Journal of Economic Literature 2017, 55(1), 5–48
https://doi.org/10.1257/jel.20141147

Criminal Deterrence: A Review of the Literature†

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We review economics research regarding the effect of police, punishments, and work on crime, with a particular focus on papers from the last twenty 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. (*JEL* J64, K42)

1. *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, sentenced, 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 that 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, a number of 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

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† Go to https://doi.org/10.1257/jel.20141147 to visit the article page and view author disclosure statement(s).

police manpower or policing intensity. A second group of papers studies the sensitivity of crime to changes in the severity of criminal sanctions. This literature assesses the responsiveness of crime to sentence enhancements, three strikes laws, capital-punishment regimes, and policy-induced discontinuities 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 and it is not unreasonable to suggest that each could merit a separate review. 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.

We are not the first to review the deterrence literature. Indeed, in last decade we count a number of comprehensive reviews on the subject including, but not limited to, Levitt and Miles (2006), Tonry (2008), Durlauf and Nagin (2011), Nagin (2013), and Chalfin and Tahamont (forthcoming). We attempt to differentiate our review in several ways. First, we have tried to synthesize research carried out by economists, as well as criminologists. In this goal, we are not alone. However, we are hopeful that by highlighting in the *JEL* research from criminology that is typically unknown to economists, we will help to further integrate the two disciplines. Second, interest in deterrence research has multiplied rapidly over the last few years, with a number of important studies having been published in the last year or two alone. Accordingly, we have done our best to include references to the newest and most cutting-edge research. Finally, in this review, we cover a topic that is often omitted from reviews of the deterrence literature—the role of labor markets in deterring crime via "carrots" rather than "sticks." The remainder of the paper is laid out as follows: section 2 considers research on the effect of police on crime, section 3 considers the effect of prison and/or sanctions on crime, and Section 4 considers the responsiveness of crime to local labor-market conditions. Section 5 concludes.

2. *Theories of Deterrence*

Deterrence is an old idea and has been discussed in academic writing at least as far back as eighteenth-century treatises by Adam Smith (1776), Jeremy Bentham (1789), and Cesare Beccaria (1764). 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 actual *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 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 and Lee and McCrary forthcoming).

2.1 *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 that results in apprehension and punishment, U_{c2}. In such a formulation, the individual chooses to commit a crime if, and only if, the following condition holds:

$$(1) \qquad (1-p)\,U_{c1} + p\,U_{c2} > U_{nc}.$$

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 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 (see footnote 16) the expected utility confronting an individual contemplating crime as

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

where Y represents the income associated with getting away with crime.[3] In this

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

[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."

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

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

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 the effective cost of punishment. Further define income, $Y = a + b$ 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 (forthcoming).

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 is accounted for.[6] The parameter r reflects the criminal benefit but it

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

[5] I represents nonlabor 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 concave function $r(\cdot)$ that translates hours spent participating in crime into income. 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.

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]

An interesting question in both Becker's model and Ehrlich's model is whether individuals are more deterred by increases in p or 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 (footnote 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.

[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 the individual's valuation of time (in terms of how much consumption the person would be willing to forgo for more time) when all time is devoted to nonmarket, noncrime activities. Second, the marginal return to crime for the first crime committed must exceed the individual's market wage. Thus, those who can command high wages or those who place very high value on time devoted to nonmarket/noncriminal 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.

The model of Lee and McCrary (forthcoming) 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

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

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

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

2.2 *Perceptions and Deterrence*

Because economic models of offending are microeconomic models that make predictions about individual behavior, our discussion of deterrence would be incomplete without a discussion of how individuals perceive risks and, especially, whether risk perceptions mirror reality. Given the scope of this review, our discussion of perceptions is necessarily brief. We direct the reader to an excellent review of this literature by Apel (2013) for a more detailed accounting of the perceptual-deterrence literature.

Perceptual deterrence is important because the vast majority of the empirical-deterrence literature operationalizes Becker's model of crime by studying the responsiveness of crime to particular policy variables, such as the number or productivity of police or the punitiveness of sanctions. This approach was initially borne out of the inadequacy of data needed to test the microfoundations of the Becker model, but has the advantage of having generated a dense literature that is practical and policy-relevant. Given that the literature studies the effect of policy variables, an important intermediate outcome and indeed a precursor to identifying deterrence is the extent to which potential offenders are aware that policy has changed (Waldo and Chiricos 1972, Nagin 1998, and Apel 2013). Apel (2013) characterizes the link between actual and perceived deterrence as involving a series of considerations that include both threat communication, the degree to which a change in the certainty or the severity of a sanction is communicated or advertised, and risk perceptions, the individual's perceived risk of being apprehended and punished. Crucially, risk perception is not assumed to be stable and indeed an important literature has arisen that seeks to understand how

[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 (2001), for example.

offenders update risk perceptions in response to experience (see Apel and Nagin 2011 for a comprehensive review on the subject).

Ultimately, one of the most important questions for perceptual-deterrence research is the degree of correspondence between actual and perceived risks. If perceptions closely mirror reality, then using policy shocks to learn about the magnitude of deterrence is straightforward. However, to the extent that changes in policy often go unnoticed by potential offenders, the outcomes of policy research will tend to be of limited value in studying deterrence. Consider, for example, a policy that increases the number of undercover police officers who are assigned to patrol a city's transit system. Assuming that policy is unannounced and, even if it is announced, that the news does not easily trickle down to potential offenders, it is difficult to imagine how deterrence will accrue. It may well be that the policy begins to be noticed by offenders as they hear about cases in which undercover officers have made arrests or if they have an acquaintance who has been arrested in this way. However, it seems likely that such information will generate deterrence only via a substantial temporal lag. Indeed, it seems likely that a highly visible change in the number of uniformed officers or, alternatively, a well-advertised policy to increase the number of undercover officers, will generate a greater deterrence effect, even if the actual intervention is no different.

The recent literature that links actual and perceived risks is relatively small. Important recent work includes that of Kleck et al. (2005) and Kleck and Barnes (2013), who conducted a telephone survey of 1,500 adults in fifty-four large urban counties in the United States. They asked each individual to estimate case clearance rates, the probability of serving time in prison, and maximum sentence for several different serious felonies. Comparing perceived risks to actual risks, they found little evidence of any correlations, a finding that extends to police manpower as well. Research by Lochner (2007) using the National Longitudinal Survey of Youth (NLSY) comes to a qualitatively similar conclusion reporting evidence of a significant, albeit weak, relationship between actual and perceived risks of apprehension. Likewise, in an application to drug use, a very common crime resulting in arrest, MacCoun et al. (2009) report that individuals living in states that have decriminalized marijuana often do not have any awareness of this and continue to believe that they can be jailed for marijuana possession. These studies are characterized by Apel (2013) as being discouraging for deterrence research. However, as each of the studies surveyed the general population, most of whom are uninvolved in crime, such research may have poor external validity. The best evidence on perceptions among a sample of active offenders comes from Lochner (2007), who reports that NLSY youth who self-report criminal involvement do, on average, have more accurate perceptions about arrest risks than noncriminally involved youth.

A second strain of research considers whether offenders change their risk perceptions in response to a past arrest. One flavor of this research has compared risk perceptions among individuals who reported more frequent arrest conditional upon offending (i.e., less successful offenders) to individuals who reported fewer arrests per offense (i.e., more successful offenders). This literature tends to find robust evidence of an association between more frequent arrest and a higher perceived probability of capture (Paternoster and Piquero 1995, Piquero and Pogarsky 2002, Pogarsky and Piquero 2003, and Carmichael and Piquero 2006). A parallel literature has found that risk perceptions are also informed by the experience of acquaintances (Piquero and Pogarsky 2002). Unfortunately, there are a number of conceptual issues that make this

literature difficult to interpret. Most notably, since these associations arise from cross-sectional data, it is not possible to discern cause from correlation. In particular, it is plausible that more successful offenders have lower perceived arrest probabilities for reasons that are a function of personality and largely unrelated to experience. In response to this concern, a more recent literature uses panel data to measure "updating"—the idea that individuals change their prior risk perceptions on the basis of whether or not they are apprehended in an earlier period. This literature has also tended to find robust evidence that risk perceptions are sensitive to actual experience (Pogarsky, Piquero, and Paternoster 2004; Pogarsky, Kim, and Paternoster 2005; Matsueda, Kreager, and Huizinga 2006; and Anwar and Loughran 2011). Several more specific findings from this literature are worth noting. First, while perceptions are responsive to experience, offending is not always responsive to perceptions, implying that at least a portion of offending may be idiosyncratic and perhaps undeterrable in a stable policy regime. Second, less experienced offenders are especially sensitive to the experiences of peers, which is sensible as they may not have sufficient history upon which to draw conclusions. Third, there is evidence that the general public, along with less frequent offenders, tend to overestimate their arrest risk and adjust their risk perceptions downward as they offend and recognize that the risk of apprehension is lower than they previously believed. An important corollary to this is that risk perceptions are more sensitive to experience early in one's criminal career, with the deterrence value of an arrest declining with experience (Anwar and Loughran 2011).

Broadly speaking, the perceptual-deterrence literature provides several reasons to be optimistic that meaningful deterrence effects can exist and can be particularly salient among younger offenders who have yet to commit to a criminal career. The best available evidence suggests that the experience of arrest does lead to an increase in the perceived likelihood of being apprehended for a future crime. What is less clear is whether perceived risks change in response to policy inputs that have more diffuse impacts and whether advertising sanctions can be a sufficiently credible threat—a proposition we discuss in further detail in the subsequent empirical section of this paper.

2.3 *Deterrence versus 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 pretrial 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 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 and, in some cases, other behavioral effects. 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, and 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.

3. *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, raises 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 is expected to 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. An additional, more conceptual 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.

3.1 *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 Frydl (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

[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.

[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 (1978) focused on the cross-sectional association between police and crime.

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 fifty-seven cities spanning 1972–97, 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. The insignificance of the coefficients is ultimately driven by the fact that the first-stage relationship between election cycles and police hiring is weak, complicating both estimation and inference.

Levitt (1997) has given rise to a series of related papers that seek to identify a national effect of police manpower on crime by isolating 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 using the size of federal Community Oriented Policing Services (COPS) grants awarded to cities to promote police hiring; and Lin (2009), who instruments for changes in police manpower using the idea that US 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 one in magnitude) cannot be differentiated from zero. Overall, Chalfin and McCrary (forthcoming) 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]

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

[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 (forthcoming), each of which report especially large elasticity estimates for murder (−0.6 to −0.8) and robbery (−0.5 to −1.4).

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 concerns 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, or (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 (forthcoming). Leveraging two potentially independent measures of police manpower (one from the FBI's Uniform Crime Reports and another from the US Census's Annual Survey of Government Employment) for a sample of 242 US cities over a fifty-one-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 elasticities 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 and Nagin 2013). Modern literature has found similarly large effects. In particular, DeAngelo and Hansen (2014) document an increase in traffic fatalities that occurred 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]

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

3.2 *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, and Berk and MacDonald 2010), "problem-oriented" policing (Braga et al. 1999; Braga et al. 2001; and Weisburd et al. 2010), and a variety of 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 that have identified plausibly exogenous variation in the intensity of policing. Three prominent examples are Klick and Tabarrok (2005), who study the effect of police redeployments in Washington, DC, that result from shifts in terror alert levels; Di Tella 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 redeployments 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 often 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 or to be successful in specific jurisdictions. 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," or disorder 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.

[20] The idea that crime hot spots might exist is immediately obvious to many and can be found in the academic literature 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 2012) and apartment buildings, as well as to places that lack formal or informal guardians (Eck and Weisburd 1995).

3.2.1 *Hot-Spots Policing*

We begin with a discussion of hot-spots policing, which we distinguish from aggregate police manpower research for several reasons. First, as the manpower literature largely uses city-level variation, it identifies the effect of adding police at the expense of some other type of public input. In contrast, hot-spots policing involves a reallocation of existing resources. Such a strategy is advantageous, as it does not require a change in current outlays. However, it also leaves open the possibility that moving police around merely shifts, rather than reduces, crime.

In order for hot-spots policing to be a viable crime reduction strategy, two conditions must be met. First, given resource constraints, the feasibility of such a deployment strategy relies on crime being sufficiently concentrated in a relatively small number of hot spots. Second, hot spots must be sufficiently stable such that the spatial distribution of crime in the absence of a change in police deployment can be predicted with a reasonable degree of accuracy. Hence, the adoption of hot-spots policing must begin with an accounting of the geographic concentration of crime, as well as an assessment of the extent to which hot spots are permanent as opposed to transitory. Sherman (1995) captures both of these ideas, characterizing crime hot spots as "small places in which the occurrence of crime is so frequent that it is highly predictable, at least over a one-year period."

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 coauthors found that just 3 percent of addresses and intersections in Minneapolis produced 50 percent of all calls for service 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 each year over a fourteen-year period.[21] With respect to predictability, Weisburd et al. (2004), using the same data from Seattle, used trajectory analysis to establish that hot spots tended to be highly persistent, often persisting for many years.[22]

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. By responsive, criminologists generally refer to the idea that crime declines in local areas that have been exposed to more intensive patrol without merely inducing equivalent spillovers to untreated adjacent areas. However, we note that while spillovers undermine the viability of hot-spots policing as a crime-reduction strategy, they nevertheless constitute evidence of responsiveness and, as such, are useful in identifying deterrence. Moreover, a particular feature of this research makes it especially 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

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

[22] Hot spots can, of course, also be temporary. An excellent accounting of efforts to predict temporary hot spots in Pittsburgh can be found in Gorr and Lee (2015).

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 hot spots 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—that is, spillovers—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, and Sherman et al. 1995), violent crime hot spots (Sherman and Rogan 1995, Braga et al. 1999, and Caeti 1999), and places associated with substantial social disorder (Braga and Bond 2008 and Berk and MacDonald 2010). Indeed, a review of the literature by Braga (2001) identified nine experiments or quasi-experiments involving hot-spots policing and noted that seven of the nine studies, including a majority of the randomized experiments, found evidence of significant and large crime reductions. Notably, a majority of the literature finds no evidence of displacement of crime to adjacent neighborhoods (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 nontreated adjacent places (Sherman and Rogan 1995, Braga et al. 1999, and Caeti 1999).[23] Both of these findings are perfectly consistent with our conceptualization of deterrence.

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

3.2.2 *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. The specifics are highly variable by design and are intended to leverage local resources to address highly local concerns. What these strategies have in common and why they are frequently referred to as "focused deterrence" strategies is that each of them attempts to generate deterrence through advertising (Zimring and Hawkins 1973). The idea is to create deterrence by making potential offenders explicitly aware of the risks of serious criminal involvement.

Undoubtedly the most well-known evaluation of a problem-oriented policing approach is that of Boston's Operation Ceasefire by Kennedy et al. (2001). The stated purpose of Ceasefire was to reduce youth gun violence in Boston. The intervention involved a multifaceted approach and included efforts to disrupt the supply of illegal weapons to Massachusetts. It also included 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 US cities included in the study.

Indeed, the perception of Ceasefire has been overwhelmingly positive and accordingly 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 et al. 2006), Chicago (Papachristos, Meares, and Fagan 2007), Stockton, CA (Braga 2008b), Lowell, MA (Braga et al. 2008), High Point, NC (Corsaro et al. 2012), 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 the cooperation of federal prosecutors to crack down on gun violence. A 2012 review of the literature by Braga and Weisburd suggests that pulling-levers strategies have been effective in reducing serious violent crime, with all reviewed studies finding negative point estimates, the majority of which were significant. 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.[24] With respect to Project Safe Neighborhoods, the research is promising but not definitive. McGarrell et al. (2010) report that declines in crime were greater in PSN cities than in non-PSN cities. However, there is a great deal of heterogeneity among cities, making it difficult to draw clear inferences.

On the whole, evaluations of pulling-levers strategies produce promising results, though inference is invariably complicated by a lack of randomized experiments and the inherent difficulty in identifying appropriate comparison cities. Identification problems are additionally compounded by the difficulty in identifying mechanisms, as each pulling-levers strategy is complex, multifaceted, and situation dependent, often involving changes in both the intensity of law enforcement as well as sentencing (e.g., Project Exile in Richmond, VA, as well as Project Safe Neighborhoods). Accordingly, it is easy to imagine that as additional resources are brought to bear, some of the effects of pulling-levers strategies might accrue via incapacitation effects. Concerns regarding identification have led Skogan and Frydl (2004) to conclude that such research is "descriptive rather than evaluative." Given the relatively large effect sizes reported in the literature, our reading of these papers is more optimistic than that of Skogan and Frydl. However, caution is warranted in characterizing this literature as having detected unassailable evidence of deterrence.

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

3.2.3 *Proactive and Disorder 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 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 police work 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. Building on a proposition in Wilson and Kelling (1982), such an empirical operationalization was first proposed by Sampson and Cohen (1988) and has been replicated to various degrees by MacDonald (2002) and Kubrin et al. (2010). 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 driving under the influence (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" or "disorder" 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 turnstile 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 (Kelling and Bratton 2015).

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, which experienced the largest decline in crime among major US cities. 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. On the other hand, later research has pointed out that these studies omit a control group and has tended to focus on the fact that New York's aggregate crime trends, while steeper, are broadly 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; and Harcourt and Ludwig 2006). The two most credible analyses, those of Harcourt and Ludwig (2006) and Rosenfeld, Fornango, and Rengifo (2007), use precinct-level data on misdemeanor arrests and violations and find either no effect or very small effects. More fundamentally, there are a number of alternative explanations for New York's dramatic reduction in crime including the receding of the crack epidemic (Blumstein 1995), changes in demographics that are poorly measured at lower levels of geographic granularity, general strategies to address disorder such as boarding abandoned buildings, and the implementation of the data-driven Compstat system (Weisburd et al. 2003). Accordingly, even if identification problems can be set aside, it is unclear that this literature can isolate the impact of disorder policing from other changes that drove crime down in New York City. Given the dramatic rollback of the New York City Police Department's "stop-and-frisk" policy in 2014 and the continued decline in serious crime, as well as the failure of the most careful studies to find evidence of large effects, we are skeptical that disorder policing has played a large role in the decline in crime in New York City.[25] Overall, our reading of this literature is that the evidence in favor of an important effect of proactive policing on crime is weak.

[25] 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 et al. (2009) provides evidence of the demographic burden of such policies that is disproportionately borne by African Americans.

Of course, New York City is not the only municipality to experiment with disorder policing and, in our view, some of the strongest evidence can be found in research from other cities. Three papers that employ especially strong research designs are worth mentioning. Braga et al. (1999) provides the first experimental evaluation of a strategy designed explicitly to address disorder. In Jersey City, NJ, twelve of twenty-four crime hot spots were randomly assigned to receive an intervention that involved disorder policing as well as other place-specific treatments that were intended to reduce crime. Such treatments include clearing vacant lots, requiring store owners to clean store fronts, and facilitating more frequent trash removal. Treated places experienced large declines in both crime and calls for service. In a follow-up study in Lowell, MA, Braga and Bond (2008) attempted to further isolate disorder policing from other types of disorder reduction, randomly assigning seventeen Lowell hot spots to receive a general disorder policing strategy. This study also showed strong reductions in crime in treated areas. However, the greatest gains were found in areas with an especially heavy focus on situational crime prevention, as opposed to arresting larger numbers of low-level offenders. Evidence in favor of an effect of misdemeanor arrests is far more limited. Finally, a particularly careful paper by Caetano and Maheshri (2014) finds no evidence of an effect of "zero tolerance" law enforcement policies on crime using microdata from police precincts in Dallas. Taken as a whole, the evidence suggests that reducing disorder is a promising strategy for controlling crime. However, it is difficult to characterize these reductions as deterrence. In particular, disorder reduction may simply help people to feel better about their neighborhoods, thus representing a shift in preferences, rather than movement along the curve that is induced by an increase in the perceived probability of capture by police. We remain skeptical that disorder *policing* provides evidence of deterrence.

3.2.4 *Changes in City-Wide 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. This literature is conceptually similar to the hot-spots literature with two exceptions. First, the identifying variation is naturally occurring in contrast to experimental manipulation, which may be excessively contrived. Second, several of the natural experiments identify the impact of a diffuse reduction in resources, rather than a concentration of resources at particular hot spots.

Three prominent studies are those of Di Tella 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. Di Tella 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. Di Tella 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 US Department of Homeland Security rise, property crime (but not violent crime) tends to fall in Washington DC, with especially large declines in areas that receive the largest redeployments of police protection. With respect to the United Kingdom, Draca, Machin, and Witt (2011) study the 2005 London tube bombings which resulted in sizable shifts in the deployments of police from the suburbs 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 (2003) 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 (2016) use a spatial regression discontinuity (RD) 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.

3.3 *Deterrence versus Incapacitation*

The literature has reached a consensus that increases in police manpower reduce crime, at least for a population-weighted average of US cities. With respect to police deployments and tactics, the literature supports the idea that crime is responsive to a visible police presence in hot spots and pulling-levers strategies that advertise deterrence, 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 that 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.[26] 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;

[26] 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.

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, which provided funding to increase the number of patrol officers in US cities, appears to have led to a decline in crime, no significant effect is found on arrests. As a result, Owens concludes that there is little evidence in favor of incapacitation, which necessarily must operate through arrests, thus implying a large role for deterrence.

While analyses by Levitt (1998) and Owens (2013) are suggestive of a meaningful role for manpower-induced 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. Moreover, the imprecise parameter estimates on arrest along with standard errors that are not trivial in size in Owens (2013) render it difficult to make strong claims regarding the null effect of police on arrests. 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 informative 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 hot spots after they are more aggressively policed is more consistent with deterrence than with incapacitation. Focused deterrence 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 incapaciitate potential offenders, on the whole, the high degree of visibility around police crackdowns or hot spots policing suggests a potentially greater role for deterrence.[27]

4. *Sanctions and Crime*

A second idea in Becker's neoclassical model of offending is that crime will be responsive to the certainty and severity

[27] 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. Matsrobuoni finds that despite large temporal discontinuities in clearance rates during shift changes, robbers do not appear to exploit these opportunities and concludes 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.

of punishment.[28] Accordingly, a parallel literature considers the responsiveness of crime to the harshness 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. In practice, this literature focuses primarily on the intensive margin, that is, the severity of punishment rather than the probability that a custodial punishment is given conditional upon being arrested. 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. Since adult sanctions are more intensive along both the intensive and extensive margins, such studies identify a reduced-form deterrence effect that does not explicitly differentiate between the certainty and the severity of punishment. 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 operationalized along the intensive margin.

4.1 *Sentencing*

One of the most basic tests of the Becker model of crime concerns the responsiveness of crime to the harshness 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 that 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. Attempts to isolate the causal effects of a change in state-level sentencing policy invariably encounter the inevitably difficult issues of choosing an appropriate comparison group and selecting from among many competing and equally plausible models of aggregate offending. Durlauf and Nagin (2011) refer to the latter of these issues as the problem of ad hoc model specification, referring specifically to the under-theorized manner in which individual-level mental processes are modeled and the arbitrary choice of control variables in regressions.

Second, just as prison populations may affect crime, crime may have a reciprocal effect on prison populations, creating the potential for simultaneity bias.[29] With respect to identifying deterrence, the chief difficulty is that harsher sanctions may lead to deterrence, but typically also to incapacitation.

[28] The term "certainty of punishment" is often used in the literature to refer either to the probability that an individual is apprehended or to the overall probability that an individual is punished conditional upon offending. In this section, in referring to the certainty of punishment, we are focusing more specifically on the probability that a punishment is handed out conditional upon arrest. This refers to the severity of punishment along the extensive margin.

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

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.

4.1.1 *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, while useful, does not offer compelling 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. This may not be strictly true, as the necessity of court orders to reduce overcrowding may itself be a function of rising crime rates. However, the strategy relies more specifically on the randomness of the precise timing of the order and, in our judgment, represents a plausible strategy for identifying a causal estimate of the effect of prison populations on crime. 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).[30] An alternative identification strategy can be found in Johnson and Raphael (2012), who develop an instrumental variable to predict future changes 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 in Johnson and Raphael 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 (forthcoming) compute an implied elasticity for violent crimes of −0.4.

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

In sum, estimates of the elasticity of crime with respect to prison are generally modest in comparison to police elasticities and fall between −0.1 and −0.7. The most recent estimates fall in the low end of that range. 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 (2012).[31] This finding 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 nontrivial deterrence effects of prison but, given that prison generates sizable incapacitation effects, the magnitude of deterrence is likely small.

4.1.2 *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 a sentence enhancement to that of uncovered crimes. The earliest literature (Loftin and McDowall 1981; Loftin, Heumann, and McDowall 1983; Loftin and McDowall 1984; and McDowall, Loftin, and Wiersma 1992) considered the effects of sentence enhancements for specific 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 United Kingdom handed down harsher sentences for "riot offenses" in the six months following the riots, Bell, Jaitman, and Machin (2014) find evidence of sizable declines in riot offenses relative to nonriot offenses which, in the absence of identifiable 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.[32]

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

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

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

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 California's 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. As Durlauf and Nagin (2011) note, individuals with one strike may not be an ideal comparison group for a variety of reasons—in particular, it may be the case that the individuals with two strikes had poorer legal representation or that the precise nature of their potential second-strike offense was qualitatively less serious. Nevertheless, the authors demonstrate that comparing two-strike to one-strike individuals is sufficient to remove a great deal of the selection bias that exists in comparing individuals with two strikes to the remainder of the charged population. 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. 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 an elasticity of crime with respect to sentence length of −0.06.

Last but not least, we survey a completely different idea with respect to changing the sanctions regime. While sentence enhancements and three-strikes laws are designed specifically to increase sanction severity across either the intensive or the extensive margin or both, it is possible to imagine simultaneously making one margin harsher and the other one less harsh. This is the premise underlying swift-and-certain sanctions regimes (Hawken and Kleiman 2009 and Kleiman 2009). The idea of swift-and-certain sanctions arises from the notion that myopic individuals are unlikely to be responsive to long sentences but may be highly responsive to short sentences if they are issued with near certainty. In recent practice, Hawaii's Opportunity Probation with Enforcement (HOPE) program is the canonical example of swift-and-certain sanctioning in action. In an effort to address chronic recidivism among probationers, Hawaii First Circuit Court Judge Steven Alm recognized that punishments for violating the terms of probation were fairly unlikely and, if meted out, tended to occur in the distant future. Moreover, the sanctions were typically harsh and, as such, costly. Judge Alm and his collaborators put into practice a program that addressed probation violations with immediate but light sanctions—typically ranging from warnings to spending up to a week in jail. Probationers were intensively monitored, with any violations resulting in a sanction. In a banner finding, Hawken and Kleiman (2009) find that individuals assigned at random to HOPE, as opposed to business as usual, were 55 percent less likely to be arrested for a new crime, 72 percent less likely to use drugs, and 53 percent less likely to have their probation revoked than

those on regular probation. In a similarly promising related study, Kilmer et al. (2013) found that a swift-and-certain program in South Dakota targeted towards persistent alcohol-involved offenders appears to have had extraordinarily large effects in counties that received the program.

4.2 *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.[33]

There have 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 Zheng (2009), which finds evidence of short-run deterrence. Event studies such as those of Grogger (1991) and Hjalmarsson (2009a) examine the daily incidence of homicides before and after executions. Both Grogger (1991) and Hjalmarsson (2009a) find little evidence of deterrence effects though, as Charles and Durlauf (2013) and Hjalmarsson (2012) note, with a limited time horizon, it is not possible to distinguish between what we typically think of as deterrence and temporal displacement. A related study, Cochran, Chamlin, 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 it is attributed with equal ease 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 (2013), the literature is plagued by several conceptual problems that 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 occurrence of an execution does not per se change the expected severity of a criminal sanction for murder.[34] Indeed the research design is often motivated by the assumption that an execution affects an offender's perceived

[33] 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.

[34] An important exception to this general point can be found in Chen (2013), which studies the effect of executions for desertion among British soldiers during World War I and finds evidence that executions deter desertion, but actually encourage desertion when the execution was for an offense other than desertion or if the executed soldier was Irish. The reason 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.

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 US 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, the propensity to seek the death penalty in practice, and the application of the ultimate punishment (Chalfin, Haviland, and Raphael 2013). This literature has generated mixed findings with several prominent papers (e.g., Dezhbakhsh, Rubin, and Shepherd 2003; Mocan and Gittings 2003; Zimmerman 2004, 2006; and Dezhbakhsh 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; and Kovandzic, Vieraitis, and Paquette-Boots 2009) finding little evidence in favor of deterrence.[35]

While evidence in favor of deterrence is mixed, recent reviews by Donohue and Wolfers (2005, 2009) and Chalfin, Haviland, and Raphael (2013), 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 (Dezhbakhsh, Rubin, and Shepherd 2003; and Zimmerman 2004), failure to report standard errors that are robust to within-state dependence (Dezhbakhsh and Shepherd 2006 and Zimmerman 2009), and sensitivity of estimates to different conceptions of perceived execution risk (Mocan and Gittings 2003).[36] 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, Vieraitis, 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 effect of capital punishment and execution risk. The authors find no evidence of a deterrent effect.

4.3 *Sanction Nonlinearities*

An additional 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—generally either seventeen or eighteen years of age, depending

[35] The debate continues with recent responses to critiques by Donohue and Wolfers (2005) offered by Zimmerman (2009), Dezhbakhsh and Rubin (2011), and Mocan and Gittings (2010).

[36] While Mocan and Gittings (2010) provide an extensive summary of the robustness of results reported in Mocan and Gittings (2003), Chalfin, Haviland, and Raphael (2013) 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.

on the state. While offenders below a given state's age cutoff are adjudicated as juveniles and face relatively low sanctions risk, offenders who are just above the age of majority are adjudicated 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 assumed to be the result of deterrence.

The canonical paper in this literature is that of Lee and McCrary (forthcoming). Using data from Florida, Lee and McCrary document a sizable discontinuity in the probability that a young offender is sentenced to prison depending upon whether the arrest occurred 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), who estimated an elasticity for Italian adults. Findings in Lee and McCrary are perhaps surprising, but are supported by results reported in Hjalmarsson (2009b), 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, thus suggesting a mechanism underlying these small effects. The implication is that deterrence is not operational because perceptions do not match the incentives created by public policy.

Lee and McCrary's research design has now been replicated to varying degrees. Most recently, Hansen and Waddell (2014) study the effect of Oregon's age of majority on juvenile offending and report some evidence of a decline in crime upon reaching the age of majority for covered crimes. However, results that utilize an appropriately small bandwidth are not significant at conventional levels indicating, at best, weak evidence in favor of deterrence effects. Finally, in a reduced-form analysis using national-level data in the NLSY, Hjalmarsson (2009b) finds little evidence of deterrence around state-specific ages of majority using self-reported data on offending.

A final paper worth mentioning is that of Hjalmarsson (2009b), 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, Hjalmarsson reports that incarcerated juveniles have lower propensities to be reconvicted of a crime. This deterrent effect is also observed for older and more criminally experienced offenders. The differential findings in Hjalmarsson (2008), on the one hand, and Hjalmarsson (2009b) and Lee and McCrary (forthcoming), on the other hand can potentially be rationalized by the fact that while the latter studies considered the behavioral response to a general threat of punishment, the former study measures the behavioral response to actual punishment that has already been experienced.

On the whole, the RD literature around the age of criminal majority produces little evidence of deterrence among young offenders. The available evidence suggests that this may, in part, be due to a lack of awareness of the size of the sanctions discontinuity, leaving open the possibility that deterrence may be found if the discontinuity is "advertised" as in pulling-levers-type focused-deterrence strategies. A remaining issue concerns the focus of the literature on arrests, which are an imperfect proxy for offending. In particular, if police officers are less likely to arrest an individual just below the age of majority relative to an individual just above the age of majority for a given crime, the resulting RD estimates will be attenuated with respect

to the actual change in offending, which may well have been positive. While no direct evidence suggests that this type of officer behavior is widely employed, the concern is worth noting.

4.4 *Deterrence versus 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 (1999), which studies the effect of California Proposition 8, a 1982 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 Proposition 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. However, while the logic is, in general, persuasive, the validity of Kessler and Levitt's results have been called into question by Webster, Doob, and Zimring (2006), who argue that overall 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 a long prison sentence regardless of the specifics of a state's 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. Overall, we do not believe this literature offers any credible evidence of deterrence, though it is not clear that variation in capital-punishment regimes will ever be sufficiently random and that murder rates will ever be sufficiently dense to allow us to credibly detect a treatment effect.

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. Hence, 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 and Helland and Tabarrok 2007). Likewise, in Drago, Galbiati, and Vertova's study of Italy's clemency bill, prisoners who faced harsher sanctions upon being rearrested were less likely to be rearrested. On the other hand, studies of sanction nonlinearities in which offenders of slightly different ages receive differential treatment report little evidence of a large deterrence effect. Of course, 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. Likewise, evidence of deterrence is found when punishment severity faced by individual offenders is both extraordinarily severe and known. However, within the range of typical changes to sanctions in contemporary criminal-justice systems, the evidence suggests that the magnitude of deterrence owing to more severe sentencing is not large and is likely to be smaller than the magnitude of deterrence induced by changes in the certainty of capture. What is less well understood is the extent to which changing sentencing severity along the extensive margin induces deterrence. Since this increases the severity of punishment in the near rather than the distant future, one might think that deterrence effects will be more easily observed.

5. *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 opportunity cost of an offender's 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—for example, 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 the responsiveness of crime to wages.[37] 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. There is also a literature that examines the relationship between crime, unemployment, and wages using individual data. We discuss the implications of this literature for the study of deterrence in the final part of this section.

5.1 *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.[38] 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 research include Entorf and Spengler (2000) for Germany, Papps and Winkelmann (2000) for New Zealand, Machin and Meghir (2004) for the United Kingdom, Andresen (2013) for Canada, and Arvanites and Defina (2006), Ihlanfeldt (2007), Rosenfeld and Fornango (2007), and Phillips and Land (2012) for the United States. With the exception of Papps and Winkelmann (2000), each of these papers finds at least some evidence in favor of a link between unemployment and crime, in particular, property crime.

[37] 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.

[38] Nonetheless, Chiricos's 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, 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.

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 1979–98 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 1 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 1 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.[39]

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, a conclusion echoed in a recent review by Cook (2010). 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).[40] 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 in which unemployment increased by 3 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 4 percentage points.

5.2 *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

[39] Fougere, Kramarz, and Pouget (2009) provide a similar analysis for France, finding effects that are similar in magnitude.

[40] Prominent IV papers, including Raphael and Winter-Ebmer (2001); Gould, Weinberg, and Mustard (2002); and Lin (2008) do not uniformly find that instrumenting results in a more positive relationship between unemployment and crime as would be predicted by the omission of procyclical 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 ordinary least squares (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.

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 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 2002). 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, 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 panels for 1984–93 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 United Kingdom, Machin and Meghir (2004) examine changes in regional crime rates in relation to changes in the tenth and twenty-fifth percentiles of the region's 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 and Fernandez, Holman, and Pepper 2014).

5.3 *Individual-Level Studies*

In addition to aggregate-level studies that examine the impact of macroeconomic fluctuations on crime, there is a parallel literature that studies the effect of wage shifts and job loss—mainly job loss—over the life course. This literature uses natural variation to examine whether offending rises when individuals find themselves out of work. An early example of such research is that of Crutchfield and Pitchford (1997), who, using data from the 1979 NLSY79, find that the approximately 8,000 adults who responded to the first wave of the survey were more likely to engage in crime when they are out of the labor force and when they expect their current job to be of short duration. A host of similar cohort studies have found similar correlations among males in London, as well as individuals born in Philadelphia in 1945 (Thornberry and Christenson 1984 and Witte and Tauchen 1993). For several reasons, we do not believe this literature has great value in uncovering deterrence effects. First, even conditioning on fixed effects, individual-level models do not plausibly account for omitted variation that may be related to both unemployment and offending. In particular, a change in an

important unobserved factor may drive both spells out of work and criminal activity. For example, illegal drug use may simultaneously cause both an unemployment spell as well as participation in crime. Alternatively, other life stresses, problems with personal relationships, mental health problems, etc. may cause the simultaneous co-occurrence of unemployment (or underemployment) and criminal activity (Chalfin and Raphael 2011). To be sure, such issues of causal identification pose a challenge in all micro-level social science research using observational data. Nonetheless, absent a clear source of exogenous variation in employment status or employment prospects, one should probably consider these sorts of longitudinal estimates as providing an upper bound on the likely effect sizes.

A second individual-level literature is, in our view, more useful. This literature considers the impact of providing employment services, or, in some cases, transitional jobs to former prisoners—in particular assessing whether such programs reduce recidivism. The research is predominantly comprised of randomized experiments and is, as such, highly credible. Experimental interventions of this nature tend to include programs that provide income, employment-based services, or skills-building social services. There are over a dozen experimental evaluations of such efforts in the United States, in which treatment group members are randomly assigned. A key advantage of these studies is that the treatment is clearly exogenous, and, as such, any observed impacts plausibly represent true causal effects. However, the reader should be careful in interpreting the results of these programmatic interventions, as it is often the case that many members of the randomized control group receive similar services elsewhere. Often, it is difficult to document such contamination and it is not always self-evident that the intervention has a large marginal impact on service delivery. Second, since these interventions are targeted at particular groups with offense histories that cross fairly stringent severity levels (former prisoners, for example), they tend to treat individuals who may not be particularly responsive to positive incentives.

Community-based employment interventions became popular in the United States beginning in the 1970s. Under authority of the 1962 Manpower Development and Training Act, the US Department of Labor launched a number of programs aimed at former prisoners beginning with the Living Insurance for Ex-Prisoners program, which provided a living stipend and job-placement assistance to prisoners returning to Baltimore between 1972 and 1974, and the Transitional Aid Research Project (TARP), which provided various combinations of cash assistance and job-placement services to five different experimental groups of ex-offenders in Georgia and Texas. The earlier demonstration evaluated by Mallar and Thorton (1978) found significant impacts of the income-support program, with considerably lower offending rates among the treatment group. However, the evaluation of the larger-scale TARP program (Rossi, Berk, and Lenihan 1980) found little effect. The latter evaluation also found a large negative effect of the transitional cash assistance on the labor supply of released inmates. In fact, the authors speculate that the lack of an overall impact on recidivism reflected the offsetting effects of the reduction in recidivism due to the cash assistance and the increased criminal activity associated with being idle (Rossi, Berk, and Lenihan 1980).

There have also been several high-quality evaluations of the impact of providing transitional employment to former inmates. The National Supported Work (NSW) program (recently reanalyzed by Uggen 2000) and the New York Center for Employment Opportunities currently under evaluation by the Manpower Development Research

Corporation (MDRC) (Bloom et al. 2007) find some evidence that providing prison releases with transitional employment forestalls recidivism during the two years post-release. However, these programs found considerable heterogeneity in program impacts with the NSW, finding significant effects for older releases and the CEO evaluation reporting significant effects for only those most recently released from prison. Moreover, the majority of the literature reports little evidence of an effect of employment status on recidivism among reentering prisoners (Visher, Winterfield, and Coggeshall 2005). It may well be that employment deters crime among individuals without prison experience, a mechanism that plausibly underlies relationships between measures of macroeconomic performance and crime. However, among individuals with serious criminal records, the evidence of deterrence is difficult to find.

5.4 *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 unemployment rate in a crime regression necessarily identifies deterrence. In particular, for several reasons, crime and unemployment may be related due to factors other than deterrence. First, a relationship between macroeconomic conditions and crime may exist due to the relationship between macroeconomic conditions and criminal opportunities (Cook and Zarkin 1985). For example, during a recession, auto thefts tend to decline presumably because fewer people are employed and therefore drive their cars less frequently (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 develop feelings of anger or loss of control that subsequently manifest 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.

6. *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 and 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, a fact that may be attributable to the visibility of such policies.

Second, while the evidence in favor of a crime–sanction link generally favors relatively small deterrence effects, there does appear to be some evidence of more meaningful deterrence 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. Moreover, it is not clear that supplying employment deters offending among the most criminally productive individuals.

Overall, the evidence suggests that individuals respond to the incentives that are the most immediate and salient. While police and local labor-market conditions influence 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 the final section of this article, we return to Becker's economic model of crime in an attempt to rationalize the empirical literature with the theory that precipitated it. We close with a couple of concrete recommendations for future work.

6.1 *Rationalizing Theory and Empirics*

A natural starting place in reconciling the empirics with the theory is to consider that Becker's model does not explicitly predict that offending will be more responsive to changes in p as opposed to f. This is because while the model allows for varying degrees of risk aversion, there is no other mechanism within the model that can lead to a differential response of crime to p and f that we tend to observe in the empirical literature. However, using more recent theoretical work that builds upon Becker as a guide, we note that there are at least three strong theoretically motivated reasons to expect that crime will be more responsive to p than to f. This is not to say that the Becker model is incorrect—on the contrary, our reading of the empirical literature is that it provides support for the model. Instead, we prefer to characterize the model as a useful starting point for thinking about some of the more nuanced aspects of deterrence.

Perhaps the most enduring criticism of the original neoclassical model is that it is static and, accordingly, does not explicitly allow for individuals to differ in their time preferences, a fact that is inspired by a generation of behavioral economics research and was noted in early work by Block and Heineke (1975) and Cook (1980). Dynamic extensions of Becker can be found in Polinsky and Shavell (1999), Lee and McCrary (forthcoming), and McCrary (2011), among others. An abbreviated version of such a dynamic model was presented in section 1 of this article. The most important insight arising from the dynamic corollary of Becker is that individuals who are myopic and engage in hyperbolic discounting will be more strongly deterred by changes in p, which affect utility immediately, than by changes in f, which, for the most part, affect utility in the distant future. To the extent that offenders tend to be hyperbolic discounters and there is ample evidence that many are, we should not expect long sentences to deter to nearly the same degree as changes in the probability of either arrest or some type of punishment.

In recent years, the Becker model has also been augmented to allow for the fact

that sanctions do not necessarily follow from apprehension. Indeed, many offenders are arrested but do not suffer anything more than a trivial criminal sanction, either because charges are not filed or are dismissed or because a custodial sentence is not handed down or is considered already served at the time of sentencing. Likewise, behavioral scientists have incorporated the notion that individuals suffer both legal and nonlegal sanctions when they are arrested for committing a crime (Nagin and Paternoster 1994, Nagin and Pogarsky 2003, and Williams and Hawkins 1986). While the effect of legal sanctions has been well understood since Becker, the extent to which individuals suffer social stigma and other social costs as a result of an arrest remains less well understood. Nagin (2013) formalizes these related concepts within the framework of the Becker model, conceptualizing p as the product of the probability of apprehension, P_a, and the conditional probability of a sanction given apprehension, $P_{(S|a)}$. With these additions, it can be shown that if individuals are more sensitive to changes in the probability of apprehension than to changes in the sanction, it is easy to see that changes in utility (and thus crime) can potentially be explained by informal sanction costs alone (Nagin 2013). A related and arguably more important insight is that in the event that informal sanctions costs are very high, it is considerably more likely that deterrence will accrue via the probability of apprehension (p_a) than via f.

Related to this is the issue of heterogeneity in individual utility over the legal sanction. In the event that the stigma of a custodial sentence declines, the disutility derived from punishment will fall as a function of the length of an individual's criminal history (Durlauf and Nagin 2011 and Nagin, Cullen, and Jonson 2009). That is, as individuals accumulate a longer criminal record, the stigma from being labeled a "criminal" may lose its effectiveness and thus no longer represent an important component of the cost of committing a crime. Likewise, the disutility of punishment may decline due to the presence of informal social networks that develop as a result of an individual's prison experience. Hence, to the extent that a large proportion of crime is committed by repeat offenders, crime should be more sensitive to the probability of apprehension than to the severity of sanction, because repeat offenders have already paid the informal costs associated with being labeled a criminal.

A final extension that merits discussion can be found in Durlauf and Nagin (2011) who consider that, in addition to exhibiting strong time preferences, individuals may have a great deal of trouble accurately estimating the risk of apprehension even after updating in response to new information (Anwar and Loughran 2011). Durlauf and Nagin develop a model in which p is probabilistic and show that for a fixed sanction, when p is perceived to be arbitrarily close to either zero or one (i.e., the probability of apprehension is thought to be either extremely low or extremely high), the effect of certainty will be greatest. This follows from the tendency of individuals to systematically overestimate the likelihood of rare events (e.g., a terrorist attack) and underestimate the likelihood of more common events (e.g., a car accident). As Durlauf and Nagin note, in a world in which the perceived probability of detection is very low, even small changes in that perceived probability can have correspondingly large effects, thus potentially rationalizing large behavioral responses to hot-spots policing and deterrence advertising.

The implication that the response of crime to p might be systematically greater than to f has far-reaching implications with respect to public policy. First, given that deterrence is cheap relative to incapacitation, efficient resource allocation demands a shift in resources toward more deterrence-intensive

inputs. Hence, in deciding how to allocate criminal-justice resources between police officers and prisons, the available evidence suggests that money is best spent on police officers, as well as perhaps on jails that might be used to detain individuals who receive short sentences. Second, it may be possible to reduce the size of the US prison population, which has grown six-fold over the course of a generation, without compromising public safety. Such an idea is strongly supported by the small crime–prison population elasticities reported by Liedka, Piehl, and Useem (2006) and Johnson and Raphael (2012), as well as Lofstrom and Raphael's recent evaluation of the effects of California's realignment policy. It is also articulated anecdotally by the experience of New York State, which has both reduced its prison population as well as its crime rate in recent years.

These points are underscored in Kleiman (2009) and Durlauf and Nagin (2011), among others, who ask the provocative question as to whether both imprisonment and crime can be simultaneously reduced. The answer to this question lies in the magnitude of the relevant deterrence elasticities, the specifics of which are cleverly illustrated in Durlauf and Nagin, who ask us to consider a world in which there are two types of criminal opportunities: desirable opportunities with corresponding probability of arrest, p_0, and undesirable opportunities with probability of arrest, p_1. Initially, individuals who encounter a criminal opportunity that is assigned probabilistically elect to commit a crime if $p > p^*$, where $p^* > p_1 > p_0$. Hence, all available opportunities are acted upon. Durlauf and Nagin next ask us to consider that p_0 and p_1 are each increased by a factor g such that $gp_0 < p^* < gp_1$. In this world, only the desirable opportunities will be acted upon. The obvious result is that crime declines as all of the opportunities are no longer attractive. What is less obvious is that clearance rates fall by construction and, accordingly, so do the number of arrests and subsequent incarcerations. As shown in Blumstein and Nagin (1978), if the magnitude of either the elasticity of crime with respect to p or f is less than 1, then the decline in the crime rate associated with an increase in p and f will not be sufficiently large to avoid an increase in the incarceration rate. The key then is to determine whether there are policies that satisfy this inequality or, in the absence of such policies, identify policies that decrease severity but have large elasticities with respect to p. In our view, swift-and-certain sanctions regimes such as that motivated by HOPE and visible police presence are two such policies that seem especially promising.

6.2 *Directions for Future Research*

As each of the deterrence literatures has matured, researchers can begin to focus less attention on standard identification problems and more attention on identifying mechanisms. This is not to say that causal identification is unimportant. On the contrary—we believe it is as important as it ever was. However, in the past decade, great strides have been made in selecting strategies that credibly identify the causal effects of a variety of policies which has, in turn, generated a consensus on the range of effects that can be expected to accrue from a given type of intervention. For several literatures that have generated no such consensus (e.g., capital punishment), we strongly suspect that there is little progress to be made in rehashing the same state-level data. Accordingly, we provide three recommendations to guide future work.

First, there is a large and growing literature that supports the deterrence value of hot-spots policing and, in particular, the importance of a visible police presence. However, the evidence on the effects of disorder policing is more mixed and the idea remains highly controversial. The best

evidence suggests that cleaning up physical disorder is important, but it is not clear whether broken-windows policing is a necessary ingredient to this strategy. Given the inherent risks of broken-windows policing that accrue to both officers and citizens and the civil-rights concerns that are intrinsic in such a strategy, future research is needed to identify the extent to which broken windows reduces crime and, if so, whether those reductions are due to deterrence or incapacitation. There continues to be substantial disagreement on this topic and, while we are skeptical of the deterrence value of broken windows, our reading of the research literature is that we do not yet have sufficient information to make an informed recommendation.

Second, while evidence is building that swift-and-certain sanctions can deter offending at dramatically lower costs for both society and offenders, the idea requires additional testing. In particular, the conditions under which such programs work and the degree to which they are replicable and scalable remains unknown. It has been suggested that the success of such programs often depends on the existence of effective leaders and an unusual degree of cooperation among local policy makers. Moreover, swift-and-certain sanctioning only works if offending can be reliably detected in the first place.

A third area that, in our view, will benefit from greater research, concerns the deterrence value of investments in private precautions by potential victims. Evaluations of LoJack (Ayres and Levitt 1998) and business improvement districts (Cook and MacDonald 2011) establish that private investments can deter (after all, they cannot incapacitate). However, we know relatively little about the effects of other types of private behavior, such as investments in burglary alarm systems, the emergence of various smart phone apps that provide information, as well as other types of technology.

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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