Criminal Deterrence: A Review of the Literature

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Abstract

We review economics research regarding the effect of police, punishments, and work on crime, with a particular focus on papers from the last 20 years. Evidence in favor of deterrence effects is mixed. While there is considerable evidence that crime is responsive to police and to the existence of attractive legitimate labor market opportunities, there is far less evidence that crime responds to the severity of criminal sanctions. We discuss fruitful directions for future work and implications for public policy.

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Introduction

The day-to-day work of individuals employed in law enforcement, corrections and other parts of the criminal justice system involves identifying, capturing, prosecuting, sentencing, and incarcerating offenders. Perhaps the central function of these activities, however, is deterring individuals from participating in illegal activity in the first place. Deterrence is important not only because it results in lower crime but also because, relative to incapacitation, it is cheap. Offenders who are deterred from committing crime in the first place do not have to be identified, captured, prosecuted, setenced, or incarcerated. For this reason, assessing the degree to which potential offenders are deterred by either carrots (better employment opportunities) or sticks (more intensive policing or harsher sanctions) is a first order policy issue.

The standard economic model of criminal behavior draws on a simple expected utility model introduced in a seminal contribution by the late Gary Becker. This model envisions crime as a gamble undertaken by a rational individual. According to this framework, the aggregate supply of offenses will depend on social investments in police and prisons as well as on labor market opportunities which increase the relative cost of time spent in illegal activities.

Using Becker's work as a guide, a large empirical literature has developed to test the degree to which potential offenders are deterred. The papers in this literature fall into three general categories. First, many papers consider the responsiveness of crime to the probability that an individual is apprehended. This concept has typically been operationalized as the study of the sensitivity of crime to police, in particular police manpower or policing intensity. A second group of papers studies the sensitivity of crime to changes in the severity of criminal sanctions. The literature assesses the responsiveness of crime to sentence enhancements, three strikes laws, capital punishment regimes and policy-induced discontinuties in the severity of sanctions faced by particular individuals. The third group of papers examines the responsiveness of crime to local labor market conditions, generationally operationalized using either the unemployment rate or a relevant market wage. This literature seeks to determine whether crime can be deterred through the use of positive incentives rather than punishments.

The papers in each of these literatures can be viewed as measuring the degree to which individuals can be deterred from participation in criminal activity. Each of the literatures is vast. A challenge remains to characterize the pattern of the empirical findings and explain why individuals appear to be more responsive (and thus more deterrable) along certain margins than along others. In this article, we provide a brief review of each of the three literatures introduced above with the intention of rationalizing several apparently divergent findings. Section I provides a brief introduction to economic theories of deterrence. Section II considers research on the effect of police on crime, Section III considers the effect of prison and/or sanctions on crime and Section IV considers the responsiveness of crime to local labor market conditions. Section V concludes.

1 Theories of Deterrence

Deterrence is an old idea and has been discussed in academic writing at least as far back as 18th century treatises by Adam Smith (1776), Jeremy Bentham (1796) and Cesare Beccaria (1798). There are three core concepts embedded in theories of deterrence — that individuals respond to changes in the certainty, severity, and celerity (or immediacy) of punishment. Interestingly, in the criminological tradition, deterrence is often characterized as being either general or specific with general deterrence referring to the idea that individuals respond to the *threat* of punishment and specific deterrence referring to the idea that individuals are responsive to the *experience* of punishment. Economics prefers different terminology, reserving the term deterrence for what the criminologist calls general deterrence and describing specific deterrence as a change in information or, perhaps more exotically, a change in preferences themselves. In this section, we briefly characterize the way in which economists have formalized these concepts. In general, economic theories of deterrence have focused more heavily on certainty and severity. However, recent writing has increasingly characterized deterrence as part of a dynamic framework in which offender behavior is sensitive to their time preferences (Polinsky and Shavell 1999; Lee and McCrary 2009).

1.1 Economic Models of Crime

The earliest formal model of criminal offending in economics can be found in Becker's seminal 1968 paper, *Crime and Punishment: An Economic Approach*. The crux of Becker's model is the idea that a rational offender faces a gamble. He can either choose to commit a crime and thus receive a criminal benefit (albeit with an associated risk of apprehension and subsequent punishment) or not to commit a crime (which yields no criminal benefit but is risk free). The expected cost of committing a crime is a function of the offender's probability of apprehension, p, and the severity of the sanction that he will face upon apprehension, f. To be more specific, the individual can be said to face three potential outcomes each of which delivers a different level of utility: 1) the utility associated with the choice to abstain from crime, U_{nc}, 2) the utility associated with choosing to commit a crime that does not result in an apprehension, U_{c1}, and 3) the utility associated with choosing to commit a crime and which results in apprehension and punishment, U_{c2}. In such a formulation, the individual chooses to commit a crime and only if the following condition holds:

$$(1-p)U_{c1} + pU_{c2} > U_{nc} \tag{1}$$

That is, crime is worthwhile so long as its expected utility exceeds the utility from abstention.[1]

In addition to the clear role played in this model by the probability of apprehension, p, the formulation also suggests the importance of two additional

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[1] The "if and only if" holds if we maintain that the case of $(1-p)U_{c1} + pU_{c2} = U_{nc}$ implies no crime, an unimportant assumption we make henceforth to simplify discussion.

exogenous factors that could influence U_{c2} and U_{nc}. Crime becomes more attractive when the disutility of apprehension is slight (e.g., less unpleasant prison conditions), and it becomes less attractive when the utility of work is high (e.g., a low unemployment rate or a high wage). Becker operationalizes the disutility associated with capture using a single exogenous variable, f, which he refers to as the severity of the criminal sanction upon capture. Typically, f is assumed to refer to something like a fine, the probability of conviction, or the length of a prison sentence.[2] To a large degree, then, government maintains control over U_{c2}.

The utility associated with abstaining from crime, U_{nc}, is principally a function of the individual's ability to derive utility from non-illicit activities. In practice, this is typically thought of as the wage that can be earned in the legal labor market. When the legal wage rises, U_{nc} rises, thus reducing the relative benefit of criminal activity. It is fair to say that while government maintains some control over U_{nc}, it does so to a lesser extent than it does over the utility of punishment, U_{c2}.

Using these ideas, Becker rewrites (fn. 16) the expected utility confronting an individual contemplating crime as

$$EU = pU(Y - f) + (1 - p)U(Y) \qquad (2)$$

where Y represents the income associated with getting away with crime.[3] In this formulation, crime occurs if and only if $EU > U_{nc}$. Equivalently, we can define an indifference point, Y^*, such that crime occurs if and only if $Y > Y^*$. It is easy to see that

$$\frac{U(Y^*) - U_{nc}}{|U(Y^* - f) - U_{nc}|} = \frac{p}{1 - p} \qquad (3)$$

Several important ideas are embedded in (3). First, for the individual to elect to engage in crime, the gain relative to its loss must exceed the odds of capture. Dividing the numerator and denominator of the left side by U_{nc} yields a natural interpretation in terms of percentages. Consider a criminal opportunity where capture is n times as likely as not. Crime occurs if the anticipated percent improvement in utility associated with getting away with it is more than n times as large as the anticipated percent reduction in utility associated with apprehension. Second, an increase in p unambiguously reduces the likelihood of crime, as this increases the right-hand side of (3). Third, an increase in f unambiguously reduces the likelihood of crime as long as $U'(\cdot) > 0$, as this decreases the left-hand side of (3).

Under risk neutrality, the equation (3) simplifies. Define a as the income associated with abstaining from crime, i.e., $U(a) = U_{nc}$, define $c = f - b > 0$ as

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[2]In principle, f can be a function of many different characteristics of the sanction including the length of the sentence, the conditions under which the sentence will be served and the degree of social stigma that is attached to a term of incarceration, all of which are likely heterogeneous among the population.

[3]As Becker is careful to say, income "monetary and psychic."

the effective cost of punishment, and define $Y = a + b$ and $Y^* = a + b^*$, where b is the criminal benefit and b^* is the criminal benefit at which the individual is indifferent between crime and abstention. Then equation (3) reduces to

$$b^* = c\frac{p}{1-p}$$

This simplified version of the Becker model is the starting point of the dynamic analysis in Lee and McCrary (2009).

A somewhat different focus can be found in Ehrlich (1973), where the notion of the opportunity cost of engaging in crime is front and center. Perhaps unsurprisingly, labor economists have found it particularly attractive to view crime as a time allocation choice, and this type of formulation is found in several prominent papers including Lemieux, Fortin, and Frechette (1994), Grogger (1998), Williams and Sickles (2002) and Burdett, Lagos and Wright (2004) among others.[4]

The typical time allocation model of crime considers a consumer facing a constant market wage and diminishing marginal returns to participation in crime. This consumer maximizes a utility function that increases in both leisure (L) and consumption (C), where consumption is financed by time spent engaged in either legitimate employment (h_m) at a market wage (w) or time spent in crime (h_c) with a net hourly payoff of r. The consumer's constrained optimization problem is to maximize his utility function, $U(C, L)$, subject to the consumption and time constraints:

$$C = wh_m + rh_c + I \tag{4}$$
$$L = T - h_m - h_c \tag{5}$$

In (4), consumption is shown to be equal to an offender's legitimate income plus his non-legitimate income.[5] In (5), T is the individual's time endowment and leisure is the remaining time after market work and time spent in crime are accounted for.[6] The parameter r reflects the criminal benefit but it also reflects the costs of committing crime, namely the risk of capture and the expected criminal sanction if captured. In other words, r can be thought of as the wage rate of crime, net of the expected costs associated with the criminal justice system. In this way, criminal sanctions drive a wedge between the consumer's productivity in offending and his market wage, in turn, incentivizing market work over crime.[7]

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[4]For further details, see Gronau (1980).

[5]I represents non-labor income.

[6]Grogger assumes that the returns to crime diminish as the amount of time devoted to criminal activity increases — i.e., there is a function $r(\cdot)$ that translates hours spent participating in crime into income is concave. Diminishing returns implies that those engaging in criminal activity first commit crimes with the highest expected payoffs (lowest probability of getting caught and highest stakes) before exploring less lucrative opportunities. However, this need not be true.

[7]In order for an individual to commit any crime at all, there are two necessary and sufficient conditions. First, the marginal return to the first instant of time supplied to crime must exceed

An interesting question in both Becker's model and Ehrlich's model is whether individuals are more deterred by increases in p or in f. Becker addresses this in a straightforward way by asking whether the expected utility of crime is decreased more by a small percent increase in p or an equivalent percent increase in f. This makes sense because participation in crime should be monotonic in its expected utility. Becker's analysis shows that p is more effective if and only if $U''(\cdot) > 0$, i.e., if and only if individuals were risk preferring.[8] If individuals are averse to risk, increasing f is more effective than increasing p, and if individuals are risk neutral, then f and p are equally effective. Becker notes (fn. 12) that this conclusion is the opposite of that given by Beccaria regarding the effectiveness of punishment versus capture, and that the conclusion is similarly at odds with contemporaneous views of judges.

The model of Lee and McCrary (2009) emphasizes the dependence of this conclusion on the time preferences of the individual.[9] Intuitively, it seems like it would be hard to deter an impatient individual using a prison sentence, since most of the disutility of a prison sentence is borne in the future. Lee and McCrary propose modeling crime using a modification of the basic job search model in discrete time with an infinite horizon. Risk-neutrality is assumed, yet individuals in this model have very different responses to the capture and punishment.

In their model, criminal opportunities are independent draws from an identical "criminal benefit" distribution with distribution function $F(b)$. The individual learns of an opportunity each period and must decide whether to take advantage of it. If the individual engages in crime and is caught, she is imprisoned for S periods, where S is an independent draw from an identical sentence length distribution. As in the Becker model, capture occurs with probability p. If the individual abstains from crime, she obtains flow utility a and faces the same problem the next period. If she commits crime and is not caught, she obtains flow utility $a + b$ and faces the same problem next period. Finally, if she commits crime and is caught, she obtains flow utility $a - c$ for S periods, before confronting the same problem at the conclusion of her sentence.

The criminal benefit at which the individual is indifferent between crime and abstention is given by

$$b^* = c\frac{p}{1-p} + \nu\left\{c\frac{p}{1-p} + p\int_{b^*}^{\infty}(1 - F(z))dz\right\} \tag{6}$$

where $\nu = \mathbb{E}[\sum_{s=1}^{S-1}\delta^s]$ is a summary parameter governing how the distribution

the individuals valuation of time (in terms of how much consumption the person would be willing to forego for more time) when all time is devoted to non-market, non-crime activities. Second, the marginal return to crime for the first crime committed must exceed the individuals market wage. Thus, those who can command high wages or those who place very high value on time devoted to non-market/non-criminal uses will be the least likely to engage in criminal activity.

[8]Note, however, the observant criticism of Brown and Reynolds (1973), showing that this clean conclusion is the result of the modeling assumption that the baseline utility is that of getting away with crime.

[9]Further details regarding the Lee and McCrary model are given in McCrary (2010).

of sentences affects decision-making and δ is the discount factor.[10,11]

As in Becker's static model, crime is reduced by increases in p and increases in c. The added feature of this model, however, is that crime is also reduced by increases in sentence lengths, and this behavioral mechanism is modulated by time preferences. As is intuitive, patient individuals are quite responsive to increases to sentence lengths, but impatient individuals show much more muted responses. In the limit as the discount factor approaches zero, the individual is arbitrarily more responsive to capture than to punishment. The Lee and McCrary model thus provides a simple way to re-introduce older ideas regarding the importance of celerity into a Becker model.[12]

Ultimately, the models proposed by Becker and Ehrlich yield three main behavioral predictions: 1) the supply of offenses will fall as the probability of apprehension rises, 2) the supply of offenses will fall as the severity of the criminal sanction increases and 3) the supply of offenses will fall as the opportunity cost of crime rises. In other words, behavioral changes can be brought about either using carrots (better employment opportunities) or sticks (criminal justice inputs). The following section connects these core predictions to the empirical literatures that have sought to test whether these predictions hold in the real world.

1.2   Deterrence vs. Incapacitation

Generally speaking there are two mechanisms through which criminal justice policy reduces crime: deterrence and incapacitation. When by virtue of a policy change individuals elect not to engage in crime they otherwise would have in the absence of the change, we speak of the policy deterring crime. On the other hand, a policy change may also take offenders out of circulation, as for example with pre-trial detention or incarceration, preventing crime by incapacitating individuals. The incapacitation effect can be thought of as the mechanical response of crime to changes in criminal justice inputs. While deterrence can arise in response to any policy that changes the costs or benefits of offending, incapacitation arises only when the probability of capture or the expected length of detention increases.

The existence of incapacitation effects has profound implications for the study of deterrence. In particular, while research that considers the effect of a change in the probability of capture will, generally speaking, identify a mixture of deterrence and incapacitation effects, research that considers changes in the opportunity cost of crime is more likely to isolate deterrence. Likewise, while

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[10]For example, if we take S to be geometric, i.e., S has support $1, 2, \ldots, \infty$ and $P(S = s) = q(1-q)^{s-1}$, where q is the per-period release probability, then standard results using infinite series show that $\nu = (1-q)\delta/(1 - (1-q)\delta)$. Interestingly, this shows that under a geometric distribution for sentence lengths, reducing the probability of release is equivalent to increasing the individual's patience.

[11]Equation (6) is not an explicit equation for b^*, but it can be viewed as defining an implicit function. We can use numerical methods to solve for b^* (e.g., Newton's method works well), and comparative statics are straightforward using the implicit function theorem.

[12]For related modeling ideas from criminology, see Nagin and Pogarsky (2000), for example.

research on the effect of sanctions typically results in a treatment effect that is a function of both deterrence and incapacitation, clever research designs have been used to identify the effect of an increase in the severity of a sanction that is unlikely to result in an immediate increase in incapacitation.

For each literature discussed in this paper, we provide a discussion of the degree to which empirical estimates can be interpreted as providing evidence of deterrence as distinct from incapacitation. However, it is important to note that deterrence is itself a black box. In order to empirically observe a behavioral response of crime to a particular policy level, it must be the case that potential offenders perceive that the cost of committing a crime has changed (Nagin 1998; Durlauf and Nagin 2011; Nagin 2013). Moreover, the behavioral response of crime will depend on the accuracy of those perceptions. To wit, an intervention that successfully convinces potential offenders that the expected cost of crime has increased, regardless of whether this is actually the case, will likely reduce crime. The challenge for cost-effective public policy is to optimally trade off between police and prisons so as to maximize perceptual and, as such, actual deterrence.

2 Police and Crime

Becker's prediction that the aggregate supply of crime will be sensitive to society's investment in police arises from the idea that an increase in police presence, whether it is operationalized through increased manpower or increased productivity, increases the probability that an individual is apprehended for having committed a particular offense. To the extent that potential offenders are able to observe an increase in police resources and perceive a correspondingly higher risk to criminal participation, crime should decline through the deterrence channel.

Empirically, the challenge for this literature is that changes in the intensity of policing are generally not random. As a result, it is difficult to identify a causal effect of police on crime using natural variation in policing. Conceptually, the issue is that the responsiveness of crime to police may also reflect an important role for incapacitation. This arises from the idea that police tend to reduce crime mechanically, even in the absence of a behavioral response, by arresting offenders who are subsequently incarcerated and incapacitated.[13] The extent to which investments in police are cost-effective depends, in large part, on the degree to which police deter rather than simply incapacitate offenders. In this section, we consider the responsiveness of crime to both police manpower and police tactics, broadly defined. For each literature we discuss the challenges with respect to both econometric identification as well as interpretation of the resulting parameters as evidence in favor of deterrence.

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[13]In this context, deterrence can arise either from a general decrease in offending or from a shift towards less productive but correspondingly less risky modes of offending — for example, a shift from robbery to larceny.

2.1 Police Manpower

A large literature has used city- or state-level panel data and, recently, a variety of quasi-experimental designs to estimate the elasticity of crime with respect to police manpower.[14] This literature is ably summarized by Cameron (1988), Nagin(1998), Eck and Maguire (2000), Skogan and Frydal (2004) and Levitt and Miles (2006), all of whom provide extensive references.

The early panel data literature tended to report small elasticity estimates that were rarely distinguishable from zero and sometimes even positive, suggesting perversely that police increase crime.[15] The ensuing discussion in the literature was whether police reduce crime at all. Beginning with Levitt (1997), an emerging quasi-experimental literature has argued that simultaneity bias is the culprit for the small elasticities in the panel data literature.[16] The specific concern articulated is that if police are hired in anticipation of an upswing in crime, then there will be a positive bias associated with regression-based strategies, masking a true negative elasticity. The recent literature has therefore generally focused instead on instrumental variables (IV) strategies designed to overcome this bias.

The first plausible instrumental variable to study the effect of police manpower on crime was proposed by Levitt (1997). Leveraging data on the timing of mayoral and gubernatorial elections, Levitt provides evidence that in the year prior to a municipal or state election, police manpower tends to increase, presumably due to the desire of elected officials to appear to be "tough on crime." The exclusion restriction is that but for increases in police manpower, crime does not vary cyclically with respect to the election cycle. Using data from 57 cities spanning 1972-1997, Levitt reports very small least squares estimates of the effect of police and crime that are consistent with the prior literature. However, IV estimates are large and economically important with elasticities ranging from moderate in magnitude for property crimes (-0.55 for burglary and -0.44 for motor vehicle theft) to large in magnitude for violent crimes such as robbery (-1.3) and murder (-3). Ultimately, following a reanalysis of the data by McCrary (2002), the IV coefficients reported by Levitt were found to be insignificant after a problem with weighting was addressed.

Levitt (1997) has given rise to a series of related papers that seek to identify a national effect of police manpower on crime by identifying conditionally exogenous within-city variation in police staffing levels. These papers include Levitt (2002) which uses variation in firefighter numbers as an instrument for police manpower, Evans and Owens (2007) who instrument for police manpower

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[14]This elasticity can be thought of as a reduced form parameter that captures both deterrence effects as suggested by neoclassical economic theory as well as incapacitation effects that arise when offenders are incarcerated and thus constrained in their ability to offend.

[15]Papers in this literature employ a wide variety of econometric approaches. Early empirical papers such as Ehrlich (1973) and Wilson and Boland (1977) focused on the cross-sectional association between police and crime.

[16]Some of the leading examples of quasi-experimental papers are Levitt (2002), DiTella and Schargrodsky (2004), Klick and Tabarrok (2005), Evans and Owens (2007), Lin (2009) and Machin and Marie (2011).

using the size of federal COPS grants awarded to cities to promote police hiring and Lin (2009) who instruments for changes in police manpower using the idea that U.S. states have differential exposure to exchange rate shocks depending on the export intensity of local industry. These strategies consistently demonstrate that police do reduce crime.[17] However, the estimated elasticities display a wide range, roughly -0.1 to -2, depending on the study and the type of crime. Moreover, relatively few of the estimated elasticities are significant at conventional levels of confidence reflecting a great deal of sampling variability and the use of relatively weak instruments. In many cases, extremely large elasticities (i.e., those larger than 1 in magnitude) cannot be differentiated from zero. Overall, Chalfin and McCrary (2013) characterize the pattern of the cross-crime elasticities as, in general, favoring a larger effect of police on violent crimes than on property crimes with especially large effects of police on murder, robbery and motor vehicle theft.[18]

A second noteworthy contribution to the modern police manpower literature is that of Marvell and Moody (1996) who leverage the concept of *Granger causality* to explore the extent to which police manpower is, in fact, responsive to changes in crime. The motivation behind such an approach is that if crime is responsive to lagged police but police staffing is not responsive to lagged crime, then the case for instrumental variables is weakened considerably. Finding no evidence of a link between lagged crime rates and current police staffing levels at either the state or city level, Marvell and Moody estimate the responsiveness of crime to police using a standard two-way fixed effects model and report elasticities that are fairly small in magnitude (ranging from -0.15 for burglary to -0.30 for motor vehicle theft) and are more consistent with the early least squares literature than the IV literature that has proliferated in recent years.

Ultimately the Granger causality exercise is subject to the same omitted variables bias issues that plague any least squares regression model and is therefore of dubious value in establishing causality. Nevertheless, the weak evidence of a link between lagged crime and current police staffing presented in Marvell and Moody is, in our view, underappreciated. Given the large discrepancy between Marvell and Moody's estimates and those in Levitt (1997) which use the same underlying data, one of two propositions must be true: 1) Marvell and Moody's estimates of the effect of lagged crime on police manpower are biased due to the exclusion of important omitted variables, 2) There is no simultaneity bias between police and crime; discrepancies between least squares and IV estimates are instead driven by measurement errors in either police staffing or measures of UCR index crimes. This is an idea that is dealt with in detail in Chalfin and McCrary (2013). Leveraging two potentially independent measures of police manpower (one from the FBI's Uniform Crime Reports and another from the

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[17]Notably, Worrall and Kovandzic (2007) report no reduced form relationship between COPS grants and crime. However, their analysis is based on a smaller sample of cities than the analysis of Evans and Owens (2007).

[18]This pattern is found in several prominent panel data papers, in particular Levitt (1997), Evans and Owens (2007) and Chalfin and McCrary (2013) each of which report especially large elasticity estimates for murder (-0.6 to -0.8) and robbery (-0.5 to -1.4).

U.S. Census' Annual Survey of Government Employment) for a sample of 242 U.S. cities over a 51 year time period, Chalfin and McCrary construct measurement error corrected IV models using one measure of police as an instrument for the other. Their principal finding is that elasticities reported in the recent IV literature can be replicated by simply correcting for measurement errors in police data and without explicitly addressing the possibility of simultaneity bias. The resulting implication is that Marvell and Moody's basic inference regarding the lack of causality running from crime to police manpower may be correct. A related contribution in Chalfin and McCrary is to estimate police elastcities with remarkable precision, reporting elasticities of -0.67 ± 0.48 for murder, -0.56 ± 0.24 for robbery, -0.34 ± 0.20 for motor vehicle theft and -0.23 ± 0.18 for burglary.

While the majority of the police manpower literature uses aggregate data, there is a corresponding literature that assesses the impact of police on crime using natural experiments in a particular jurisdiction. An early account of such a natural experiment is found in Andenaes (1974) who documents a large increase in crime in Nazi-occupied Denmark after German soldiers dissolved the entire Danish police force (Durlauf and Nagin 2011; Nagin 2013). Modern literature has found similarly large effects. In particular, DeAngelo and Hansen (2010) document an increase in traffic fatalities that occured in the aftermath of a budget cut in Oregon that resulted in a mass layoff of state troopers. Similarly Shi (2009) reports an increase in crime in Cincinnati, OH in the aftermath of an incident in which police used deadly force against an unarmed African-American teenager.[19]

2.2 Police Deployment and Tactics

The police manpower literature is informative with respect to the aggregate response of crime to increases in police staffing. However, the aggregate manpower literature leaves many interesting and important questions unanswered. In particular, to what extent do the estimated elasticities reflect deterrence? Likewise, what is the specific mechanism that leads to deterrence — if the mechanism is based on perceptual deterrence — the idea that offenders observe an increase in police presence and adjust their behavior accordingly — then it should be the case that offending is especially sensitive to large and easily observed changes in police deployment and tactics. To address these questions, a related literature that is found mostly in criminology has studied the effect of changes in the intensity of policing on crime with a distinct focus on the crime-reducing effect of various "best practices." In particular, declines in crime that are not attributable to spatial displacement have been linked to the adoption of "hot spots" policing (Sherman and Rogan 1995, Sherman and Weisburd 1995, Braga 2001, Braga 2005, Weisburd 2005, Braga and Bond 2008, Berk and MacDonald 2010), "problem-oriented" policing (Braga et al 1999, Braga, Kennedy, Waring and Piehl 2001, Weisburd, Telep, Hinckle and Eck 2010) and a variety of

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[19]As Shi (2009) notes, the police response to the riot was to reduce productivity disproportionately in riot-affected neighborhoods.

other proactive approaches. Similarly, a large research literature that has examined the local impact of police crackdowns has consistently found large and immediate (but typically not lasting) reductions in crime in the aftermath of hyper-intensive policing (Sherman 1990). Such findings are further supported by evidence from several informative natural experiments which have identified plausibly exogenous variation in the intensity of policing. Three prominent examples are Klick and Tabarrok (2005) who study the effect of police redployments in Washington DC that result from shifts in terror alert levels, DiTella and Schargrodsky (2004) who study the effect of a shift in the intensity of policing in certain areas of Buenos Aires after a 1994 synagogue bombing and Draca, Machin and Witt (2011) who study police redployments in the aftermath of the 2005 London tube bombings.

The literature on police deployments and tactics has focused predominantly on three types of interventions. The first is an innovation commonly referred to as "hot spots" policing. As the moniker suggests, hot spots policing describes a strategy in which police are disproportionately deployed to areas in a city that appear to attract disproportionate levels of crime.[20] The second type of intervention is broadly referred to as "problem oriented" policing. This term is used broadly and refers to a collection of focused deterrence strategies that are designed to change the behavior of specific types of offenders. A final intervention that has received attention in the literature is that of "proactive" policing. Proactive policing refers to strategies that are deigned to make policing more intensive, holding resources fixed. The idea can be traced back to the concept of "broken windows" policing introduced by Wilson and Kelling (1982) and refers to the notion that just like fixing a broken window sends a message to would-be vandals that the community cares about maintaining social order, arresting individuals for relatively minor infractions sends a message to potential offenders that the police are watchful.

2.2.1 Hot Spots Policing

We begin with a discussion of hot spots policing. As obvious as the idea may sound, given resource constraints, the feasibility of such a deployment strategy relies on crime being sufficiently concentrated in a relatively small number of hot spots. A seminal paper by Sherman, Gartin and Buerger (1989) is the first to provide descriptive data on the degree to which crime is spatially concentrated. Using data from Minneapolis, Sherman and co-authors found that just three percent of addresses and intersections in Minneapolis produced 50 percent of all calls for service to to the police. This finding is echoed by Weisburd, Maher and Sherman (1992) and in more recent papers by Weisburd et al (2004) and Weisburd, Morris and Groff (2009) which report that a very small percentage of street segments in Seattle accounted for 50 percent of crime incidents for

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[20]The idea that crime hot spots might exist is an old one and can be found at least as far back as Shaw and McKay (1942). Modern research has linked criminal activity to specific types of places such as bars (Roman and Reid 2010) and apartment buildings as well as to places that lack formal or informal guardians (Eck and Weisburd (1995).

each year over a fourteen year period.[21] Naturally, the observation that crime is so highly concentrated in a very small number of places has led to efforts to intensify the focus of police resources on these places. These interventions have, in turn, led to a corresponding experimental and quasi-experimental research literature that seeks to evaluate the efficacy of such strategies.

The first order question that the hot spots policing literature seeks to address involves the degree to which highly localized crime is responsive to a change in the intensity of policing. However, a particular feature of this research makes it particularly salient for the study of deterrence (Nagin 2013). Notably, while the literature tends to find that intensive policing reduces crime, elements of intensive policing such as rapid response times do not appear to increase the likelihood of an arrest (Spelman and Brown 1981). Such a pattern in the data tends to be consistent with deterrence but not with incapacitation.

The first test of policing crime hot spots may be found in a 1995 randomized experiment conducted by Sherman and Weisburd in Minneapolis. The experiment tested whether doubling the intensity of police patrols in crime hotspots resulted in a decrease in crime and found that crime declined by approximately 10 percent in experimental places relative to control places. No evidence of crime displacement — the idea that crime is merely moved to an adjacent place — was found. Findings in Sherman and Weisburd (1995) have, to a large extent, been replicated in other places and contexts including the presence of open air drug markets and "crack' houses" (Hope 1994; Weisburd and Green 1995; Sherman and Rogan 1995b), violent crime hot spots (Sherman and Rogan 1995a; Braga et al 1999; Caeti 1999) and places associated with substantial social disorder (Braga and Bond 2008; Berk and MacDonald 2010). Indeed a review of the literature by Braga (2008) identified nine experiments or quasi-experiments involving hot spots policing and noted that seven of the nine studies found evidence of significant crime reductions. Notably, a majority of the literature finds no evidence of displacement of crime to adjacent neighborhoods indicating that the data are more consistent with deterrence (Weisburd et al 2006) while a number of studies have found that the opposite is true — that there tends to be a diffusion of benefits to non-treated adjacent places (Sherman and Rogan 1995a; Braga et al 1999; Caeti 1999).[22]

2.2.2   Problem-Oriented Policing

Intensive policing of hot spots is one way that police potentially deter crime. Another broad deterrence-based strategy is that of problem-oriented policing. Broadly speaking, this strategy entails engaging with community residents to identify the most salient local crime problems and designing strategies to deter unwanted behavior. Undoubtedly the most well-known evaluation of a problem-oriented policing approach is that of Boston's *Operation Ceasefire* by Kennedy,

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[21] An excellent review of this literature may be found in Weisburd, Bruinsma and Bernasco (2009).

[22] An excellent review of the theory and empirical findings regarding displacement in this literature can be found in Weisburd et al (2006).

Braga, Piehl and Waring (2001). The stated purpose of Ceasefire was to reduce youth gun violence in Boston, MA. The intervention involved a multi-faceted approach and included efforts to disrupt the supply of illegal weapons to Massachusetts as well as messages communicated by police directly to gang members that authorities would use every available "lever" to punish gangs collectively for violent acts committed by individual gang members. In particular, police indicated that the stringency of drug enforcement would hinge on the degree to which gangs used violence to settle business disputes. The result of the intervention was that youth violence fell considerably in Boston relative to other U.S. cities.

Indeed Ceasefire was so successful that it has given rise to a number of similarly-motivated strategies that are collectively referred to as "pulling levers." Prominent evaluations of pulling levers interventions include research carried out in Richmond, VA (Raphael and Ludwig 2003), Indianapolis (McGarrell at el 2006), Chicago (Papachristos, Meares and Fagan 2007), Stockton, CA (Braga 2008b), Lowell, MA (Braga et al 2008), High Point, NC (Corsaro, Hunt, Hipple and McGarrell 2012), Newark, NJ (Boyle et al 2010), Nashville (Corsaro and McGarrell 2010), Cincinnati (Engel, Corsaro and Tillyer 2010) and Rockford, IL (Corsaro, Brunson and McGarrell 2010). Researchers have also evaluated a multi-city pulling levers strategy known as *Project Safe Neighborhoods* (PSN) which enlisted to cooperation of federal prosectuors to crack down on gun violence.

On the whole, evaluations of pulling levers strategies produce extremely promising results though inference is invariably complicated by a lack of randomized experiments and the inherent difficulty in identifying appropriate comparison cities. With respect to individual evaluations, reductions in crime have been found in High Point, Chicago, Indianapolis, Stockton, Lowell, Nashville and Rockford and null findings have been found in Richmond and Cincinnati.[23] With respect to *Project Safe Neighborhoods*, the research is promising but not definitive. While McGarrell, Corsaro, Hipple and Bynum (2010) report that declines in crime were greater in PSN cities than in non-PSN cities, Corsaro, Chalfin and McGarrell (2013) point out that when pre-intervention trends are more fully accounted for, average declines in crime continue to exist albeit with a great deal of heterogeneity among cities.

2.2.3 Proactive Policing

A final strand of the police tactics literature in criminology investigates the responsiveness of crime to the intensity of policing, holding resources constant, an idea is that is generally referred to as "proactive" policing. As there is no standardized way to assess the extent to which individual police departments engage in policework that is proactive, in practice, this literature seeks to understand if the intensity of arrests for minor infractions has an effect on the incidence of more serious crimes. Such an empirical operationalization was first proposed

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[23]Braga and Weisburd (2012) provided an excellent review of the literature including a comprehensive meta-analysis of the research findings.

by Sampson and Cohen (1988) and has been replicated to various degrees by MacDonald (2002) and Kubrin, Messner, Deane, McGeever and Stucky (2005). The general strategy is to regress crime rates on a measure of policing intensity. In practice, policing intensity has been operationalized using the number of DUI and disorderly conduct arrests made per police officer. Using this approach has, in some cases, led to findings that are consistent with a deterrence effect of proactive policing. However, in the best controlled models, coefficients on the proactive policing proxy become small and insignificant. More importantly, these models are plagued by problems of simultaneity bias, omitted variables and the inevitable difficulty involved in finding a credible proxy for the concept of proactive policing as opposed to simply an environment that is rich in opportunities for police officers to make arrests.

A second focus of the literature has been on the advent of "broken windows" policing (also known as "order maintenance policing," a policy innovation proposed by Wilson and Kelling (1982). The idea behind broken windows policing is that police can affect crime through tough enforcement of laws governing relatively minor infractions such as vandalism and turnstyle jumping. Broken windows policing, in theory, operates primarily through perceptual deterrence — if offenders observe that police are especially watchful, they may update their perceived probability of apprehension for a more serious crime and accordingly will decrease their participation in crime. In the popular media, broken windows policing is an idea that is heavily associated with Mayor Rudolph Giuliani and New York Police Commissioner William J. Bratton who has attributed the dramatic decline in crime in New York City after 1990 to its rollout. A corresponding research literature has arisen to evaluate the effectiveness of broken windows policing — in practice, this literature has focused disproportionately on the experience of New York City. This literature produces mixed findings. On the one hand, time series analyses by Kelling and Sousa (2001) and Corman and Mocan (2005) find that misdemeanor arrests are negatively associated with future arrests for more serious crimes such as robbery and motor vehicle theft. However, later research has pointed out that these studies omit a control group and has tended to find that New York's aggregate crime trends are similar to those of other cities that did not institute a policy of broken windows policing (Eck and Maguire 2000; Rosenfeld, Fornango and Baumer 2005; Harcourt and Ludwig 2006). With respect to the newest research, a 2007 paper by Rosenfeld, Fornango and Rengifo uses precinct-level data and finds evidence of a negative effect of broken windows policing on crime, albeit a very small effect. Similarly, a particularly careful paper by Caetano and Maheshri (2013) finds no evidence of an effect of "zero tolerance" law enforcement policies on crime using micro-data from Dallas.[24] Overall, our reading if this literature is that the evidence in favor of an important effect of proactive policing on crime is weak.

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[24]Broken windows policing and the associated "stop and frisk" policy implemented by the New York City police department has generated substantial public controversy. A 2009 paper by Fagan Gellar, Davies and West provides evidence of the demographic burden of such policies which is disproportionately borne by African-Americans.

2.2.4 Police Deployments

The ubiquity of the hot spots, problem-oriented and proactive policing literatures in criminology has spawned a parallel literature in economics that seeks to learn from natural experiments in police deployments. Three prominent studies are those of DiTella and Schargrodsky (2004), Klick and Tabarrok (2005) and Draca, Machin and Witt (2011). Each of these studies leverages a redeployment of police in response to a perceived terrorist threat. The appeal of these studies is that terrorist threats are plausibly exogenous with respect to trends in city-level crime and therefore represent a unique opportunity to learn about the response of crime to changes in normal routines of policing. DiTella and Schargrodsky study the response of police in Buenos Aires to the 1994 bombing of a Jewish community center. In the aftermath of the bombing, Argentine police engaged in a strategy of "target hardening" synagogues by deploying disproportionate numbers of officers to blocks with synagogues or other buildings housing Jewish organizations. DiTella and Schargordsky report that the intervention led to a large decline in motor vehicle thefts on the blocks that received additional police patrols though the effects. Notably this result has been called into question by Donohue, Ho and Leahy (2013) who reanalyzed the original data and report evidence that is more consistent with spatial displacement of crime rather than crime reduction. However, with respect to identifying behavioral changes among offenders, both stories are equally consistent with deterrence. In a similar study, Klick and Tabarrok (2005) utilize the fact that when terror alert levels set by the U.S. Department of Homeland Security rise, property crime (but not violent crime) tends to fall in Washington D.C., with especially large declines in areas that receive the largest redployments of police protection. With respect to the U.K., Draca, Machin and Witt (2011) study the 2005 London tube bombings which resulted in sizeable shifts in the deployments of police from the surburbs to central London and find that "street crimes" such as robbery and theft are reduced considerably in areas that received additional officers.

With respect to studying variation in the spatial concentration of police, two additional papers are worth noting. Cohen and Ludwig (2002) exploit short-term variation in the intensity of police patrols by day of the week in several different Pittsburgh patrol areas. They found that shootings were considerably lower in areas and on days that received more intensive police patrols. With respect to the long-term consequences of patterns of police deployments, MacDonald, Klick and Grunwald (2013) use a spatial regression discontinuity design to study the impact of especially intensive policing around the University of Pennsylvania, a large urban university campus. In particular, areas directly adjacent to the university received police patrols from both the university and municipal police. Areas slightly further away from the campus received only municipal police patrols. The finding is that street crimes are substantially higher in the blocks just outside the area patrolled by the university police relative to the blocks just inside the university patrol area.

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2.3 Deterrence vs. Incapacitation

The literature has reached a consensus that increases in police manpower reduce crime, at least for a population-weighted average of U.S. cities. With respect to police deployments and tactics, the literature supports the idea that crime is responsive to hot spots policing and pulling levers strategies while evidence in favor of an effect of proactive policing strategies such as broken windows and disorder policing is more suspect. A remaining issue is to address the degree to which each of these literatures is informative with respect to disentangling deterrence from incapacitation.

With respect to the aggregate manpower literature, Levitt (1998) provides the first attempt to systematically unpack the relationship between deterrence and incapacitation by empirically examining the link between arrest rates and crime, a relationship which is negative. Levitt posits that this negative relationship can be explained either by deterrence, incapacitation or measurement errors in crime. Ruling out measurement errors as a likely culprit, he differentiates between deterrence and incapacitation using the effect of changes in the arrest rate for one crime on the rate of other crimes.[25] As Levitt notes, "in contrast to the effect of increased arrests for one crime on the commission of that crime, where deterrence and incapacitation are indistinguishable, it is demonstrated that these two forces act in opposite directions when looking across crimes. Incapacitation suggests that an increase in the arrest rate for one crime will reduce all crime rates; deterrence predicts that an increase in the arrest rate for one crime will lead to a rise in other crimes as criminals substitute away from the first crime." Levitt concludes that deterrence appears to be the more important factor, particularly for property crimes. Owens (2013) reports a similar finding, examining whether variation in police staffing resulting from the COPS hiring program led to increased arrests. Despite the fact that the program appears to have led to a decline in crime, no effect is found on arrests. As a result, Owens concludes that there is little evidence in favor of incapacitation, thus implying a large role for deterrence.

While analyses by Levitt (1998) and Owens (2013) are suggestive of a meaningful role for deterrence, it is nevertheless difficult to disentangle deterrence from incapacitation in this way. In particular, a null relationship between police and arrests is also consistent with the idea that police productivity decreases when there are fewer crimes to investigate. For this reason, while the aggregate data literature is ideal for understanding the overall relationship between police and crime, it is only somewhat informative with respect to the magnitude of deterrence. This point is further compounded by the observation that research has yet to document the degree to which offenders perceive or are aware of increases in police manpower (Nagin 1998).

We suspect that the literature on police tactics is considerably more infor-

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[25]Utilizing an insight from Grilliches and Hausman (1986) — that measurement errors should yield the greatest bias in short-differenced regressions — Levitt (1998) compares regression estimates of the relationship between crime and arrest rates using short- and long-differences, finding similar effects.

mative with respect to identifying deterrence. In particular, offenders are more likely to be aware of an enhanced police presence in small, local areas than relatively small changes in the number of police in a city spread out over a large geographic area. Likewise, while offenders tend to commit crimes locally, in order for incapacitation to explain the large declines in crime that occur in hot spots, it would have to be the case that offending is so local so as to be specific to a group of one or two blocks. The large drops in crime that occur in crime hotspots after they are more aggressively policed is more consistent with deterrence than with incapacitation. Focused deterrence ("pulling levers") strategies are also particularly informative in that declines in crime have been shown to be specific to the focus of the intervention. To the extent that at least some offenders are generalists rather than specialists who commit only a certain type of offense, such a pattern is more consistent with deterrence than with incapacitation.

In sum, while it remains possible that an increased police presence lowers crime by situating police officers in locations where they are more likely to arrest and incapacitate potential offenders, on the whole, the high degree of visibility around police crackdowns or hot spots policing suggests a potentially greater role for deterrence.[26]

3   Sanctions and Crime

A second idea in Becker's neoclassical model of offending is that crime will be responsive to the severity of punishment. Accordingly, a parallel literature considers the responsiveness of crime to the severity of criminal sanctions, along both the intensive and extensive margin. Three literatures, in particular, are worth mentioning. First, a series of papers considers the effect of sentencing policy generally or, alternatively, sentence enhancements on crime to test the prediction that crime will decrease in response to a sanction regime that either raises the probability of a prison sentence or raises the length of a prison sentence if given. A corresponding literature considers the effect of laws that govern the age of criminal majority and, as such, generate large and pervasive discontinuities in the sanctions that individual offenders face. Finally, a particularly prominent literature considers the effect of a capital punishment regime or the incidence of executions on murder. Since executions enhance the expected severity of the sanction without directly affecting an offender's probability of capture, this literature is potentially compelling with respect to understanding deterrence as, subject to satisfying the standards of econometric identification, it allows for the isolation of a pure deterrence effect.

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[26]An important exception to this intuition however can be found in Mastrobuoni (2013) who studies the responsiveness of crime to regular shift changes among the various police forces in Milan, Italy. Matsrobuoni finds that despite large temporal discontinuities in clearance rates during shift changes, robbers do not appear to exploit these opportunities and oncludes that there is only limited evidence of deterrence. A remaining question is the extent to which the result depends on the ability of potential offenders to accurately perceive these discontinuities.

3.1 Sentencing

One of the most basic tests of the neoeconomic model of crime concerns the responsiveness of crime to the severity of criminal sanctions. Over the past few decades, a literature has arisen to document the sensitivity of crime to various sentencing schemes, sentence enhancements, clemency policies,"three strikes" laws and other legislative actions which change the expected cost of a criminal sanction. A corresponding literature measures the responsiveness of crime to the size of the prison population. With respect to identification, two challenges are particularly pressing. First, it is difficult to discern the effect of sentencing policies (which, in the United States, are generally enacted at the state level) from other crime reduction interventions as well as time-varying factors that inform the supply of crime more generally. Second, just as prison populations may affect crime, crime may have a reciprocal effect on prison populations creating the potential for simultaneity bias.[27] With respect to identfying deterrence, the chief difficulty is that sanctions lead to deterrence but typically also to incapacitation. This section reviews the literature that seeks to understand the relationship between sanctions and offending with a particular interest in discerning the effect that sanctions have on deterrence.

3.1.1 Prison Populations and Crime

While identifying the elasticity of crime with respect to a sanction, in principle, requires an exogenous shock to the sanctions regime, a natural starting point in unraveling the crime-sanctions relationship is to consider the elasticity of crime with respect to the size of the prison population. Studies of the crime-prison population elasticity generally utilize state-level panel data and regress the growth rate in crime on the first lag of the growth rate in a state's share of prisoners. Marvell and Moody (1994) provide the first credible empirical investigation of the elasticity of crime with respect to prison populations, estimating an elasticity of -0.16. As in their police paper, they use the concept of Granger causality in an attempt to rule out a causal relationship that runs from crime to prison populations. As discussed in Section 2, the approach does not offer particularly strong reasons to believe in the ignorability of selection bias.

A genuine breakthrough in this literature is found in Levitt (1996) who, using similar data, exploits exogenous variation in state incarceration rates induced by court orders to reduce prison populations. The intuition behind the approach is that the timing of discrete reductions in a state's prison population owing to a court order should be as good as random. Levitt's estimated elasticities are considerably larger than those in Marvell and Moody: -0.4 for violent crimes and -0.3 for property crimes, while the largest elasticity reported is for robbery (-0.7).[28] An alternative identification strategy can be found in Johnson and Raphael (2012) who develop an instrumental variable to predict future changes

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[27]For a comprehensive review of identification issues in this literature see Durlauf and Nagin (2011).

[28]Levitt's analysis is replicated by Spelman (2000) who reports qualitatively similar findings.

in incarceration rates. The instrument is constructed by computing a theoretically predicted dynamic adjustment path of the aggregate incarceration rate in response to a given shock to prison entrance and exit transition probabilities. Given that incarceration rates adjust to permanent changes in behavior with a dynamic lag, the authors identify variation in incarceration that is not due to contemporaneous criminal offending. Using state level panel data covering 1978-2004, Johnson and Raphael (2012) estimate the elasticity of crime with respect to prison populations of approximately -0.1 for violent crimes and -0.2 for property crimes. Notably the estimated elasticities for earlier time periods were considerably larger and closer in magnitude to those estimated by Levitt (1996). Johnson and Raphael conclude that the criminal productivity of the marginal offender has changed considerably over time as incarceration rates have risen, a conclusion that is echoed by Liedka, Piehl and Useem (2006). With respect to juveniles, Levitt (1998) studies the response of juvenile crime to the punitiveness of state-level juvenile sentencing along the extensive margin (the number of juveniles in custody per capita), concluding that changes in juvenile sentencing explain approximately 60 percent of the growth in juvenile crime during the 1970s and 1980s. Using Levitt's results, Lee and McCrary (2009) compute an implied elasticity for violent crimes of -0.4.

In sum, estimates of the elasticity of crime with respect to prison are generally modest and fall between -0.1 and -0.7. Estimates for violent and property crimes are of approximately equal magnitude and there is evidence that the elasticity has diminished considerably over time as prison populations have grown. Our best guess is that the current elasticity of crime with respect to prison populations is approximately -0.2 as reported by Johnson and Raphael.[29] This funding is further bolstered by a recent evaluation of "realignment," a policy implemented in California to reduce prison overcrowding by sending additional inmates to county jails where they tend to serve shorter sentences. Lofstrom and Raphael (2013) report that, with the exception of motor vehicle theft, there is no evidence of an increase in crime despite the fact that 18,000 offenders who would have been incarcerated are on the street due to the realignment policy. The magnitude of this elasticity leaves open the possibility for non-trivial deterrence effects of prison but, given that prison generates sizeable incapacitation effects, the magnitude of deterrence is likely small.

3.1.2   Shocks to the Sanctions Regime

A related literature considers the effect of a discrete change in a jurisdiction's sanctions regime that is plausibly not a function of crime trends more generally. The general approach is to utilize a differences-in-differences design to compare the time-path of crimes covered by the sentence enhancement to that of uncovered crimes. The earliest literature (Loftin and McDowall 1981; Loftin, Heumann and McDowall 1983; Loftin and McDowall 1984; McDowall, Loftin and Wiersma 1992) considered the effects of sentence enhancements for specific

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[29]A 2009 review of the literature by Donohue reaches a similar conclusion.

crimes — particularly gun crimes, generally finding little evidence in favor of deterrence. A more recent paper studies the impact of changes in sentencing in the aftermath of London's 2011 riots. Leveraging the fact that judges in the U.K. handed down harsher sentences for "riot offenses" in the six months following the riots, Bell, Jaitman and Machin (2013) find evidence of sizeable declines in riot offenses relative to non-riot offenses which, in the absence of changes in policing, they attribute to the advent of a harsher sanctions regime. This claim is bolstered by the fact that there was a relative decline in riot offenses in sectors that experienced the brunt of the 2011 riots as well as sectors that saw no riot activity.[30]

A second class of studies has examined the impact of changes in the sanctions regime that have heterogeneous impacts on different groups of offenders. For example, Drago, Galbiati and Vertova (2009) study the effect of a 2006 collective clemency of incarcerated prisoners in Italy. Prisoners incarcerated prior to May 2006 were released from prison with the remainder of their sentences suspended while prisoners incarcerated after May 2006 were ineligible for the clemency. Released prisoners, however, were subject to a sentence enhancement for any future crimes committed that were serious enough to merit a sentence of at least two years. For such crimes, the sentence would be augmented by adding the amount of time the prisoner was sentenced to serve prior to his pardon to his new sentence. Thus the intervention created a situation in which otherwise similar individuals convicted of the same crime faced dramatically different sanctions regimes. The results of this natural experiment suggest an elasticity of crime with respect to sentence length of approximately -0.5 at one year follow up. Utilizing the same natural experiment, Buonanno and Raphael (2013) report evidence that incapacitation effects forgone as a result of the collective clemency were large, thus constraining the magnitude of the deterrence effect.

Similar findings are reported for the United States by Helland and Tabarrok (2007). Using data from Californias three strikes regime, Helland and Tabarrok (2007) compare the criminal behavior of individuals convicted of a second "strikeable" offense to those tried for a second strikeable offense but who were ultimately convicted of a lesser offense. The authors find evidence of an appreciable deterrent effect, calculating that California's three strikes legislation reduced felony arrest rates by approximately 20 percent among criminals with two strikeable offenses against them on their record. Similarly while Zimring, Hawkins and Kamin (2001) find little evidence of an overall effect of three strikes legislation, they do find evidence that individual offenders with two strikes are less likely to be arrested. Given that the deterrence margin is most salient at two strikes, these studies stand out as especially important with respect to identifying a meaningful deterrence effect of sentencing. On the other hand, the magnitude of the response is actually quite small once one considers the increase in sentence lengths associated with three strikes. Helland and Tabarrok's estimates suggest to an elasticity of crime with respect to sentence length

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[30]Sentencing did change along both the intensive and extensive margins indicating the incapacitation cannot be ruled out.

of -0.06.

3.2   Capital Punishment Regimes

Variation in the presence or intensity of capital punishment generates a potentially excellent source of variation with which to test for the magnitude of general deterrence. In particular, to the extent that variation in a state's capital punishment regime is unrelated to changes in the intensity of policing, the effect of capital punishment represents a pure measure of deterrence with any response of murder to the presence or intensity of capital punishment not plausibly attributable to incapacitation.[31]

There are been two primary approaches to identifying deterrence effects of capital punishment. One approach considers the use of granular time series data or event studies to identify the effect of the timing of executions on murder. Time series studies typically use vector autoregression to assess whether murder rates appear to decline in the immediate aftermath of an execution. Prominent examples include Stolzenberg and D'Alessio (2004) which finds no evidence of deterrence and Land, Teske and Zhang (2009) which finds evidence of short-run deterrence. Event studies such as those of Grogger (1990) and Hjalmarrson (2009) examine the daily incidence of homicides before and after executions. Both Grogger (1990) and Hjalmarrson (2009) find little evidence of deterrence effects though as Charles and Durlauf (2012) and Hjalmarrson (2012) note, with a limited time horizon, it is not possible to distinguish between deterrence and temporal displacement. A related study, Cochran, Chamblin and Seth (1994) considers the effect of Oklahoma's first execution in more than twenty years and finds evidence that the execution appears to have increased murder among strangers, an effect they attribute to a "brutalization" hypothesis though is equally well attributed to statistical noise. A final study worth noting is that of Zimring, Fagan and Johnson (2010) who compare homicide rates between Singapore which uses the death penalty with variable intensity and Hong Kong which does not use the death penalty. The paper finds no evidence in favor of deterrence as both countries experience similar homicide trends over the thirty-five year time period studied.

Broadly speaking, the time series and event studies literatures offer little support in favor of deterrence though, as noted by Charles and Durlauf (2012), the literature is plagued by several conceptual problems which compromise the interpretability of estimated treatment effects. In particular, the focus of the time series literature on executions as opposed to the sanctions regime more generally marks a divergence from the neoclassical model of crime insofar as the occurence of an execution does not per se change the expected severity of a criminal sanction for murder.[32] Indeed the research design is often motivated by the

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[31]The argument is that in the absence of a capital punishment regime or a death sentence, a convicted offender would nevertheless be sentenced to a lengthy prison sentence such as a life sentence without the possibility of parole.

[32]An important exception to this general point can be found in Chen and Horton (2012) which studies the effect of executions for desertion among British soldiers during World War

assumption that an execution affects an offender's perceived sanction. However, there is little evidence, empirical or otherwise to support this assumption. Second, Charles and Durlauf note that the underlying logic of time series analyses of executions and murder operationalize as deterrence the dynamic correlations between a shock to one time series and the levels of another. As the authors note, this is an arbitrary conceptualization of what is meant by deterrence.

A second literature studies the deterrent effect of capital punishment utilizing panel data on U.S. states to identify the effect of a capital punishment statute or the frequency of executions on murder among the public at large. In particular, these studies have exploited the fact that in addition to cross-state differences in sentencing policy, there is also variation over time for individual states in the official sentencing regime, in the propensity to seek the death penalty in practice, and in the application of the ultimate punishment (Chalfin, Haviland and Raphael 2012). This literature has generated mixed findings with several prominent papers (e.g., Dezbakhsh, Rubin and Shepherd 2003; Mocan and Gittings 2003; Zimmerman 2004, 2006; Dezbakhsh and Shepherd 2006) finding large and significant deterrence effects and several equally prominent papers (Katz, Levitt and Shustorovich 2003; Berk 2005; Donohue and Wolfers 2005, 2009; Kovandzic, Vieraitis and Paquette-Boots 2009) finding little evidence in favor of deterrence.[33]

While evidence in favor of deterrence is mixed, recent reviews by Donohue and Wolfers (2005, 2009) and Chalfin, Haviland and Raphael (2012) as well as a 2011 report commissioned by the National Academy of Sciences point to substantial problems in a number of papers that purport to find deterrence effects of capital punishment. These problems include the use of weak and/or inappropriate instruments (Dekbakhsh, Rubin and Shepherd 2003; Zimmerman 2004), failure to report standard errors that are robust to within-state dependence (Dezbakhsh and Shepherd 2006; Zimmerman 2009) and sensitivity of estimates to different conceptions of perceived execution risk (Mocan and Gittings 2003).[34] More generally, the panel data literature suffers from the threat of policy endogeneity, failure to include accurate controls and a lack of knowledge regarding how potential offenders perceive execution risk. Finally, as noted by Berk (2005) and Donohue and Wolfers (2005), results are highly sensitive to the inclusion of certain states and even certain influential data points (i.e., Texas in 1997). The most careful paper to date is that of Kovandzic, Vieritis and Paquette-Boots (2009) who use a dataset spanning a longer period of time, employ an expanded set of control variables and explore a wide variety of operationalizations of the

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I and finds evidence that executions deter desertion, but actually encouraged desertion when the execution was for an offense other than desertion or if the executed soldier was Irish. The reason why this study stands as an exception to the rule proposed by Charles and Durlauf is that during a time of war, the sanction regime is likely to be in constant flux.

[33]The debate continues with recent responses to critiques by Donohue and Wolfers (2005) offered by Zimmerman (2009), Dezbakhsh and Rubin (2010) and Mocan and Gittings (2010).

[34]While Mocan and Gittings (2010) provide an extensive summary of the robustness of results reported in Mocan and Gittings (2003), Chalfin, Haviland and Raphael (2012) point out that the responsiveness of murder to execution risk relies on the assumption that individuals are executed fairly soon (within six years) of a conviction.

effect of capital punishment and execution risk. The authors find no evidence of a deterrent effect.

3.3   Sanction Nonlinearities

A second literature that seeks to estimate the magnitude of deterrence effects does so by exploiting nonlinearities in the severity of sanctions faced by certain offenders. Typically, these studies estimate the incidence of arrest rates for young offenders who are either just below or just above the age of criminal majority — either 17 or 18 years of age depending on the state. While offenders below a given state's age cutoff are treated as juveniles, offenders who are just above the age of majority are tried as adults and are subject to considerably more severe sanctions. Given that the conditional probability of an arrest is smooth as a function of age around the age of criminal majority, any behavioral response of offenders to the threshold is likely to be due to deterrence.

Using data from Florida, Lee and McCrary (2009) document a sizeable discontinuity in the probability that a young offender is sentenced to prison depending upon whether the arrest occured prior to or after the offender's eighteenth birthday. Despite the fact that the expected sentence length for an adult arrestee is over twice as great as that faced by a juvenile offender, Lee and McCrary find little evidence of deterrence. Their estimates suggest an elasticity of crime with respect to sentence lengths of approximately -0.05, an estimate that is far smaller than that of Drago, Galbiati and Vertova (2009). Findings in Lee and McCrary are perhaps surprising but are supported by results reported in Hjalmarrson (2009) who documents that perceived increases in the severity of sanctions at the age of criminal majority among juvenile offenders are smaller than the actual changes. The implication is that deterrence is not operational because perceptions do not match the incentives created by public policy. Indeed, in a reduced form analysis using national-level data in the National Longitudinal Survey of Youth (NLSY), Hjalmarrson (2009) finds little evidence of deterrence using self-reported data on offending.

A final paper worth mentioning is that of Hjalmarrson (2008) which studies the effect of serving time in prison on subsequent arrest among juvenile offenders in Washington State. Exploiting a discontinuity in the state's sentencing guidelines, Hjalmarrson reports that incarcerated juveniles have lower propensities to be reconvicted of a crime. This deterrent effect is also observed for older and for more criminally experienced offenders. The differential findings in Hjalmarrson (2008) on the one hand and Hjalmarrson (2009) and Lee and McCrary (2009) on the other hand can potentially be rationalized by the fact that while the latter studies considered general deterrence (the behavioral response to a general threat of punishment), the former study measures specific deterrence — that is, the behavioral response to actual punishment that has already been experienced.

3.4 Deterrence vs. Incapacitation

As with the literature examining the response of crime to the certainty of apprehension, the primary conceptual challenge to interpreting the empirical literature on sanctions is that it is difficult to discern between deterrence and incapacitation. With respect to studies of the crime-prison population elasticity, two issues merit discussion. First, the size of a state's prison population is only a proxy for the punitiveness of the sanctions regime. In practice, the size of the prison population is a function of many things: the underlying rate of offending, the certainty of punishment (in part due to the probability of apprehension) and the criminal propensity of the marginal offender when the prison population changes. Prison population is a stock not a flow and accordingly when the prison population declines it can be due to either an increase in the contemporary probability of a custodial sentence or to flows out of prison (Durlauf and Nagin 2011). Likewise, deterrence is only one of the mechanisms by which prisons affect crime, the other being incapacitation. For these reasons, the literature that examines the crime-prison population elasticity while important with respect to public policy is not particularly informative with respect to deterrence.

In our view, research that studies the instantaneous impact of shocks to the sanctions regime are considerably more informative. Indeed identifying the sensitivity of crime to a shock to the sanctions regime is conceptually close to testing Becker's prediction that behavior will respond to the severity of a sanction. However, even with perfect identification, attributing a change in offending that occurs in the aftermath of a sanctions shock to deterrence requires a logical leap. In particular, the logical leap is greatest when the sanctions regime becomes more punitive along both the intensive and extensive margin. To the extent that a custodial sentence becomes both longer and more likely, tougher sentencing generates both deterrence and incapacitation effects. This is an issue in interpreting much of the literature on sentence enhancements. Such a concern is addressed in Kessler and Levitt (1998) which studies the effect of California Proposition 8, a ballot amendment that enhanced the length of sentences for certain felonies but not for others. Because prior to Proposition 8, each of the felonies already required mandatory prison time, any instantaneous response of crime to Proportion 8 would have to be attributable to deterrence. Kessler and Levitt find that crimes that were eligible for the enhancement fell by between 4 and 8 percent in the aftermath of Proposition 8 relative to a control group of crimes not eligible for the enhancement. The implication of these findings is that increased sanctions promote substantial deterrence. The validity of Kessler and Levitt's results have been called into question by Webster, Doob and Zimring (2006) who argue that crime did not fall in the aftermath of Proposition 8 and by Raphael (2006) who argues that crimes ineligible for sentence enhancements do not form an appropriate control group for crimes eligible for the enhancement.

Changes in a state's use of capital punishment, in theory, offers a more appropriate means of identifying deterrence. This is because capital murder is

sufficiently serious as to warrant prison time regardless of the specifics of the sentencing regime. Hence, when an offender is sentenced to death (as opposed to a sentence of life without the possibility of parole), there is no instantaneous incapacitation effect. With respect to capital punishment, the evidence of deterrence is, at best, mixed with the most rigorous studies failing to find evidence of deterrence. Moreover, the identification problems in the literature are considerable as it is difficult to identify a shock to a state's capital punishment regime that is plausibly exogenous.

Undoubtedly the best tests for deterrence may be found in research that follows individual offenders who, upon being apprehended, face different sanctions for a given crime. To the extent that differential treatment is driven by arbitrary distinctions within the criminal justice system, research can identify deterrence by comparing the behavior of offenders who are otherwise similar but are treated differently. Such a research design is truly quasi-experimental in the sense that treatment effects can be interpreted using the language of the Rubin causal model. Moreover, individual-level studies track the behavior of individuals who are not in prison and accordingly are not incapacitated, any behavioral shift is plausibly attributable to deterrence. These individual-level studies produce mixed evidence. On the one hand, studies of three strikes laws establish that offenders with two strikes are less likely to reoffend than offenders with one strike (Zimring, Hawkins and Kamin 2001; Helland and Tabbarok 2007). Likewise, in Drago, Galbiati and Vertova's study of Italy's clemency bill, prisoners who faced harsher sanctions upon being re-arrested were less likely to be re-arrested. On the other hand, studies of sanction non-linearities in which offenders of slightly different ages receive differential treatment report little evidence of a large deterrence effect. These results might be rationalized by differences in the responsiveness to a sanction among offenders of different ages.

To date, the degree to which offenders are deterred by harsher sanctions remains an open question. Undoubtedly deterrence can exist in extreme circumstances in which the punishment is immediate and harsh. However, within the range of typical changes to sanctions in contemporary criminal justice systems, the evidence suggests that the magnitude of deterrence is not large and is likely to be smaller than the magnitude of deterrence induced by changes in the certainty of capture.

4   Work and Crime

The final pillar of the neoclassical model of crime considers the responsiveness of crime to a carrot (better employment opportunities) rather than a stick (certainty or severity of punishment). In particular, since the benefit of a criminal act must be weighed against the value of the offender's time spent in an alternative activity, an increase in the oportunity cost of an offenders' time can be thought of as a deterrent to crime. Indeed, this principle has generated considerable public support for a variety of policies designed to reduce recidivism

among offenders returning from prison — in particular, the provision of job training, employment counseling and transitional jobs.

The empirical literature examining the impact of local labor market conditions on crime can be divided into two related but distinct research literatures. The first literature examines the relationship between unemployment and crime. A second literature examines the impact of responsiveness of crime to wages.[35] With respect to both literatures, approaches to study the effect of labor markets on crime are varied and include papers that use individual microdata as well as state- or county-level variation. Taken as a whole, the literature that uses aggregate data to disentangle the effect of economic conditions on crime presents a mixed picture. In general, results are sensitive to the time period studied, the population under consideration, the type of wage or unemployment rate that is employed, as well as the criminal offenses analyzed. However, more recent and carefully identified papers tend to find evidence of a fairly robust relationship between both unemployment and wages and crime.

4.1   Unemployment

Periods of unemployment are thought to generate incentives to engage in criminal activity either as a means of income supplementation or consumption smoothing or, more generally, due to the effect of psychological strain (Chalfin and Raphael 2011). To the extent that a decline in unemployment raises the opportunity cost of crime without generating a subsequent increase in the probability of apprehension or the severity of the expected sanction, the response of crime to changes in the unemployment rate can be thought of as capturing, among a host of behavioral responses, deterrence.

In general, the early literature linking unemployment and crime has produced mixed and frequently contradictory results leading Chiricos (1987) to characterize scholarly opinion on the topic as a "consensus of doubt." In particular, Chiricos found that, among the studies he reviewed, fewer than half found significant positive effects of aggregate unemployment rates on crime rates.[36] This conclusion is echoed in reviews by Freeman (1983), Piehl (1998), Mustard (2010) and Chalfin and Raphael (2011).

Recent literature on the topic of unemployment and crime has benefited from several methodological advances — in particular, the use of panel data as opposed to a cross-sectional data or national time series. Examples of panel data

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[35]There is also a large and growing experimental literature that evaluates how at-risk individuals have responded to the provision of job coaching, employment counseling, career placement and other employment-based services.

[36]Nonetheless, Chiricos review also found that the unemployment-crime relationship was three times more likely to be positive than negative and fifteen times more likely to be positive and significant than negative and significant, indicating a basis for further research. The results were especially strong for property crimes; in particular, for larceny and burglary. Chiricos suggests that research results are generally consistent by level of aggregation though they tend to be more consistently positive and significant at lower levels of aggregation. This hypothesis is echoed by Levitt (2001) who likewise argues that national-level time-series analyses obscure the unemployment-crime relationship by failing to account for rich variation across space.

research include Entorf and Spengler (2000) for Germany, Papps and Winkelmann (2002) for New Zealand, Machin and Meghir (2004) for the United Kingdom and Arvanites and Defina (2006) and Ihlanfeldt (2007) for the United States. With the exception of Papps and Winkelmann (2002), each of these papers finds evidence in favor of a link between unemployment and crime, in particular, property crime.

A second innovation in the recent literature has been to employ instrumental variables to address the potential endogeneity between labor market conditions and crime. The first such study is that of Raphael and Winter-Ebmer (2001) who use a state-level panel data set covering 1979-1998 to study the effect of unemployment rates on various types of crime employing two instruments for the unemployment rate – the value of military contracts with the federal government as well as the regional impact of shocks to the price of oil. For property crime rates, the results consistently indicate a positive effect of unemployment on crime with a one percentage point increase in the unemployment rate predicting a 3-5 percent increase in property crime. For violent crime, however, the results are mixed. Gould, Weinberg and Mustard (2002) provide a similar analysis at the county level, using a county's initial industry mix and measures of skill-biased technical change as an instrument for unemployment. They too find evidence of a positive relationship between unemployment and crime, particularly property crime. Taken as a whole, results reported in Raphael and Winter-Ebmer (2001) and Gould, Weinberg and Mustard (2002) imply that variation in unemployment rates explained between 12 percent and 40 percent of the decline in property crime during parts of the 1990s. In a more recent paper, Lin (2008) builds on these approaches using exchange rate shocks to isolate exogenous variation in unemployment rates. Lin reports that a one percentage point increase in unemployment leads to a 4 to 6 percent decline in property crime and would explain roughly one third of the crime drop during the 1990s.[37]

On the whole, the preponderance of the evidence suggests that there is an important relationship between unemployment rates and property crime but little impact of unemployment on violent crime. In the recent literature which is more careful with respect to addressing omitted variables bias and simultaneity, the relationship between unemployment and property crime is found regardless of the level of aggregation (counties or states).[38] The relationship between unemployment and property crime is empirically meaningful as property crime would be predicted to rise by between 9 and 18 percent during a serious recession

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[37]Fougere et al. (2009) provide a similar analysis for France, finding effects that are similar in magnitude.

[38]Prominent IV papers including Raphael and Winter-Ebmer (2001), Gould, Weinberg and Mustard (2002) and Lin (2008) do not uniformly find that instrumening results in a more positive relationship between unemployment and crime as would be predicted by the omission of pro-cyclical control variables or simultaneity bias. Another explanation for slippage between least squares and IV estimates of the effect of unemployment on crime is measurement errors in the unemployment rate. To the extent that such errors are classical, attenuation bias will mean that 2SLS estimates will exceed OLS estimates. To the extent that this pattern is not found, there is the possibility that OLS estimates are actually *upward* biased due to simultaneity or omitted variables.

in which unemployment increased by three percentage points. Moreover, this, if anything, may understate the magnitude of the relationship as crime appears to be particularly sensitive to the existence of employment opportunities for low skilled-men (Schnepel 2013). Nevertheless, the estimates remain sensitive to the time period studied. To wit, property crime has generally continued to decline through the recent Great Recession which increased unemployment rates nationally by as many as four percentage points.

4.2   Wages

A second and related research literature considers the impact of wage levels on crime rates. There are several a priori reasons to expect a stronger relationship between crime and wages than between crime and unemployment. First, as noted by Gould, Weinberg and Mustard (2002), since criminal participation is associated with a set of fixed costs, crime may well be more responsive to long term labor market measures such as levels of human capital or wages than to unemployment spells, which are typically ephemeral. Second, at any given time, the number of individuals who are employed in low-wage jobs vastly outnumbers the number of unemployed and, as such, wages for unskilled men may play a proportionally greater role than unemployment in encouraging crime (Hansen and Machin 1999). In fact, among individuals who reported engaging in crime during the past year, a large majority reported wage earnings (Grogger 1998) and three quarters were employed at the time of their arrest (Lynch and Sabol 2001), indicating that the behavior of a majority of offenders should be sensitive to changes in the wage.

The literature linking wages to crime has, in general, generated more consensus than the unemployment literature. Prominent panel data papers include Doyle, Ahmed and Horn (1999) who analyze state-level panel for 1984-1993 and find that higher average wages reduce both property and violent crime (elasticity estimates vary between -0.3 and -0.9) and Gould, Weinberg and Mustard (2002) who restrict their analysis to the wages of relatively low skilled men and find, using a county-level panel spanning 1979 to 1997, that the falling wages of unskilled men in this period led to an 18 percent increase in robbery, a 14 percent increase in burglary and a 7 percent increase in larceny. These findings are striking in that they indicate that wage trends explain more than half of the increase in both violent and property crimes over the entire period.

In a similar analysis for the U.K., Machin and Meghir (2004) examined changes in regional crime rates in relation to changes in the 10th and 25th percentile of the regions wage distribution and focus on the retail sector, an industry where low skilled workers have the ability to manipulate their hours of work. They find that crime rates are higher in areas where the bottom of the wage distribution is low. With regard to microdata, Grogger (1998), leveraging data from the NLSY , finds that youth wages account for approximately three quarters of the variation in youth crime. Finally, a related literature considers the responsiveness of crime to minimum wages and consistently finds evidence in favor of a negative relationship between the two variables (Corman and Mocan

2005; Hansen and Machin 2006; Fernandez, Holman and Pepper 2012).

4.2.1 Identifying Deterrence

A first order issue in interpreting research on the effect of police and prisons on crime concerns the extent to which deterrence can be disentangled from incapacitation. This issue is not relevant in considering the responsiveness of crime to changes in wages or employment conditions. Nevertheless, it is worth considering whether a significant coefficient on the wage or the unemployment rate in a crime regression necessarily identifies deterrence. In particular, for several reasons, crime and unemployment may be related for reasons other than deterrence. First, a relationship between macroeconomic conditions and crime may exist due to the relationship between macroeconomic conditions and *criminal opportunities* (Cook 1985). For example, during a recession auto thefts tend to decline presumably because fewer people are employed and therefore drive their cars (Cook 2010). Second, employment conditions and crime may be linked through behavioral changes that cannot be properly characterized as deterrence. For example, a displaced worker may well have feelings of anger or loss of control that manifest themselves in violent behavior. In such a case, the job may not be protective against crime through any deterrence mechanism *per se*. Nevertheless a robust relationship between economic conditions and crime is potentially consistent with the idea that individuals respond to incentives, at least at the margin.

5 Conclusion

We reviewed three large literatures regarding the responsiveness of crime to police, sanctions and local labor market opportunities. Three key conclusions are worth noting. First, there is robust evidence that crime responds to increases in police manpower and to many varieties of police redeployments. With respect to manpower, our best guess is that the elasticity of violent crime and property crime with respect to police are approximately -0.4 and -0.2, respectively. The degree to which these effects can be attributed to deterrence as opposed to incapacitation remains an open question though analyses of arrest rates suggests a role for deterrence (Levitt 1998, Owens 2013). With respect to deployments, experimental research on hot spots policing and focused deterrence efforts have, in some cases, led to remarkably large decreases in offending.

Second, while the evidence in favor of a crime-sanction link is generally mixed, there does appear to be some evidence of deterrence effects induced by policies that target specific offenders with sentence enhancements. This is seen in the effect of California's three strikes law on the behavior of offenders with two strikes (see Helland and Tabarrok 2007) and in the behavior of pardoned Italian offenders (Drago, Galbiati and Vertova 2009). On the other hand, while the elasticity of crime with respect to sentence lengths appears to be large in the Italian case, it is quite small in the California case.

Finally, there is fairly strong evidence, in general, of a link between local labor market conditions, proxied using the unemployment rate or the wage, on crime. While these effects are unlikely to be appreciably contaminated by incapacitation effects, they may reflect behavioral responses aside from deterrence.

Overall, the evidence suggests that individuals respond to the incentives that are the most immediate and salient. While police and local labor market conditions influences costs that are borne immediately, the cost of a prison sentence, if experienced at all, is experienced sometime in the future. To the extent that offenders are myopic or have a high discount rate, deterrence effects will be less likely. Moreover, given that an empirical finding of deterrence depends on the existence of perceptual deterrence, it may be the case that potential offenders are more aware of changes in policing and local labor market conditions than they are of changes in incarceration policy, with the exception of specific sentence enhancements that are individually targeted.

In closing, we note that Gary Becker's recent passing prompts us to acknowledge yet again the decisive impact of his landmark 1968 paper. As his ubiquity in this review makes clear, his insights launched an entire literature—one that has had and continues to have profound implications for and impact on public policy and safety.

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