistics, 2008). Thus reducing crime by this magnitude would cost on the order of \$3.8 billion, or \$4760 per crime averted.

McCollister et al. (2009) provide the most recent estimates of the cost of crime, building on tort awards, estimates of the value of life and other sources.¹⁴ Table 6.2 lists costs for a sample of important crimes. Multiplying by the number of crimes in each category for 2007 leads to a total crime cost of \$407 billion, or \$21 000 per crime. Thus the cost–benefit ratio for increased police protection is very large.¹⁵

Given the cost of crime and the elasticity of crime with respect to police, how many police should we hire? We can get a rough estimate with some simple analytics. Write the total cost of crime as $Cost \times C(P)$, where $Cost$ is the cost per crime and $C(P)$ is the number of crimes as a function of the number of police (P) and write the cost of hiring police as $Wage \times P$. Then our problem is to minimize $TotalCost$, written as

$$\text{Min}_{(P)\text{olice}} TotalCost = Cost \times C(P) + Wage \times P$$

A necessary condition for minimization is the first-order condition (FOC):

Table 6.2 Cost of crime by crime offense type

Type of offense	Total cost(\$)*
Murder	8 980 497
Rape/sexual assault	238 366
Aggravated assault	104 610
Robbery	39 900
Motor vehicle theft	8 362
Household burglary	4 052
Larceny/theft	1 122

Note: * Adjusted to exclude cost of police.

Source: McCollister et al. (2009).

$$\text{Cost} * \frac{dC}{dP} = - \text{Wage} \quad (6.1)$$

Multiplying FOC (6.1) by $P/C(P)$, using the definition of elasticity and rearranging, we have:

$$\text{Wage} = \frac{E}{P} \times \text{Cost} \times C(P) \quad (6.2)$$

where E is the elasticity of crime with respect to the number of police. Notice that we can write the cost of crime at any P as the cost of crime at some initial P , P_0 , minus the change in the cost of crime as P moves from P_0 :

$$\text{Cost} \times C(P) = \text{Cost} \times (C(P_0) - \Delta C(P))$$

since $E = \frac{\Delta C(P)}{\Delta P} \times \frac{P}{C(P)}$ we can further rewrite this as

$$\text{Cost} \times \left(C(P_0) - E \frac{\Delta P}{P_0} C(P_0) \right)$$

Finally, substituting into (6.2) we have

$$\text{Wage} = \frac{E}{P} \times \text{Cost} \times C(P_0) \left(1 - E \frac{(P - P_0)}{P_0} \right) \quad (6.3)$$

If we use the fact that $\text{Cost} \times C(P_0)$ is the current cost of crime given the current number of police officers, which we assumed above is \$400 billion, $\text{Wage} = \$54000$ and $E = 0.35$ we can calculate the optimal P . Using these numbers produces an optimal police force of 1.5 million, or 117.8 percent larger than the currently existing force. Moreover, this number is surprisingly robust. Table 6.3 uses equation (6.3) to calculate the recommended increase in the police force as we vary the elasticity of police with respect to crime from 0.2 to 0.5 and as we vary the total cost of crime from \$200 billion to \$600 billion. All the estimates are positive and only with the lowest estimate of the cost of crime and the lowest estimate of the elasticity of police with respect to crime do we get a small recommended increase of 4.8 percent.

These estimates suggest that large increases in the number of police would be optimal. Given the difficulties of using political methods to optimally allocate funds to public

Table 6.3 Recommended percentage increase in police force by elasticity estimate and cost of crime

Elasticity \ Cost (\$billion)	0.2	0.25	0.3	0.35	0.4	0.45	0.5
200	4.8	24.3	39.8	51.7	60.5	66.7	70.8
400	78.4	99.0	111.4	117.8	120.0	119.7	117.7
600	133.0	149.0	154.9	154.7	151.1	145.8	139.6

goods, these estimates do not seem implausible. Nevertheless, there may be other considerations at play. The only cost of increasing police that we have taken into account is the cost of wages. If the public or a subset of the public assigns other costs to greater numbers of police, the political process may be responding to these concerns.

The literature reviews conducted by Cameron (1988) and Sherman (1992) concluded that the extant empirical analyses provide little confidence that larger police forces lead to crime reductions. Perhaps a better conclusion would have been that the extant empirical analysis provided little confidence that economists could identify the effect of police on crime. Since that time, advances in econometric identification strategies, including quasi-experimental methods and instrumental variables techniques, have allowed researchers to develop more credible estimates of the causal effect of police on crime. The evidence suggests that police have both a statistically and economically significant negative effect on crime rates. The large recommended increases in the number of police illustrate the power of economic reasoning and the importance of developing credible estimates.

PRISONS

Econometric research on the effects of prisons on crime falls into two general categories: (1) studies examining the effects of changing the number of criminals in prison (including the effects of changing a given individual's expected sentence length) and (2) studies examining the deterrence effects associated with changing prison conditions.

In studies examining changes in the number of individuals imprisoned, including those focusing on changes in sentencing, a fundamental problem involves distinguishing between deterrence effects and incapacitation effects. That is, changing a criminal's expected sentence length might affect his propensity to engage in criminal activity altogether. Also, by imprisoning more people (or the same population for a longer period), criminals will have fewer opportunities to commit crimes outside of the prison setting. From an empirical perspective, both of these effects would show up as a negative relationship between expected prison sentence served and crime, but the policy implications of the two causal mechanisms may differ. For example, in the parole context, if longer expected prison terms deter crime, it may be cost-effective to instruct parolees that if they recidivate, they will face much longer prison terms. In this way, the state can generate cost savings by releasing the criminal with the knowledge that the parolee is unlikely to commit a crime. On the other hand, if incapacitation is doing much of the work in generating a negative relationship between expected sentences and crime, parole itself becomes a relatively more expensive policy option.

One of the early papers to apply modern microeconomic methods to the relationship between prison and crime is Levitt (1996). In that paper, Levitt exploits the shock to the prison population induced by prison overcrowding litigation. In these lawsuits, public-interest groups argue, on behalf of prisoners, that crowded conditions violate the prisoners' civil rights. When successful, court orders mandate that if prison populations in a given state cannot be reallocated to ease the overcrowding problem, prisoners must be released to lower prison population density to acceptable levels. Levitt uses the status of these lawsuits as instruments to explain the change in crime rates for various crime categories conditional on a number of policy, demographic and economic covariates,

as well as state and year fixed effects. Levitt finds very large marginal effects related to releasing a prisoner, with results implying that for every additional prisoner released, an additional 15 crimes are committed during the next year. This effect is comparable to the mean number of crimes criminals report committing in a given year based on surveys. Presumably, most of this estimate is driven by changes in incapacitation since sentences do not formally change.¹⁶ Levitt finds that while this effect is present for both violent and property crimes, it is relatively larger for violent crimes. He goes on to estimate that increasing prison capacity and sentences likely represents a net gain to society, although these estimates are necessarily quite crude.

The validity of the Levitt (1996) result hinges on how much of a shock prison litigation actually represents. Levitt provides results from a test of overidentifying restrictions, suggesting that his instruments (i.e. changes in the status of this litigation) do indeed meet the exclusion restriction necessary for identification in the instrumental variables setting. Unfortunately, this test is not likely to be very powerful in this setting. Intuitively, what the test for overidentifying restrictions does is estimate the effect of the instrumented variable using each combination of $n-1$ of the n instruments. If each of these iterations generates effectively the same coefficient on the endogenous regressor, the test is 'passed'. The test is most powerful in cases where the instruments are largely unrelated to each other since in that case it is unlikely that each instrument could be endogenous in the same way by random chance. However, in Levitt's application, each instrument is some variation of a prison overcrowding litigation indicator, leaving open the possibility that each instrument is endogenous in the same way. Thus confidence in Levitt's results hinges on a reader's intuitions regarding how random such litigation is. If interest groups target their litigation resources toward those states most sympathetic to the plight of prisoners and if this characteristic is correlated with other unmodeled influences on crime in a state, causal interpretation is not possible.

Perhaps a stronger attempt to examine the effects of prison on crime involves exploiting sentencing changes within a state that affect some criminals but not others. The sentencing change provides the presumptive shock to prison expectations and/or populations while the presence of a comparison group of criminals/crimes allows the researcher to control for unobservable changes and trends in the state's crime patterns.

Kessler and Levitt (1999) take this approach by exploiting the passage of Proposition 8 in California in 1982. Proposition 8 increased the sentencing enhancement applied to repeat offenders, adding the greater enhancement of five additional years to a sentence for each prior conviction of a serious felony or an additional year for each prior prison term served for any offense. Kessler and Levitt then examine the crime patterns for serious and nonserious felonies before and after Proposition 8's implementation. To control for contemporaneous changes in the relationship between serious and nonserious felonies, they also look at similar results for the rest of the country. They find that affected crime rates decline by 4 percent in 1983 and the reduction grows during the following seven years. They interpret these results as being consistent with deterrence given the large immediate effect on crime, and the growing effect over time suggests that incapacitation is important as well.

While the presence of a within-state comparison increases confidence in the Kessler and Levitt estimates, DiNardo (2006) raises a concern about Kessler and Levitt's choice of examining only data from every other year without explanation and notes that

Webster et al. (2006) find conflicting results when all of the annual data are examined. Specifically, they find that the decline in crime rates in the effected categories begins before Proposition 8's enactment, and the slope of this trend remains constant through implementation. For example, for the rate of aggravated assaults with a firearm, Webster et al. show that the rate of decline is almost perfectly linear from 1980 to 1983 when data from individual years are examined. Additionally, they argue that nonserious felonies in general do not serve as a strong comparison group, instead preferring an examination of more directly comparable crimes. Namely, they compare aggravated assaults with (affected by Proposition 8) and without (not affected by Proposition 8) a firearm and burglary of a residence (affected) and burglaries of non-residential properties (not affected). In both cases, they find smaller effects (for burglaries, they find no effect) than those Kessler and Levitt attribute to Proposition 8's enactment in 1982.

Further, by focusing on aggregate state by crime category data, Kessler and Levitt do not fully exploit the available 'experiment' since the enhancements apply only to individuals with a previous record. By using micro data, one could identify any effect by examining recidivism among criminals who were convicted of serious felonies, using those without convictions for serious felonies as the within-state control group. Such a design would more precisely isolate the sentencing shock.

Helland and Tabarrok (2007) implement this micro data approach in examining California's subsequent 'three-strikes' sentencing enhancement implemented in March 1994. Under this policy, individuals previously convicted of certain serious felonies face an automatic doubling of their sentence for any subsequent conviction, as well as the requirement that they serve at least 80 percent of the sentence. For a third conviction, the sentence rises to 25 years to life, again with the 80 percent requirement. Using data from the US Department of Justice Bureau of Justice Statistics' (*Recidivism of Prisoners Released in 1994*), Helland and Tabarrok examine micro-level data on 7183 randomly selected prisoners who were released from California prisons in 1994.

Helland and Tabarrok compare the rearrest patterns of individuals with two previous offenses that count toward the three-strikes tally with the pattern of individuals who were tried for such offenses twice in the past but were eventually convicted of lesser offenses in one of those trials, so they only have a single strike under the law. Helland and Tabarrok estimate nonparametric hazard models to draw their comparisons, finding that individuals with two strikes were about 20 percent less likely to be arrested during the following three years as compared to individuals with only a single strike. This result represents deterrence without any confounding effect of incapacitation. They find similar results for Texas, a state with a significant number of observations in the data set, which also has a three-strikes law. To test for the possibility that unobservable differences between the 'treatment' and 'control' groups drives their results, Helland and Tabarrok perform the same analysis on data from prisoners released from Illinois and New York (two states with significant observations in the BJS data that did not pass a comparable three-strikes law), finding essentially no difference in rearrest likelihood between individuals with one or two previous strikes.

Shepherd (2002) also finds deterrent effects of the three-strikes law for both violent and property crimes using data aggregated at the county level using an instrumental variables analysis that is used to account for unobserved heterogeneity in sentencing across California counties. Although Shepherd reports that her instrumented results pass a test

for overidentifying restrictions, as described in the text, it does not appear as though she actually uses more than one plausibly exogenous instrument, a measure of the percentage of each county's population voting Republican in the previous presidential election. Further, because political preferences very likely influence many (unmodeled) norms and policies in a county, this instrument is unlikely to meet the exclusion restriction. Thus confidence in Shepherd's results boils down to whether one believes that county sentencing practices are not influenced by crime expectations. Worrall (2004) finds that, once county-specific trends are accounted for, the deterrence and incapacitation effects of differences in county sentencing practices with respect to the three-strikes law disappear, although he makes no attempt to control for endogeneity.

For prison to have a deterrent effect, the likelihood of being imprisoned must enter a criminal's analysis of marginal cost. For the work on sentencing enhancements, an underlying issue is whether the criminals are aware of the increases in sentence length generated by Proposition 8 and the later three-strikes law, and whether this information is incorporated into the individual's calculus. Viscusi (1986) is one of the few papers in the literature to examine this general issue directly. Using data from the NBER Survey of Inner City Black Male Employment (1979–80), Viscusi analyzes the relationship between an individual's subjective expectation of arrest, conviction and imprisonment for a particular crime and the compensating income differential associated with these likelihoods. Viscusi finds that income is positively related to these expectations, implying that criminals require higher compensation to subject themselves to a higher risk of being imprisoned. This result is consistent with the deterrence hypothesis and fills out some of the missing structural elements not explored in reduced-form microeconomic studies looking at crime rates directly. Although it remains unclear how aware criminals are of changes in sentencing probability distributions and more work needs to be done in this area, the standard economic approach of assuming rational maximizing behavior appears to work for criminals as well as for other agents in the economy.

In general, while most of the better-done studies do find a negative effect of prison sentences on crime, via both the deterrence and incapacitation channels, with many of the rough calculations suggesting that increasing sentences is cost-justified, skepticism is still reasonable on this question. The natural experiments in the literature allow for plausible endogeneity stories. Prison overcrowding litigation is likely to be targeted, not random, and sentencing behavior is not necessarily randomly affected by legislation. Presumably, more use of within-jurisdiction comparisons using micro data will allow researchers to make progress on this issue in future applications.

A second interesting strand of the prison literature involves examinations of the effect of prison conditions on crime. In particular, does prison brutalize or do brutal conditions merely increase deterrence? The results of this literature are mixed, with some studies finding deterrence effects while others do not. This is another area where it is unclear how well criminals incorporate information into their decision-making process. This issue is one that has been examined experimentally in the context of juvenile awareness programs that bring at-risk children to prisons to observe their conditions directly, presumably to scare the children into not committing crimes. In a meta-analysis of randomized treatment-control studies looking at subsequent criminal behavior, Petrosino et al. (2003b) found that these programs have no systematic deterrent effect on criminal outcomes among at-risk children. While this result may be reflective of the quality of the

awareness programs, it may suggest that prison conditions are not particularly salient in the criminal decision-making process.¹⁷

Among the most important papers to examine the effect of prison conditions on crime is Katz et al. (2003), which looks at the relationship between the death rate (from sources other than execution) in state prisons and crime rates, hypothesizing that prisons where more inmates die from violence or disease are viewed as being less attractive to potential criminals. They also note that this measure might proxy for prison conditions more generally. While they find no evidence of an effect on murder rates, they do find a statistically significant and large effect on violent crime rates that is robust across various time controls. For property crime, the effect is sensitive to the time control used.¹⁸ While the difference between violent and property crime might be attributable to noise, another possibility is that violent offenders, on average, go to prisons with worse conditions, so any deterrence effect of bad prison conditions will be concentrated among violent offenders. Although not the focus of this chapter, the Katz et al. paper has implications for research on the deterrence effect of the death penalty. In distinction to much of the work on this topic, Katz et al. find no systematic death penalty effect. Because essentially none of the other death penalty research includes prison death rates as a control, those results suffer an omitted variables bias if prison conditions are an important determinant of crime rates.

Chen and Shapiro (2007) examine the effect of prison conditions through a different approach. By focusing on recidivism rates, they ask whether prisoners in worse conditions recidivate at lower rates (presumably because they realize that prison is unattractive) or higher rates (because the bad prison conditions provide criminal human capital or have an anti-socialization effect). They argue that assignment of federal convicts to the various security levels¹⁹ of federal prison is essentially random at the margins. That is, while assignment level is generally a positive function of criminal severity, the break points are essentially arbitrary. They exploit this intuition to implement a regression discontinuity design whereby they instrument prison assignment (by security level) by whether the prisoner's security custody score is above the normal cut-off for assigning a prisoner to a facility above minimum security. In a variety of specifications, they find that inmates assigned to above minimum security prisons are more likely to be rearrested in any given period, suggesting that exposure to prisons with worse conditions does not generate a deterrent effect. Rather, it would seem that such exposure hardens criminals or provides them with additional criminal human capital. Confidence in the Chen and Shapiro results rests on their assertion that security custody scores are not systematically based on any unobservable (to the researcher) signals that lead officials to believe a criminal has a higher likelihood of recidivism. That is, Chen and Shapiro's identifying assumption is that individuals just above and below the minimum security cut-off point are, in fact, comparable. Chen and Shapiro present qualitative evidence that these kinds of unobservable characteristics do not affect an inmate's security custody score. However, the criteria Chen and Shapiro use to demonstrate the harsher treatment of prisoners in higher-security prisons may actually indicate worse baseline behavior of the inmates in those prisons, cutting against their identifying assumption.

Incorporating the Katz et al. and Chen and Shapiro findings into policy decisions is not easy. The Katz et al. estimates suggest that criminals do take into account prison conditions when making their decisions about committing a crime. While this may

suggest that making prisons more dangerous by providing substandard healthcare and by providing little protection against intra-prisoner violence can provide cheap deterrence, such policies would certainly come under legal scrutiny in the USA as violating prisoners' constitutional rights. As for Chen and Shapiro's findings, while higher-security prisons might have this unintended anti-deterrence effect, they presumably do a better job at incapacitation. This trade-off cannot be estimated directly from Chen and Shapiro's recidivism data.

Another paper in this vein looks at crime among women, a topic that generates relatively little attention. Bedard and Helland (2004) examine female violent crime rates in cities with populations exceeding 100,000 people during the period 1981–95. They hypothesize that if a woman expects to be sent to a prison that is relatively far from her residence, she will be less likely to engage in criminal activity for fear of losing contact with family and friends. They present a host of survey results suggesting that visits and phone calls to inmates decline as the distance between the inmate's city of residence and the prison increases. During their sample period, through the construction and reallocation of prison facilities, 45 cities experienced substantial decreases in the distance to the nearest female penitentiary, 68 cities saw the average distance increase substantially, and 83 cities saw negligible changes. The timing of these changes is spread throughout their sample period.

Bedard and Helland find that increases in the distance to the relevant female penitentiary lead to reductions in female crime rates. They find this result for violent crimes (7 percent reduction associated with a 40-mile increase) and property crimes (3 percent reduction associated with a 40-mile increase). Perhaps surprisingly, they find an even larger effect for homicide rates (13 percent reduction associated with a 40-mile increase), although this last result is statistically significant only at the 10 percent level.

Although Bedard and Helland suggest that this effect may be peculiar (or at least the effect may be larger) to women, and their design only allows estimation of the effect on female crime, if the effect is more general, it could suggest a relatively cheap way to induce deterrence. Namely, a strategy of placing prisoners far from their place of residence could generate a large benefit for a relatively small public cost.²⁰ British use of Australia as a penal colony is a notable (although perhaps extreme) case in point.

CONCLUSION

Although empirical work on the effects of the death penalty and gun laws on crime generates significant attention from both academics and public policy decision-makers, police and prisons may represent the best tools policy-makers have for combating crime. While early work on these subjects ran into difficult identification problems, researchers have made progress by exploiting natural experiments and micro-level data sets that allow for construction of better controls for unobservable effects that may bias the results of a policy evaluation.

Through these more sophisticated tools and highly detailed data sets, researchers are reaching a consensus that the expansion of police forces is causally associated with relatively large reductions in property crime rates on average. Further, most analyses suggest that hiring the marginal cop is more than cost justified on this basis. Consistent evidence

with respect to violent crime does not exist. The strong identification strategies used in this research leave open the possibility that results are peculiar to the natural experiment observed, suggesting that there is a high value to reproducing these studies in different jurisdictions to confirm external validity.

While the designs used to study the effects of imprisonment are not generally as powerful as those used in the police context, there is an emerging consensus that increasing criminals' expected prison terms does lead to deterrence. Combining this effect with the incapacitation effect generated by prisons, expansion of the prison system would also appear to be cost-justified. Evidence on the effect of prison conditions is more limited, but there are indications that making prison conditions harsher might also lead to a deterrence effect.

NOTES

1. We thank Dino Falaschetti and the editors for useful comments.
2. More generally, this is an instance of omitted variable bias wherein there is some variable or vector of variables z that affects both crime rates and staffing decisions. If the researcher is not able to control for z , the estimated correlation between crime and staffing will include the effect of z .
3. Clark, Gerald (1969), 'What happens when the police go on strike', *New York Times Magazine*, 16 November, sec. 6, 45, 176–85, 187, 194–5.
4. Elasticities here represent the percentage change in the crime rate for a 1 percent change in the number of police officers.
5. According to the Social Science Citation Index, this paper is Levitt's most cited article as of 20 September 2007.
6. McCrary was also unable to recreate Levitt's coding for the mayoral election timing. Although McCrary's own coding is more predictive of police hiring levels, using his coding leads to less precision in the estimates of the effect of police on crime. Levitt notes that he called a number of the mayor's offices in the cities whose election dates differed between Levitt (1997) and McCrary (2002) and representatives of those offices provided election dates that matched neither Levitt's nor McCrary's coding.
7. In an instrumental variables analysis, a researcher identifies one or more variables that are correlated with the regressor of interest, in this case number of police per capita, but are not directly related to the outcome of interest (i.e. crime). By removing the endogenous part of the police variable in a first-stage regression, the researcher is left with a measure of police that is not affected by the variable or variables z that jointly determines crime and police staffing levels. In this context, the intuition is that police and firefighter hiring are both affected by things like electoral strategies, the power of unions etc., but the number of firefighters should not influence crime rates directly.
8. The finding with respect to stolen automobiles is especially notable since the reporting rate within this crime category is very high (since a police report is needed to make an insurance claim). This cuts against an additional hypothesis that police are simply less interested in 'normal' crime (and, thus, unwilling to take a crime report) during the periods when the terror alert is raised.
9. The burglary result also cuts against the 'fewer tourists' alternate hypothesis since burglary, by definition, involves targets (i.e. residences) that cannot leave the city when the terror alert level changes.
10. However, it should be noted that Klick and Tabarrok's estimated elasticity is relatively close to that estimated by Di Tella and Schargrotsky (2004) and the property crime elasticity estimated in Levitt (1997). Another study, Corman and Mocan (2000), which uses changes in the size of the New York City police force to estimate the effect of police on crime in time series regressions, finds comparable effects of police on crime, although the results are not generally statistically significant.
11. The authors of this chapter as well as others have sought daily crime data from a number of big city police departments, including Chicago, Los Angeles and New York City, to examine whether the DC results are representative. Unfortunately, all of these cities have refused to provide daily crime data for these purposes.
12. A crime is said to be cleared if the police make an arrest and turn over the arrestee to the court for prosecution.
13. Mas suggests that these estimates are similar to other estimates of the effect of police on crime, although the results are not directly comparable unless one assumes that clearances are proportional to hires.

14. These are estimates from a preliminary working paper.
15. The costs estimated by McCollister et al. are within the range of those estimated per crime in previous studies, although the variance across studies is quite large (see, e.g., Cohen, 1988; Cohen et al., 2004; Miller et al., 2006; Rajkumar and French, 1997). Anderson (1999) estimates the total cost of crime at over \$1 trillion and the National Institute of Justice (1996) estimated the cost of medical expenses, lost earnings, and pain and suffering alone to be on the order of \$450 billion – these studies were done at a time when crime was higher but also when wages were lower. Thus a total of \$400 billion also seems well within the range of previous estimates and perhaps on the low side.
16. This is perhaps a bit of an overstatement if criminals incorporate the chance of prison litigation affecting their own sentences in the future, in which case deterrence may be at work as well.
17. Another possibility is that juveniles and adults differ systematically on this point. The comparability of children and adults in the deterrence context is an open question. Levitt (1998) finds substantial deterrence effects among juveniles by exploiting differentials in prison terms that arise when an individual attains the age of majority. However, using micro data, Lee and McCrary (2005) do not find a similar result, at least among Florida youth. The latter study may be problematic, however, given its assumption that youth penalties are less severe than those applied to adults (Levitt attempts to construct a measure of relative punitiveness based on how many minors are in prison relative to the arrest rate of minors). Legal scholars, drawing upon more complete institutional data, have thrown this assumption into question. See, for example, Fagan (2007), citing evidence that some jurisdictions may be more likely to give a life sentence without parole to a juvenile than to an adult.
18. Note that the regressions in this paper do not cluster standard errors at the state level, instead clustering at the state X decade level. Given the concerns of Bertrand et al. (2004) regarding serial correlation, the Katz et al. estimates may be too optimistic since there is likely positive dependence in both the death rate and the crime rate data that is not adequately accounted for.
19. They present evidence that as the prison security level increases: (1) likelihood of furlough declines; (2) the likelihood a prisoner is in his cell for more than eight hours a day increases; (3) the likelihood of serious injury increases; and (4) the likelihood of the prisoner being found guilty of violating a prison rule increases.
20. Interestingly, while some commentators may suggest that such a policy would unfairly disadvantage criminals with families, it could remediate some of the institutional privileges that criminals with families enjoy. For thorough discussions of these issues, see Collins et al. (2007, 2008).

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