2011 Empirical Studies of Judicial Systems.
Institutum Iurisprudentiae, Academia Sinica, Taiwan.
Book No. 17. Editor: Yun-chien Chang.

How Do We Decide How Long to Incarcerate? *

David S. Abrams * *

Suggested citation format:

Footnote: David S. Abrams, *How Do We Decide How Long to Incarcerate?*, *in* 2011 EMPIRICAL STUDIES OF JUDICIAL SYSTEMS ____ (Yun-chien Chang ed., 2013).

Reference: Abrams, David S. 2013. How Do We Decide How Long to Incarcerate?. Pp. __-__ in *2011 Empirical Studies of Judicial Systems*, edited by Yun-chien Chang. Taipei: Institutum Iurisprudentiae, Academia Sinica.

---

* Submission: Nov. 15, 2011; acceptance: Feb. 3, 2012.
Editorial assistants: Jung Chen, Kai-Chih Chang.

* * Ph.D. in Economics, Massachusetts Institute of Technology. Assistant Professor of Law, Business Economics and Public Policy, University of Pennsylvania Law School and the Wharton School. Email: dabrams@law.upenn.edu. Thanks to participants in The Second International Conference on Empirical Studies of Judicial Systems and two anonymous referees for helpful comments. Eric Hague and Kathy Qian provided excellent research assistance.

Contents

Abstract

In the last several decades in the United States there has been a concomitant rise in the rate of incarceration, as well as the study of the phenomenon. Recent financial pressures have led to a willingness to reexamine the motivations for incarceration. This chapter aims to inform the debate by using some of the most sophisticated analyses from the law and economics of crime on the first-order effects of incarceration: general deterrence, specific deterrence, and incapacitation. While early research on these topics was often poorly identified, recent work has estimated causal effects of incarceration on crime through each of these channels. I use my own research as well as that of others to go beyond the specific mechanisms and yield policy-relevant recommendations.

KEYWORDS: crime, incarceration, sentencing, costs of crime, costs of incarceration, sentence length, general deterrence, specific deterrence, incapacitation, recidivism, add-on gun laws, empirical analysis, law and economics, cost-benefit.

I.Introduction

With over 2.4 million Americans behind bars (as of 2008)[1], devising a more rational incarceration policy is of paramount importance for both crime control and economic reasons. The topic of depriving an individual of his freedom is one that arouses substantial moral intuitions and arguments. I take a different approach: an economic one. As with other important policy considerations, decisions about crime, punishment, and incarceration must be based on rational, fact-based studies and cannot be left to often ill-informed theorizing.

In this chapter, I build on some of my recent work and supplement this analysis with other research that shares one attribute: the careful analysis of empirical data sufficient to allow for causal inference. Unfortunately, the vast majority of criminal justice research lacks this character, and thus many studies yield unjustified or uncertain conclusions. Poor inputs lead to poor policy analysis, and thus until recently it was difficult to draw meaningful policy implications from academic research. This is not the first attempt to do so, but I hope it represents a more sophisticated attempt than has come before. It should certainly not be the last.

The goal is to use quantitative estimates of each of the principal effects of incarceration to provide guidance on when incarceration makes sense. Doing so requires the results of a number of recent studies on general deterrence, specific deterrence, incapacitation, and the cost of crime. There are certainly other important implications of incarceration

---

1 See the Bureau of Justice Statistics website for recent data on incarceration rates: U.S. Dep't Just., *Bureau of Justice Statistics*, OFFICE OF JUSTICE PROGRAMS, http://www.ojp.usdoj.gov/bjs/ (last visited Mar. 27, 2012).

that will not be accounted for, but I assume these to be the primary ones. I now explain why these are the mechanisms on which I focus in this chapter.

Incarceration (and other penalties) can reduce crime simply by its threatened use. This effect is called general deterrence by criminologists, and it has the advantage of potentially reducing crime without increasing costs at all (or even reducing them)— because crime reduction due to longer sentences is concomitant with a reduction (although increase in duration) of incarcerated individuals. There has been and continues to be much debate among economists and criminologists about the magnitude and even existence of general deterrence. I report some of the most recent findings from my own work and those of others that generally find a non-zero, but relatively small general deterrent effect.

Another pathway through which incarceration can impact crime is through its effect on those who experience it. Does a prison term make an individual less likely to commit crime, having been chastened by the experience? Or do individuals learn to be better criminals while behind bars? Knowing the answers to these questions is clearly essential to understanding the overall impact of incarceration. The net effect of the experience of incarceration on crime is called specific deterrence. It is very difficult to obtain a causal estimate of it, because this requires a situation approximating random assignment of sentences to offenders. In a recent paper, I use a natural experiment where offenders are randomly assigned to public defenders as a way to approximate the random assignment of sentences.

Incapacitation describes the simple, direct impact of incarceration on crime: the fact that offenders are unable to commit crimes (outside of

prison) during their period of incarceration. While simple to state, incapacitation is not simple to estimate, and must account for replacement effects (the fact that locking up a criminal means another may very well take his place).

Incarceration carries explicit costs, including direct financial costs of feeding, clothing, housing, and monitoring prisoners. I use the most recent estimates from the literature to estimate these costs. In addition, there are costs to prisoners themselves, in terms of lost wages as well as lost freedom. I build on work with Chris Rohlfs in which we estimate the magnitude of these factors using individual bail decisions.[2] There are also costs incurred by relatives and friends of offenders, some of which may be incorporated into the bail decision.

In order to balance the costs and benefits of incarceration, it is necessary to choose one unit to measure everything. Putting everything in dollar terms is both necessary and difficult. Putting a dollar figure on crime victimization or freedom is an inherently challenging exercise. Fortunately there have been some clever attempts to use individual choices as indicia of their valuations of these abstract quantities through the mechanism of revealed preference. I use work by Mark Cohen and others to estimate the cost of foregone crime via the various pathways of incarceration's impact.[3]

There are other ways in which incarceration may directly or indirectly affect crime and aggregate welfare. Reduced employment

---

2 *See* David S. Abrams & Chris Rohlfs, *Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment*, 49 ECON. INQUIRY 750 (2011).

3 *See* Mark A. Cohen & Alex R. Piquero, *New Evidence on the Monetary Value of Saving a High Risk Youth*, 25 J. QUANTITATIVE CRIMINOLOGY 25 (2009).

prospects for ex-inmates is one mechanism that will not be addressed in this chapter. While there is good evidence (see, e.g., Kling.[4]) that earnings are reduced, if one assumes efficient labor markets, this should not have an overall welfare effect. I will also not attempt to value retribution in this chapter. Very little is known about this empirically, although there is some recent experimental evidence (see, e.g., Fehr et al.[5]) that indicates that individuals are willing to pay in order to punish others. Additionally, rehabilitation will not be explicitly valued in this chapter, except as it is incorporated into specific deterrence.

Because incarceration is by far the most widely used punishment for serious crime in most countries, it is the only mechanism for reducing crime that I consider in this chapter. This is not to say that changes in expenditures on imprisonment are the most effective way to increase the efficiency of the criminal justice system. A number of recent papers suggest that increasing the probability, as opposed to the magnitude of deterrence, may be a more cost-effective way to decrease crime.[6] But since incarceration is likely to remain an important component of criminal justice expenditures for the foreseeable future, it is the focus of this chapter.

It is important to stress that this is one of the first attempts at a welfare analysis of incarceration and as such has a number of limitations.

---

4 *See* Jeffrey R. Kling, *Incarceration Length, Employment, and Earnings*, 96 AM. ECON. REV. 863 (2006).

5 Ernst Fehr & Urs Fischbacher, *Third Party Punishment and Social Norms*, 25 EVOLUTION & HUMAN BEHAVIOR 63 (2004).

6 *See* Jonathan Klick & Alexander Tabarrok, *Using Terror Alert Levels to Estimate the Effect of Police on Crime*, 48 J.L. & Econ. 267 (2005); Jonathan Klick & Alexander Tabarrok, *Police, Prisons, and Punishment: The Empirical Evidence on Crime Deterrence*, *in* HANDBOOK ON THE ECONOMICS OF CRIME 127, 127-44 (Bruce L. Benson & Paul R. Zimmerman eds., 2010).

The results are based on the studies I rely on, each of which has important limitations. In addition, there is a limit to how much I am able to account for individual characteristics (race, sex, criminal history, socioeconomic status), differences by jurisdiction, by time, or by type of crime, although of course these are important in policy-making. My hope is that this will set out the framework for estimating the value of incarceration, and that the estimates will be substantially improved as our understanding of the underlying phenomena increases.

The rest of the chapter is structured as follows. In Section II, I discuss research and estimates about general deterrence. Section III does the same for specific deterrence and Section IV for incapacitation. In Section V, I introduce the cost of crime estimates that will be used and present the main results, combining all of the aforementioned factors. I discuss the policy implications and limitations on the results in Section VI and conclude in Section VII.

II. General Deterrence

A. Background

There has been a great deal of research on general deterrence over the past several decades, but a lack of consensus remains. Recent literature reviews on general deterrence have mixed conclusions. Nagin (1998) finds evidence for an overall deterrent effect in the criminal justice system, but believes more work is needed to better establish that increased sentences deter crime.[7] Doob and Webster (2003) review a

7 *See* Daniel S. Nagin, *Criminal Deterrence Research at the Outset of the Twenty-First*

handful of papers by economists and a large number by criminologists and conclude that the lack of strong evidence for deterrence is widespread enough to conclude that there is a null effect.[8] These authors along with Frank Zimring (Webster, Doob, Zimring 2006) pay particular attention to Kessler and Levitt's 1999 paper, which they do not finding convincing.[9] Robinson and Darley (2004) take a more nuanced view that there are circumstances in which increased sentences may deter, although they argue that the magnitude is insufficient to influence policy decisions.[10] In the *Handbook of Law and Economics*, Levitt and Miles (2007) discuss several economic studies that find evidence for general deterrence, but conclude that more research on the topic is necessary.[11]

Ehrlich (1981) performs some of the most publicized empirical work on deterrence by an economist and focuses to a great extent on murder.[12] In the past fifteen years, greater attention has been paid to the careful identification of deterrence as well as the separation of deterrence and incapacitation effects, as in Levitt (1998a).[13] Other examples include an investigation into the effects of California's three

---

Century, 23 CRIME & JUST. 1.

8 *See* Anthony N. Doob & Cheryl M. Webster, *Sentence Severity and Crime: Accepting the Null Hypothesis*, 30 CRIME & JUST. 143 (2003).

9 *See* Cheryl M. Webster, Anthony N. Doob & Franklin E. Zimring, *Proposition 8 and Crime Rates in California: The Case of the Disappearing Deterrent*, 5 CRIM. & PUB. POL'Y 417 (2006).

10 *See* Paul H. Robinson & John M. Darley, *Does Criminal Law Deter? A Behavioural Science Investigation*, 24 OXFORD J. LEGAL STUD. 173 (2004).

11 *See* Steven D. Levitt & Thomas J. Miles, *Empirical Study of Criminal Punishment, in* 1 HANDBOOK OF LAW AND ECONOMICS 457 (A. Mitchell Polinsky & Steven Shavell eds., 2007).

12 *See* Isaac Ehrlich, *On the Usefulness of Controlling Individuals: An Economic Analysis of Rehabilitation, Incapacitation and Deterrence*, 71 AM. ECON. REV. 307 (1981).

13 *See* Steven D. Levitt, *Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?*, 36 ECON. INQUIRY 353 (1998).

strikes law by Helland and Tabarrok (2007). They find a decrease in arrests of around 20% among felons with two strikes.[14] An Italian natural experiment that resulted in individual-level variation in sentencing is investigated in Drago, Galbiati, and Vertova (2009).[15]

Finding discontinuities in sentences is one of the primary obstacles to obtaining good empirical estimates of general deterrence; sentencing changes at the age of majority are perhaps the most commonly used type of discontinuity. Interstate differences in the relative harshness of adult sanctions versus juveniles' are used in Levitt (1998b).[16] He finds that those states with larger jumps in punishment have larger decreases in adult crime relative to juvenile. Hjalmarsson (2009) examines offender perceptions of penalties and finds that they vary much less than actual changes at the age of majority. She also finds little evidence of deterrence in self-reported data.[17] Using high-frequency data from Florida, Lee and McCrary (2009) look for a discontinuity in offending around the eighteenth birthday.[18] They find a drop in crime of 2% around this discontinuity and suggest that part of the low response might be due to myopic behavior.

The approach I describe below uses a similar methodology to that

---

14 *See* Eric Helland & Alex Tabarrok, *Does Three Strikes Deter: A Non-Parametric Estimation*, 42 J. HUM. RESOURCES 309 (2007).

15 *See* Francesco Drago, Roberto Galbiati & Pietro Vertova, *The Deterrent Effects of Prison: Evidence from a Natural Experiment*, 117 J. POL. ECON. 257 (2009).

16 *See* Steven D. Levitt, *Juvenile Crime and Punishment*, 106 J. POL. ECON. 1156 (1998).

17 *See* Randi Hjalmarsson, *Crime and Expected Punishment: Changes in Perceptions at the Age of Criminal Majority*, 11 AM. L. & ECON. REV. 209 (2009).

18 *See* David S. Lee & Justin McCrary, *The Deterrence Effect of Prison: Dynamic Theory and Evidence* (Princeton U. . Ctr. for Econ. Policy Studies, Working Paper No. 189, 2009).

of Kessler and Levitt (1999) in which they make use of sentence enhancements in California enacted by the passage of Proposition 8 to estimate deterrence.[19] The authors use a difference-in-difference and triple difference approach, using variation by time, type of crime, and state to isolate the effect of the California law change. They also separate deterrence from incapacitation by examining changes in short-term crime rates for serious offenses with lengthy underlying sentences and find the mean three year deterrent effect to be 8%. Another paper close in subject matter to that described below is that of Marvell and Moody (1995) in which they examine the effect of gun laws on a range of outcomes.[20] Using a data set on firearm sentencing enhancements, prison population, and crime, the authors find little evidence for impact of firearm enhancements on either deterrence or incapacitation.

B. Methodology

Any convincing research on general deterrence has to approximate random variation of sentence length, in order to determine the causal effect on crime. Absent random variation, one is not able to distinguish a correlation between sentence length and crime (perhaps due to an unobservable variable) from a causal impact of sentence length. This is a common problem that constantly recurs in social science research.[21]

---

19 *See* Daniel P. Kessler & Steven D. Levitt, *Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation*, 42 J.L. & ECON. 343 (1999).

20 *See* Thomas B. Marvell & Carlisle E. Moody, *The Impact of Enhanced Prison Terms for Felonies Committed with Guns*, 33 CRIMINOLOGY 247 (1995).

21 In general, changes in prison populations are correlated with a number of variables and thus isolating the effect of a single causal mechanism is difficult. For example, states with higher prison populations tend to have higher crime. This does not mean that incarceration causes crime, but rather a third factor (an "omitted variable", for example a bad economy) likely causes both. In order to obtain a clear causal effect of the variable of interest (here sentence length) it is necessary to employ a research

General deterrence is even more difficult to gauge because increased sentences may impact crime both through general deterrence and through increased incapacitation. Several recent studies (mentioned above) take pains to distinguish these effects, using California ballot measures, changes in sentence length at the age of majority, and a natural experiment in Italian prisons in order to do so. I now describe the approach I make use of in a recent paper[22] to gain a causal estimate of general deterrence.

Add-on gun laws result in increased sentences for defendants convicted of possessing of a firearm while committing a crime. These laws grew popular in the United States in the 1970s and 80s, with thirty states adopting one of these laws by 1996 (Vernick and Hepburn 2003).[23] One may use add-on gun laws as a unique set of natural experiments in order to distinguish the deterrent effect of incarceration from the incapacitative effect. The key to the approach is the fact that add-on laws apply only to defendants who would otherwise receive sentences of incarceration. Thus the short-term impact of an add-on gun law should be purely deterrent.

Consider the change in the gun robbery rate between the month before and the month after an add-on gun law goes into effect, in a jurisdiction where robbery carries a sentence of six years and the add-on

---

design that approximates a randomized experiment. That is what most of the papers discussed here attempt to do.

22 *See* David S. Abrams, *Estimating the Deterrent Effect of Incarceration using Sentencing Enhancements* 4 American Economic Journal: Applied Economics (2012) 32-56.

23 *See* Jon S. Vernick & Lisa M. Hepburn, *State and Federal Gun Laws: Trends for 1970–99*, *in* EVALUATING GUN POLICY: EFFECTS ON CRIME AND VIOLENCE 345 (Jens Ludwig ed., 2003).

is two years. After the law change, criminals convicted of gun robbery will now receive sentences of eight years instead of six. If there is no deterrent effect of the add-on law, there will be no difference in the number of defendants newly incarcerated the month before the introduction of the add-on law and the month after. If there is a change in the number of gun robberies in the month after the law's introduction, it cannot be due to a change in incapacitation, and the change may be attributed to the deterrent effect of the increased sentence length.

Several characteristics of add-on gun laws make them ideal for isolating the deterrent effect of incarceration. First, add-on laws were adopted in many different states, yielding many separate experiments for the analysis. Thus, Abrams (2012) is able to expand on previous studies of deterrence, which usually focus on a single state (see, e.g., Loftin and McDowall, 1989; Kessler and Levitt, 1999).[24] Second, the laws are generally only applied in cases in which the underlying crime would merit a sentence of at least several years. This allows a good amount of time to detect the deterrent effect. Finally, add-on laws were adopted over a long time period (over forty years), during which there was substantial variation in crime rates. The temporal and cross-sectional variation in adoption of add-on laws makes it possible to control for time trends and state fixed effects.

C. Empirical Estimates

Using hand-cleaned Uniform Crime Reports (UCR) data, newly collected data on add-on gun laws, as well as that from the literature,

---

24 *See* Colin Loftin & David McDowall, *The Deterrent Effects of the Florida Felony Firearm Law*, 75 J. CRIM. L. & CRIMINOLOGY 250 (1984); Kessler & Levitt, *supra* note 19.

estimates of the deterrent effect of increased sentences are reported in Abrams (2012). The main empirical results are from a regression of reported log gun robberies per capita on post add-on law dummies. Table 3.1 reproduces these results, with each column representing a separate regression. The log specification is preferred because it counts equivalent relative declines in per capita gun robberies equally. "Balanced panel" indicates that data points were included only if they were within seven years prior to, or six years after the effective date for an add-on law. This is the maximum range of data that is available for all states that passed add-on laws. In half of the specifications the data is restricted to years after 1974 because there is a discontinuity in several variables in a large number of agencies in 1975 in the UCR data. Panels A, B, and C differ in the number of years included in coding the post add-on dummy. For example, in panel B, the add-on law dummy is one for the first two years following the add-on law effective date and zero otherwise. All errors reported allow for intrastate correlation and are weighted by state population. All specifications included state and year effects and the controls for poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population.

Log per capita crime rate is the preferred dependent variable in Abrams (2012) and this preference may be illustrated by the following example. Assume Albany has a pre-gun law level of 200 gun robberies per 100,000 residents and Seattle has a pre-gun law level of 100 gun robberies per 100,000 residents. We might believe that the severity of the impact of a marginal crime decreases with level of crime, so a reduction from 100 to 50 gun robberies per 100,000 residents is more meaningful than one from 200 to 150 per 100,000 residents. If this belief about

social preferences is accurate, it is appropriate to focus on the logarithm of the per capita rate of gun robberies as the outcome of interest.

Across specifications there appears to be a consistent finding that gun robbery rates decline after add-on gun laws go into effect. The impact is insignificant in the first year, but is significant at the 1% level after two or three years. The coefficients in Table 3.1 provide an estimate of the magnitude of the impact. Although the coefficients vary somewhat across specifications, there is a decline of 6–14% within the first two years and 5–18% within the first three years of introduction of the law. In the preferred specification (column 8), which is the most conservative, with a balanced panel restricted to post-1974 data and including state-specific time trends, there is an impact which seems to level off to 6% within two years, and 5% within three years. Note that the addition of state-specific time trends does not affect the coefficient substantially. This provides some support for the notion that the timing of add-on gun laws is exogenous.

In order to gain more information on the timing of the impact of the law change, Abrams (2012) estimates a regression of log per-capita gun robberies on dummy variables for year relative to the date of passage of the law change in a particular state. The results, reported in Figure 3.1, support the findings discussed above. Gun robberies rates (both with and without controlling for state trends) are fairly stable in the years preceding implementation of an add-on gun law, then decline for approximately two years and then level out.

The evidence from UCR data on gun robberies supports the notion that criminals are deterred by the implementation of add-on gun laws. There are a number of important confounds that could be belie this

interpretation, and they are addressed at length in Abrams (2012). But it is important to take note of the strength of the evidence presented here. By using panel data with state and time fixed effects, the attempt is made to rule out that spurious results could be obtained due to an overall national time trend in crime, or cross-sectional endogeneity in passage of add-on gun laws. Adding state trends increases the strength of the exogeneity assumption by ruling out endogenous response in law passage not just to levels, but also to state trends in crime. Making use of timing dummies relative to the law effective date allows for the detection of the dynamic response of crime relative to implementation of the law.

Other papers use sentencing changes that are substantially different from those in this chapter, and so a comparison of elasticities is more illuminating. A quick back-of-the-envelope calculation yields an elasticity of approximately –.10 in Abrams (2012).[25] This magnitude is consistent with that found by Lee and McCrary (2009).[26] They bound allowable elasticities consistent with their data and model to have a magnitude no greater than –.13, although their preferred parameter values yield elasticities close to 0. The largest recent empirical elasticity estimates come from Drago, Galbiati, and Vertova (2009) using Italian data, where they find a magnitude of –.74 for seven months.[27] This may be an indication that the substantially lower incarceration rate in Italy makes it difficult to extrapolate to the United States. A back-of-the-envelope calculation using Helland and Tabarrok's (2007) results from examining three-strikes-induced change yields an elasticity around –

25 This elasticity is calculated by simply dividing the percentage change in crime (about -5%) by the average percentage change in sentence length (about 50%) due to the add-on gun laws.

26 *See* Lee & McCrary, *supra* note 18.

27 *See* Drago, Galbiati & Vertova, *supra* note 15.

.07.[28]

III. Specific Deterrence

A. Background

Another important way in which incarceration may impact crime is through the effect of incarceration on subsequent criminal behavior. A number of studies of specific deterrence have been made, but a large fraction suffer from omitted variables bias. For that reason, I focus on recent studies that make use of techniques that allow them to obtain causal estimates of the impact of incarceration on subsequent recidivating behavior.

Recent reviews of work on specific deterrence and recidivism (Nagin, Cullen and Jonson 2009; Bushway and Paternoster 2009) both point out the wide spread of findings in the field.[29] One of the reasons this is the case is that authors are frequently investigating slightly different questions. For example, one may investigate the extensive margin (the experience of incarceration at all) or the intensive margin (the length of incarceration). In either case, one may examine jail or prison, first-time offenders or recidivists. Whether examining either margin, obtaining some kind of randomization in the likelihood and length of incarceration is difficult. Once these definitions are clarified,

---

28 *See* Helland & Tabarrok, *supra* note 14.

29 *See* Daniel S. Nagin, Francis T. Cullen & Cheryl L. Jonson, *Imprisonment and Reoffending*, 38 CRIME & JUST. 115 (2009); Shawn Bushway & Raymond Paternoster, *The Impact of Prison on Crime*, *in* DO PRISONS MAKE US SAFER? THE BENEFITS AND COSTS OF THE PRISON BOOM 119 (Steven Raphael & Michael A. Stoll eds., 2009).

and identification obtained, there is still the matter of defining and measuring recidivism, a subject about which entire books have been written (Maltz, 2001).

Perhaps the best study on the subject is one of the earliest, a field experiment carried out in California in the 1970s (Berecochea and Jaman 1981). The California Department of Corrections chose a treatment group of prisoners to be released six months earlier than the control group. They followed the two groups for two years after the final release dates and compared recidivism rates. While the paper reports no significant difference in recidivism, in fact a simple t-test on the data shows that the early-release group has a significantly higher recidivism rate.

Kuziemko (2007) also looks at an early release of prisoners, in this case from a Georgia prison in 1981. That release was not part of a field experiment, so there is not a natural control group. Instead of a simple comparison of recidivism rates, Kuziemko uses variation in when prisoners' sentences began as an instrument for the amount of time ultimately served. She finds that an extra month in prison yields a 7% decline in recidivism, a substantial effect.

In 2006, Italy introduced a dramatic policy change where all inmates received a sentence commutation of up to three years. As part of the policy change, prisoners with longer commutations also receive greater penalties if they are re-arrested. Drago, Galbiati, and Vertova (2009) study this policy change and find a substantial impact on re-arrest rates.[30] They find that prisoners that receive longer commutations have

30 *See* Drago, Galbiati & Vertova, *supra* note 15.

larger drops in recidivism.

While the experiment is significant, and although they find substantial changes in re-arrest rates, the interpretation is difficult. Because prisoners both have sentences shortened and penalties lengthened, the effect they estimate is a combination of specific and general deterrence. If, as some of the other studies discussed here suggest, the impact of specific deterrence is negative (less crime after longer incarceration) then general deterrence must dominate in the Italian experiment since defendants who received greater commutations had larger drops in recidivism. Another recent paper using a European natural experiment (Maurin and Ouss 2010) uses the 1996 Bastille Day pardon in France to estimate the effect of sentence reductions on recidivism.[31] They find that a greater sentence reduction via the pardon leads to an increase in expected future recidivism, consistent with a number of the other studies.

Turning back to American data, Hjalmarsson (2009) uses a regression discontinuity approach made possible by the Washington State juvenile sentencing guidelines to investigate the extensive margin of incarceration on juveniles.[32] She finds that juveniles who are incarcerated have a lower probability of recidivism than those who receive a local (non-incarceration) sanction.

A Maryland law change in 2001 is the natural experiment that Bushway and Owens (2010) exploit to examine how perception of

---

31 *See* Eric Maurin & Aurelie Ouss, *Sentence Reductions and Recidivism: Lessons from the Bastille Day Quasi Experiment* (IZA, Discussion Paper No. 3990, 2009), *available at* http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1351154.

32 *See* Hjalmarsson, *supra* note 17.

sentence length impacts recidivism. Their paper is not directly comparable to other papers on specific deterrence, because they propose a behavioral response of prisoners in which the frame of the sentence has an important impact on recidivism.[33] They find that shorter sentences relative to a reference point lead to higher recidivism rates.

In the next two sections I discuss recent work by this author (Abrams 2013) investigating specific deterrence using an instrument for sentence served. There are several recent papers that use a similar IV approach to the question, three of which use instruments based on judicial identity.[34] Loeffler (2013) uses judge-specific sentencing tendencies and random case assignment to obtain exogenous variation in sentence length.[35] This study, using data from Essex County, New Jersey, focuses on the extensive margin and does not find a strong relationship between incarceration and the likelihood of recidivism.

Berube and Green (2007) and Green and Winik (2010) use a very similar approach to that used by Loeffler.[36] Both Green co-authored papers both make use of data from the DC superior court and both find

---

33 *See* Shawn D. Bushway & Emily G. Owens, *Framing Punishment: Incarceration, Recommended Sentences, and Recidivism* (Working Paper, 2012), *available at* http://www.human.cornell.edu/pam/people/upload/Framing-JLawEc-Final.pdf..

34 See David S. Abrams, *Building Criminal Capital vs. Specific Deterrence: The Effect of Incarceration Length on Recidivism* (Working Paper, 2013), *available at* https://www.law.upenn.edu/cf/faculty/dabrams/workingpapers/AbramsRecidivism.pdf

35 *See* Charles Loeffler, *Does Imprisonment Alter the Life Course? Evidence on Crime and Employment from a Natural Experiment*, 51 CRIMINOLOGY 137 (2013).

36 *See* Danton A. Berube & Donald P. Green, *The Effects of Sentencing on Recidivism: Results from a Natural Experiment* (2nd Annual Conference on Empirical Legal Studies Paper, 2007), *available at* http://ssrn.com/abstract=999445; Donald P. Green & Daniel Winik, *Using Random Judge Assignments to Estimate the Effects of Incarceration and Probation on Recidivism among Drug Offenders*, 48 CRIMINOLOGY 357 (2010).

little relationship between sentence length and recidivism. They find a negative and statistically significant relationship between incarceration length and recidivism when doing ordinary least squares regressions. But the sign becomes positive and the effect statistically insignificant when 2SLS or LIML estimation is performed (although the standard errors are larger).

The closest approach to that discussed below is found in Turner (2009).[37] Using the Clark County data set used here, Turner instruments for sentence length using random assignment to attorney and heterogeneity in attorney performance. She finds a mostly insignificant relationship between sentence length and recidivism, although one that varies somewhat by the recidivism window.

B. Methodology

As discussed above, much early research on specific deterrence was plagued by unobservables that could potentially confound their findings. The studies mentioned above use several different approaches to address this problem. Here, I report a recent study in which I use a new approach to estimating the effect of sentencing length on recidivism. It is the first study that has the advantage of being fully non-parametric and allowing for a heterogeneous deterrent effect as a function of sentence length.

In a previous paper (Abrams and Yoon, 2007) I investigated the impact of individual attorneys on case outcomes using data from the Public Defender's Office of Clark County (Las Vegas), Nevada.[38] One

37 *See* Emily Turner, *Using Random Case Assignment and Heterogeneity in Attorney Ability to Examine the Relationship Between Sentence Length and Recidivism* (2009) (unpublished dissertation, University of Pennsylvania).

38 *See* David S. Abrams & Albert H. Yoon, *The Luck of the Draw: Using Random Case*

of the central findings of this paper is that there is substantial heterogeneity in attorney ability. Because public defenders are randomly assigned to first-time offenders and there is substantial heterogeneity in expected sentence by attorney, I use attorney assignment as an instrument for sentence length. Based on attorney assignment, I then estimate the impact of time served on recidivism.[39]

One challenge present in this context is that the relationships investigated are complicated and may not be well-suited to a single coefficient as in an OLS or probit regression. I estimate both OLS and IV regressions, using different assumptions about the sentencing distribution. Depending on the model for the sentencing distribution, I find either a negative relationship or, more frequently, no significant relationship between recidivism and sentence length. The relationship between the variables is not always monotonic, and I further explore it through non-parametric techniques.

C. Empirical Estimates

Table 3.2 presents the main regression results from the IV strategy, although, as I will discuss shortly, linear regression tells a very incomplete story in this context. Using an IV approach is important in this context to avoid the unobserved variables problem since unobserved criminal propensity is likely to bias the coefficient on sentence upward. This is borne out by examining the results reported in column 2 of Table 3.2, where the coefficient on sentence length is almost twice the

---

Assignment to Investigate Attorney Ability, 74 U. CHI. L. REV. 1145 (2007).

39 Recidivism is defined here as reappearance in the public defender data set. This means that at a minimum offenders have been rearrested and charges have not been immediately dropped.

magnitude as that in the OLS regression, where the first stage is linear. The coefficient is little-changed when adjusting for the recidivism-age profile.

In the IV regressions, I find a statistically insignificant relationship between recidivism and sentence length. The relationship is actually more complicated: it is negative for relatively low sentence lengths and a flat relationship thereafter. The IV results are similar when any incarceration is the endogenous independent variable. Again, there appears to be a declining relationship for low probabilities of incarceration that flattens out as probability of incarceration exceeds about 40%.

Because of a potential weak instruments concern, I also perform a LIML IV regression, which should be consistent even with weak instruments. The point estimate from this specification is negative, but smaller in magnitude and statistically insignificant. An examination of two- or three-year recidivism finds negative coefficients, ones that are larger in magnitude than OLS (not reported here), and statistically indistinguishable from zero. Offender age is significantly negatively related to recidivism while black and male offenders have higher recidivism rates.

Another approach is to use a Tobit model for the first stage, which leads to somewhat distinct results, but ones that are consistent with the OLS first stage results. The coefficient on sentence length is more positive for each of the three time windows examined, and all are still statistically insignificant. An examination of Figures 3.2 and 3.3 provides the visual analog of these regressions. While the IV approach is key to getting an unbiased measure of specific deterrence, the

coefficients are not meaningful if the relationship between deterrence is not linear, as has been assumed thus far.

Besides examining point estimates, it is illuminating to make a visual inspection of the relationship between recidivism and sentence length. Figures 3.2 and 3.3 show a complicated relationship between recidivism and sentence length, one that is not well-captured in a monotonic regression as reported in Table 3.2. The relationship between recidivism and sentence length is generally negative, but does not appear to be very linear (this may explain the insignificant regression results). The evidence seems to suggest that increasing sentences may reduce recidivism for short sentence lengths, but that the effect rapidly diminishes.

In addition to the impact of a longer sentence on recidivism, I also investigated the extensive margin: the effect of a change in likelihood, rather than length, of incarceration on future recidivism. The results are reported in Table 3.3 and Figure 3.4. The OLS results here also appear to be upward-biased, as with the intensive margin. This likely results from the fact that judges sentence individuals that appear more prone to commit crime based on characteristics unobservable to the researcher to incarceration at a higher rate. The point estimates for all time ranges are positive and statistically significant for OLS, but not for IV. It is possible that there could be a substantial positive impact of incarceration on recidivism of up to 10 percentage points, but due to the large standard errors in the IV specifications it is not possible to distinguish an effect of this magnitude from a null effect.

IV. Incapacitation

My estimates of the effects on incapacitation are taken not from my own research, but from several recent papers that produce credible estimates of the crime-reduction effect of incapacitating the marginal offender. I review some of these papers and then use their estimates in calculations below.

There are several important challenges inherent in incapacitation research. First, the relationship between incapacitation length and crime is a complicated one, with the causal arrow almost certainly going in both directions. This has led to two main types of attempted solutions: structural modeling of the relationship and the use of instrumental variables to break the simultaneity. The aim of either approach is to find a causal estimate of the elasticity of crime with respect to incarceration rate (usually denoted ε or η) and/or the distribution of offenses averted due to incapacitation (λ in the criminology literature).

Levitt (1998a) attempts to separate incapacitation from deterrence by comparing the impact of arrest rates for one crime on crime rates for that crime with crime rates for other crimes.[40] While there are endogeneity concerns with this approach, Levitt finds a larger impact of deterrence than incapacitation. His incapacitation estimates suggest that an additional person-year of incapacitation reduces prevents 5.1 to 8.2 index crimes, depending on category. He notes that these numbers are somewhat lower than self-reports of twelve to fifteen estimated crimes per year. This disparity could be due to multiple offender commission of single crimes. Offender reports also don't account for "replacement" effects—in other words, that the foregone crimes not committed by incarcerated offenders are likely to be at least partially replaced by non-

40 *See* Levitt, *supra* note 13.

incarcerated individuals.

Some of the best recent work on incapacitation was published in an issue of the Journal of Quantitative Criminology (JQC) organized by Shawn Bushway and Peter Reuter (Reuter and Bushway, 2007). The papers in this issue both summarize much previous work from both the economic and criminology perspective, and add some new estimates. Piquero and Blumstein (2007) surveys much of the recent literature and reports on estimates of elasticity of crime with respect to incarceration:

> "Estimates of the crime-reduction potential of incapacitation are both numerous and diverse, reflecting different assumptions made by different researchers (Cohen 1983; Visher 1987). Most incapacitation studies suggest that prison exerts a significant suppression effect on crime; however, the estimated effects vary markedly from study to study depending on the extent of incapacitation, how much the prison population is increased, and the values of the model parameters used (Spelman 2000a; Weatherburn et al. 2006). In an early review, Cohen (1978, pp. 219–221) estimated elasticities of –0.05 to –0.70, while Spelman's (1994, p. 220) estimates ranged from –0.12 to –0.20, with a best guess estimate of –0.16. DiIulio and Piehl (1991), using Wisconsin prisoner data from 1989, report an elasticity estimate of –0.22, while Piehl and DiIulio's (1995) analysis of New Jersey offender data indicates an elasticity estimate of -0.26. Likewise, Weatherburn and colleagues (2006) carried out an analysis of burglary offenders from New South Wales and obtained an adjusted elasticity of burglary with respect to imprisonment of –0.3. These various estimates cover a reasonable range of elasticity of about –0.2, or a requirement for a 5%

> increase in the prison population to achieve a 1% reduction in crime."

Piquero includes a number of studies that do not overcome the endogenous relationship between incarceration rates and crime. Still, the range of elasticities (–0.3 to –0.2) estimated in these studies are not far off from others that attempt to more explicitly deal with this problem. I summarize the elasticity estimates from the most reliable studies in Table 3.4.

Bhati (2007) in the same issue of JQC, uses a structural model of the offending process to estimate the incapacitation effect. His information-theoretic model is estimated using data from about a dozen state prisons on inmates released in 1994. He finds mean crimes averted in line with other studies. For property crimes, an average of 8.5 are averted per year of incarceration; the mean is 1.9 for crimes against persons. Bhati's elasticity estimates are substantially larger in magnitude than those of much other work, with a mean of –0.93 for crimes against persons and –0.69 for property crimes.

Owens (2009) uses a natural experiment to attempt to estimate the magnitude of the incapacitation effect. A 2001 law change in Maryland reduced sentences for young offenders with juvenile records. She is thus able to gain a causal estimate of incapacitation on crime for this group. She finds a comparatively low impact of the foregone incarceration: about 1.5 crimes per year, a number several times lower than other estimates. This may be because the affected group is a particularly low-offending one.

Most recently, Johnson and Raphael (2011) use the fact that incarceration rates adjust to permanent shocks with a dynamic lag in

order to estimate incapacitation effects. They find elasticities of between –0.06 and –0.11 for violent crime and between –0.15 and –0.21 for property crime. Moreover, they present evidence that the magnitude of the elasticities has declined in more recent years.

V. Analysis and Results

Previous work (Donohue 2009; DiIulio and Piehl 1991) has focused on answering the hypothetical question "what would be the effect of incarcerating an additional individual?"[41] This omits the mechanism by which the additional incarceration is achieved. The potential mechanisms are numerous, including increased sentencing standards, policing, detection rates, prosecution rates, or reduced parole rates, among other possibilities. The mechanism by which incarceration is increased will almost certainly affect its overall impact and welfare effects.

This chapter attempts to determine the impact of a very specific policy change: a marginal increase in sentence lengths. As discussed above, the first-order effects of sentence length increase will likely accrue due to general and specific deterrence and incapacitation. These effects can be quantified using cost of crime estimates and weighed against direct costs of incarceration. I now proceed with this calculation.

The equation for the total elasticity of crime is:

---

41 *See* John Donohue, *Assessing the Relative Benefits of Incarceration: The Overall Change Over the Previous Decades and the Benefits on the Margin*, *in* DO PRISIONS MAKE US SAFER? THE BENEFITS AND COSTS OF THE PRISON BOOM 269 (Steven Raphael & Michael Stoll eds., 2009).

$$\eta_{crime} = \eta_{GD} + \int_{n}^{life} \eta_{SD}\, f(sent) dsent + \eta_{Inc} \quad (1)$$

where η_{GD} is the elasticity of crime with respect to sentence length due to general deterrence, η_{SD} is the elasticity of crime with respect to sentence length due to specific deterrence, and η_{Inc} is the incapacitation elasticity. This equation assumes negligible discounting which allows crime-reducing effects that will be realized at different times (like specific deterrence and incapacitation) to be added directly. It would be simple to incorporate a discount factor into the equation.

The net annual benefit (or cost) of a doubling of sentence lengths will be:

$$\text{Net Benefit} = \eta_{crime} * crime * c_{crime} - c_{prison} * prisoners \quad (2)$$

Where c_{crime} is the cost of the mean crime, c_{prison} is the cost of a year of prison, *crime* is the number of crimes committed annually, and *prisoners* is the number of offenders incarcerated.[42]

I have discussed appropriate estimates for some of these parameters above. For η_{GD} I will use –.10 estimated in Abrams (2012) and consistent with a number of other recent studies. Estimates of incapacitation elasticities were discussed above and summarized in Table 3.4. There is substantial variation in these estimates, but there seems to be a growing view that some of the more consistent estimates are toward the lower end, between –.05 and –.30 (Donohue 2009). I will focus on this range and examine the impact of allowing some variation.

The recidivism study discussed above is somewhat difficult to directly relate to an elasticity. Fortunately, it is not necessary to calculate

42 Assuming (counterfactually) a stationary distribution of sentence lengths.

this directly, since what is desired is actually the number of foregone crimes. Using the empirical recidivism crime profile (Fig. 3) I estimate the increase in recidivism from a 1% sentence length increase. Using the sentencing distribution, *f(sent*), from the same dataset, I can compute the number of expected additional crimes via the specific deterrence pathway. I find a 1% increase in sentence length should lead to a decrease in recidivism of about .1 percentage points.

Thus I now have a total impact on crime from a 1% increase in sentence length, a range from –.25% to –.50% depending on the value used for the incapacitation elasticity. All that remains is to use this information in the cost benefit analysis (Equation 2). For this, a few more parameters are necessary. For *crime* I use the 2005 level adjusted for underreporting of 28,892,802 as in Donohue (2009). Mean sentence length is derived from National Judicial Reporting Program data (Rosenmerkel et al., 2010) and found to be 37.2 months for all convicted felons sentenced to a jail or prison sentence in 2006.

Since much of the cost of crime may be indirect (e.g., crime prevention, behavioral response) or intangible (e.g., emotional distress) estimating its cost is quite challenging. Several approaches have been used, including: survey data (contingent valuation), jury awards, and hedonic pricing. The last of these is in many ways most appealing, because it is the only one that relies on actual expenditure decisions by individuals. I use cost estimates from Cohen and Piquero (reproduced in Table 3.5) that derive from both contingent valuation (“bottom up”) and hedonic pricing (“WTP”) approaches. These build on earlier work by Cohen and coauthors, including the most comprehensive “bottom up” study to date, reported in Miller, Cohen and Wiersema (1996).

The final parameter necessary is the cost of incarceration. Ideally this should include both direct costs of feeding, housing, and monitoring prisoners, as well as indirect costs like the value of lost freedom, foregone wages, impact on families, and capital costs. I estimate direct annual imprisonment costs at $25,000, which is in line with data from Stephen (2004), Hill and Harrison (2004), and Bierie (2007). In addition, I use the value of freedom calculated in Abrams and Rohlfs (2011) adjusted for the higher overall employment rate in the general offender population, relative to the sample they used (Cohen and Piquero 2009). This augments the cost of imprisonment by about $10,600 and should incorporate some of the impact on families and friends as well as foregone wages and lost freedom. I add $3,000 for capital costs, as in Donohue (2009), for a total incarceration cost of $38,600 per prisoner per year.

I now combine the various parameters to yield an aggregate social benefit/cost under these different assumptions. The results are reported in Table 3.6. As with other attempts at similar calculations, the results are quite sensitive to crime cost and also vary substantially as a function of elasticity. I discuss implications and limitations of these results below.

VI. Policy Implications and Discussion

The exercise above yields a wide range of potential costs and benefits from a 1% increase in all felony incarceration sentences. At the low end, assuming a total elasticity of 0.25 and a low cost of the average crime, the costs of the sentence increase would exceed the benefits by over $900 million dollars. At the other extreme, using the high cost of

crime estimate and the high range of the elasticity estimates implies the sentence increase would yield a net benefit of almost $2.5 billion.

Given the sensitivity to the cost of crime, what is a policy-maker to take from these results? First, there is the obvious implication that greater research on costs of crime is necessary in order to narrow this very wide range of estimates. If any sort of rational criminal justice policy is to be possible, it will require more precise estimates of how much an averted crime is worth.

The cost of crime is a hugely understudied area by economists, which have tended to focus more on estimates of deterrence. This is not wholly surprising, since many recent empirical advances have to do with carefully drawn causal inference. Methodologies to measure the cost of crime, like contingent valuation or using jury awards, have not advanced as much in the past two decades. The cost-benefit calculation makes it clear just how important it is to have more precise costs of crime. My hope is that this chapter will spur more research on this topic.

But even though the policy implications are ambiguous when considered in the aggregate, there is something more than can be derived from this work. Thus far, I have ignored variation by type of crime, location, criminal background and other sources of variation. In reality, criminal justice policies correctly vary according to all of these factors, as they should. It is a fool's errand to attempt a single unified incarceration policy for all crimes, criminals, and locales. Nevertheless, this study can help inform the realistically tailored incarceration policies.

For a given elasticity, one can calculate the cost of crime where costs of increased incarceration exactly balance the benefits, the break-even point. That is, where

$$\eta_{crime} * crime * c_{crime} = c_{prison} * prisoners$$

I solve this for c_{crime} using my preferred elasticity estimate of 0.35, implying an elasticity of about 0.15 for incapacitation. The break-even point under these assumptions is $13,400 per crime; assuming an overall elasticity of 0.30 yields a break-even of $15,600.

Now the break-even point can be used as a concrete criterion for considering changes in sentence lengths for particular crimes. For example, larceny or drug possession almost certainly impose a societal cost of less than these amounts. Of course, they also (generally) currently carry lower sentences than the mean, so the calculation would need to be adjusted for this as well. But there will almost certainly be examples where the cost from certain crimes or offenders is well below the break-even point, and lower sentences would be warranted. I hope to further develop this crime-specific approach in future work.

VII. Conclusion

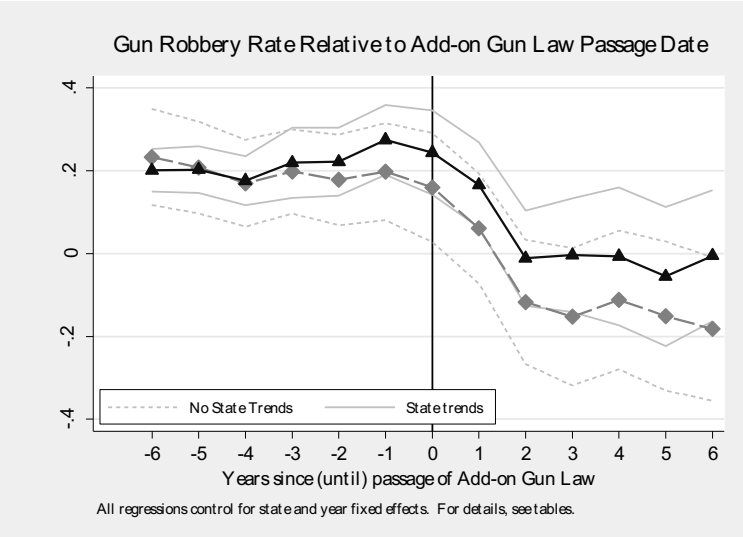
Determining the appropriate level of punishment for crimes is one of the most important functions of society, government, and the criminal justice system. Recent advances in data availability and analysis have made a scientific approach to these questions possible. In this chapter I have attempted to build on my own recent work and that of others to help further this goal.

Unlike previous studies, I have attempted to account for general deterrence, specific deterrence, incapacitation as well as a variety of indirect costs of incarceration, including lost freedom and capital costs.

For general deterrence, I use elasticity estimates from my study that makes use of add-on gun laws adopted in different states. For specific deterrence, I use estimates from my study using random assignment of cases to public defenders as an instrument for sentence served. For incapacitation, I survey some of the most recent, sophisticated studies on the topic. In addition, I use estimates of the cost of crime, cost of incarceration, value of freedom, and other parameters from my own work and that of other economists working in the field.

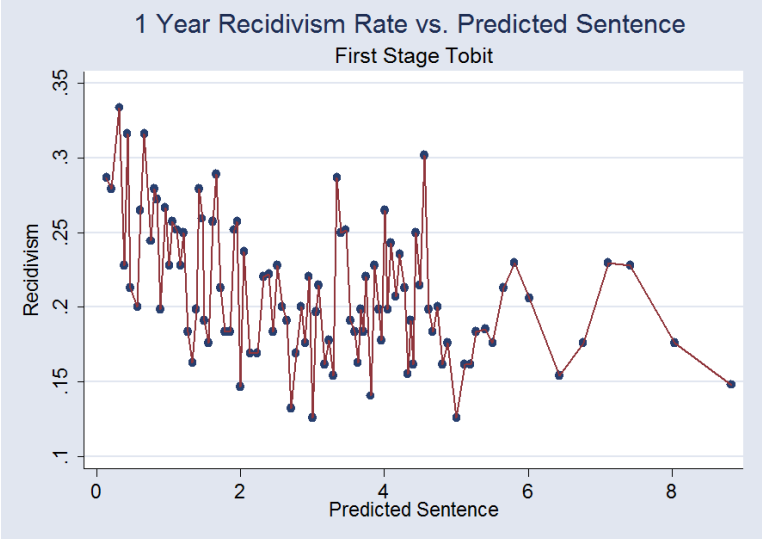
Because I incorporate more mechanisms by which sentencing affects crime, I use larger elasticities than some other studies, although not substantially larger. I find that the results are very dependent on the cost of crime estimates. For low cost of crime, shorter sentences are welfare enhancing, while for high cost of crime, the reverse is true. This points to the need for more study and more precision on the cost of crime. However, the approach used here may still be useful as it yields break-even points that can give some guidance for appropriate sentences by type of crime.

Figure 3.1



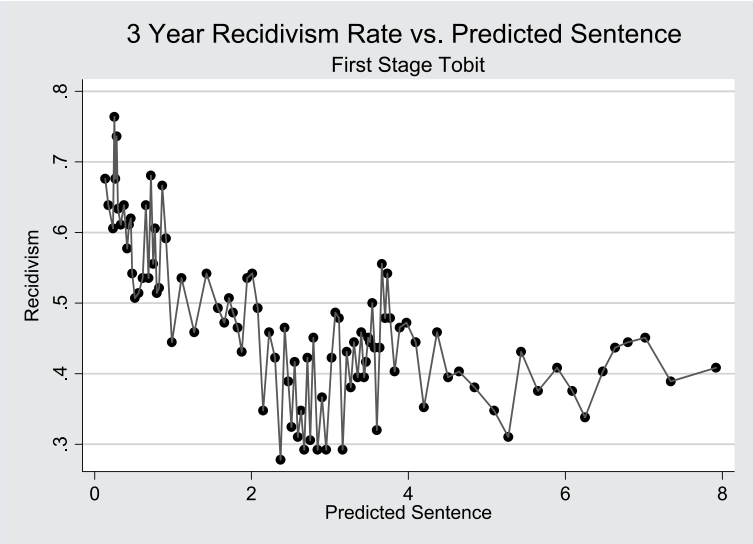
Reproduced from Abrams (2012)

Figure 3.2



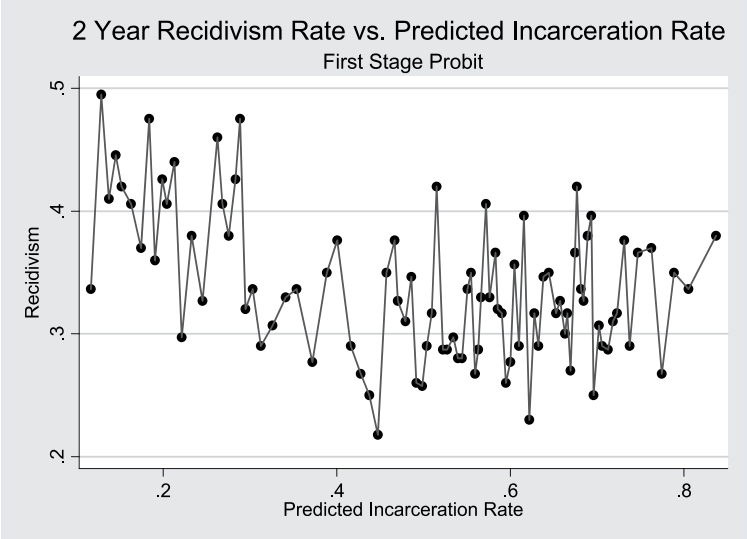
Reproduced from Abrams (2013)

Figure 3.3



Reproduced from Abrams (2013)

Figure 3.4



Reproduced from Abrams (2013)

Table 3.1

Panel A: One year Impact

	Annual Reported Gun Robberies per 100,000 residents							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Add-on Law Effective Date	-0.04 (0.06)	-0.06 (0.04)	-0.05 (0.03)	-0.02 (0.02)	-0.06 (0.07)	-0.07 (0.05)	-0.04 (0.03)	-0.01 (0.02)
Post MM Law Effective Date	-0.04 (0.07)	0.00 (0.08)	-0.12 (0.10)	-0.13 (0.09)	0.01 (0.06)	-0.07 (0.07)	-0.07 (0.16)	-0.10 (0.10)
State-specific time trends	n	y	n	y	n	y	n	y
Balanced Panel	n	n	y	y	n	n	y	y
Restrict to Post-1974	n	n	n	n	y	y	y	y
Observations	15516	15516	2975	2975	12979	12979	2234	2234
R-squared	0.17	0.18	0.18	0.19	0.16	0.18	0.18	0.19

Panel B: Two year Impact

	Annual Reported Gun Robberies per 100,000 residents							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Add-on Law Effective Date	-0.12 (0.07)	-0.14** (0.04)	-0.11** (0.03)	-0.08** (0.02)	-0.14 (0.07)	-0.15** (0.05)	-0.10** (0.02)	-0.06* (0.02)
Post MM Law Effective Date	-0.03 (0.07)	0.02 (0.07)	-0.10 (0.10)	-0.11 (0.09)	0.02 (0.06)	-0.05 (0.07)	-0.05 (0.15)	-0.09 (0.10)
State-specific time trends	n	y	n	y	n	y	n	y
Balanced Panel	n	n	y	y	n	n	y	y
Restrict to Post-1974	n	n	n	n	y	y	y	y
Observations	15516	15516	2975	2975	12979	12979	2234	2234
R-squared	0.17	0.18	0.18	0.19	0.17	0.18	0.18	0.19

Panel C: Three year Impact

	Annual Reported Gun Robberies per 100,000 residents							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Add-on Law Effective Date	-0.14 (0.08)	-0.17** (0.05)	-0.13** (0.04)	-0.09** (0.02)	-0.17 (0.09)	-0.18** (0.06)	-0.11** (0.03)	-0.05* (0.02)
Post MM Law Effective Date	-0.02 (0.07)	0.04 (0.07)	-0.10 (0.10)	-0.10 (0.09)	0.04 (0.06)	-0.04 (0.06)	-0.04 (0.15)	-0.10 (0.10)
State-specific time trends	n	y	n	y	n	y	n	y
Balanced Panel	n	n	y	y	n	n	y	y
Restrict to Post-1974	n	n	n	n	y	y	y	y
Observations	15516	15516	2975	2975	12979	12979	2234	2234
R-squared	0.17	0.18	0.18	0.19	0.17	0.18	0.18	0.19

* significant at 5%; ** significant at 1%

Note - The data consists of agency-year level observations. Standard errors reported in parentheses are clustered at the state level to allow for intra-state correlation in error structure. State and year effects are included in all specifications. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

Reproduced from Abrams (2012)

Table 3.2

IV regression results

		1 Year Recidivism				2 Year Recidivism		3 Year Recidivism	
		First Stage OLS		First Stage Tobit	IV LIML	First Stage OLS	First Stage Tobit	First Stage OLS	First Stage Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	First Stage Sentence Length	No age control	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment	Control for age at offense	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment	Recidivism-Age Profile Adjustment
Sentence		-0.0148	-0.0157	-0.00478	-0.00715	-0.0126	0.00123	-0.00757	0.00514
		(0.00519)**	(0.00525)**	(0.00425)	(0.00434)	(0.00741)	(0.00544)	(0.0103)	(0.00824)
Sex (1=male)	1.081	0.0735	0.0744	0.0624	0.0695	0.107	0.0923	0.1000	0.0901
	(0.123)**	(0.0101)**	(0.0103)**	(0.00965)**	(0.00873)**	(0.0142)**	(0.0128)**	(0.0171)**	(0.0161)**
Black Offender	0.0516	0.0369	0.0359	0.0355	0.0338	0.0622	0.0622	0.0803	0.0791
	(0.110)	(0.00831)**	(0.00844)**	(0.00844)**	(0.00744)**	(0.0111)**	(0.0111)**	(0.0134)**	(0.0134)**
Hispanic Offender	-0.0235	-0.0200	-0.0265	-0.0261	-0.0328	-0.0302	-0.0285	-0.0246	-0.0251
	(0.106)	(0.00888)*	(0.00886)**	(0.00887)**	(0.0105)**	(0.0124)*	(0.0123)*	(0.0156)	(0.0157)
Drug Offense	0.896	0.0199	0.0240	0.0209	-0.0530	0.0392	0.0217	0.0214	-0.00385
	(0.187)**	(0.0145)	(0.0147)	(0.0164)	(0.0149)**	(0.0210)	(0.0228)	(0.0288)	(0.0330)
Violent Offense	1.397	-0.0169	-0.0171	-0.0288	-0.0949	-0.0160	-0.0402	-0.0533	-0.0796
	(0.233)**	(0.0165)	(0.0166)	(0.0177)	(0.0157)**	(0.0237)	(0.0246)	(0.0311)	(0.0337)*
EFT	1.631	0.0357	0.0359	0.0223	-0.0437	0.0468	0.0215	0.0113	-0.0181
	(0.171)**	(0.0158)*	(0.0159)*	(0.0177)	(0.0167)**	(0.0220)*	(0.0235)	(0.0302)	(0.0325)
Sex Offense	1.207	-0.0135	0.000429	-0.00687	-0.0807	-0.0369	-0.0481	0.0202	0.00150
	(0.361)**	(0.0276)	(0.0294)	(0.0307)	(0.0238)**	(0.0397)	(0.0411)	(0.0512)	(0.0532)
Age at Offense	0.000453								
	(0.00425)								
Constant	-0.884	0.233	0.214	0.230	0.271	0.363	0.375	0.514	0.519
	(0.386)*	(0.0485)**	(0.0463)**	(0.0461)**	(0.0174)**	(0.0514)**	(0.0510)**	(0.0504)**	(0.0500)**
Observations	13573	13573	13557	13557	13725	10087	10087	7146	7146
Adjusted R-squared	0.165	0.024	0.022	0.021	0.022	0.043	0.042	0.074	0.074

** p<0.01, * p<0.05 Robust standard errors in parentheses

Note: Offender-level data obtained from the Clark County Public Defender Office for the years 2001-2008. Monthly time dummies included to allow for time variation in overall crime rates. Sentence is the minimum sentence measured in months, EFT = embezzlement, fraud, theft. Data includes 78 Public Defenders, each with a minimum of 50 cases and 51 judges. Recidivism is defined as reappearance as a Public Defender client within the indicated time after release from incarceration (if sentenced to incareceration).

Reproduced from Abrams (2013)

Table 3.3

Recidivism and Incarceration

	1 Year Recidivism		2 Year Recidivism		3 Year Recidivism	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	OLS	IV	OLS	IV	OLS	IV
Incarceration	0.0335	0.00396	0.114	0.0148	0.159	0.0889
	(0.00774)**	(0.0437)	(0.0106)**	(0.0521)	(0.0131)**	(0.0804)
Sex (1=male)	0.0543	0.0574	0.0807	0.0910	0.0777	0.0861
	(0.00836)**	(0.00954)**	(0.0115)**	(0.0126)**	(0.0146)**	(0.0162)**
Black Offender	0.0333	0.0345	0.0565	0.0591	0.0706	0.0734
	(0.00832)**	(0.00839)**	(0.0108)**	(0.0109)**	(0.0130)**	(0.0134)**
Hispanic Offender	-0.0272	-0.0257	-0.0325	-0.0289	-0.0311	-0.0288
	(0.00901)**	(0.00907)**	(0.0124)**	(0.0126)*	(0.0156)*	(0.0162)
Drug Offense	0.00782	0.00995	0.00777	0.0161	-0.0257	-0.0358
	(0.0140)	(0.0186)	(0.0189)	(0.0259)	(0.0238)	(0.0436)
Violent Offense	-0.0409	-0.0393	-0.0443	-0.0393	-0.0799	-0.0959
	(0.0147)**	(0.0188)*	(0.0201)*	(0.0261)	(0.0254)**	(0.0403)*
EFT	0.00654	0.00914	0.0115	0.0192	-0.0284	-0.0405
	(0.0134)	(0.0191)	(0.0179)	(0.0257)	(0.0222)	(0.0404)
Sex Offense	-0.0254	-0.0223	-0.0510	-0.0500	-0.0164	-0.0337
	(0.0270)	(0.0309)	(0.0367)	(0.0407)	(0.0466)	(0.0563)
Age at Release	-0.00159	-0.00157	-0.00288	-0.00282	-0.00303	-0.00294
	(0.000340)**	(0.000340)**	(0.000457)**	(0.000460)**	(0.000575)**	(0.000588)**
Constant	0.296	0.290	0.487	0.467	0.633	0.608
	(0.0490)**	(0.0498)**	(0.0535)**	(0.0546)**	(0.0532)**	(0.0545)**
Observations	13573	13540	10103	10059	7156	7110
Adjusted R-squared	0.026	0.025	0.060	0.050	0.100	0.082
** p<0.01, * p<0.05	Robust standard errors in parentheses					

Note: This table reports the results of OLS and 2SLS regressions or recidivism on a binary incarceration indicator. Offender-level data obtained from the Clark County Public Defender Office for the years 2001-2008. Monthly time dummies included to allow for time variation in overall crime rates. EFT = embezzlement, fraud, theft. Data includes 81 Public Defenders, each with a minimum of 50 cases and 50 judges. A tobit regression is used for the first stage of the IV regressions reported in this table.

Reproduced from Abrams (2013)

Table 3.4

Incapacitation Estimates from Previous Work		
Paper	**λ (crimes/year avoided)**	**Elasticity**
Marvell and Moody (1994)		.16
Levitt (1996)	5 - 8	.31
Sweeten Apel (2007)	4.9 to 8.4	
Bhati (2007) - person	1.93	.93
Bhati (2007) - property	9.47	.69
Owens (2009)	1.5	
Johnson & Raphael (2011) - property	9	.15 - .21
Johnson & Raphael (2011) - violent	0.15	.06 - .11

Table 3.5

Estimated "bottom up" and WTP for Crimes (2007 dollars)					
	Victim Costs	**CJ Costs**	**Offender Productivity**	**Total**	**WTP Estimate**
Murder	$4.6 million	$300,000	$140,000	$5.00 million	$11.80 million
Rape	$135,000.00	$8,300	$4,500	$150,000	$290,000
Armed robbery	$29,000	$14,700	$8,000	$50,000	$280,000
Robbery	$12,000	$7,400	$4,000	$23,000	$39,000
Aggravated assaults	$37,000	$13,500	$6,400	$55,000	$85,000
Simple assaults	$4,500	$5,000	$1,300	$11,000	$19,000
Burglary	$2,000	$2,300	$1,000	$5,000	$35,000
Motor vehicle theft	$5,500	$2,900	$1,000	$9,000	$17,000
Larceny	$450	$1,700	$700	$2,800	$4,000
Drunk driving	$28,000	$1,700	$700	$30,000	$60,000
Arson	$57,000	$1,700	$700	$60,000	$115,000
Vandalism	$370	$630	–	$1,000	$2,000
Fraud	$1,100	$1,700	$700	$3,500	$5,500
Other offenses (prostitution, loitering, false statements, etc.)	$500	–	$500	$1,000	

Reproduced from Cohen and Piquero (2009)

Table 3.6

Net Benefit Calculation from Inreasing Sentence Lengths

		Total Crime Elasticity				
		0.25	0.3	0.35	0.45	0.5
Low Crime Cost	Benefit (Millions of Dollars)	423	508	593	762	847
($5,860)	Cost (Millions of Dollars)	1355	1355	1355	1355	1355
	Net Benefit (Millions of Dollars)	-932	-847	-762	-593	-508
High Crime Cost	Benefit (Millions of Dollars)	1907	2288	2670	3432	3814
($26,400)	Cost (Millions of Dollars)	1355	1355	1355	1355	1355
	Net Benefit (Millions of Dollars)	552	933	1315	2078	2459

Benefits and costs from a 1% overall increase in sentence lengths of felony offenders sentenced to to incarceration. See text for details.

References

Abrams, David S. 2012. Estimating the Deterrent Effect of Incarceration using Sentencing Enhancements. *American Economic Journal: Applied Economics* 4:32-56.

Abrams, David S. 2013. Building Criminal Capital vs. Specific Deterrence: The Effect of Incarceration Length on Recidivism. Working Paper.

Abrams, David, and Albert Yoon. 2007. The Luck of the Draw: Using Random Case Assignment to Investigate Attorney Ability. *University of Chicago Law Review* 74:1145-1177.

Abrams, David, and Chris Rohlfs. 2011. Optimal Bail and the Value of Freedom: Evidence from the Philadelphia Bail Experiment. *Economic Inquiry* 49:750-770.

Berube, Danton A., and Donald P. Green. 2007. The Effects of Sentencing on Recidivism: Results from a Natural Experiment. 2nd Annual Conference on Empirical Legal Studies Paper Working Paper.

Bushway, Shawn, and Emily G. Owens. 2012. Framing Punishment: Incarceration, Recommended Sentences, and Recidivism. Working Paper.

Bushway, Shawn, and Raymond Paternoster. 2009. The Impact of Prison on Crime. Pp. 119-150 in *Do Prisions Make Us Safer? The Benefits and Costs of the Prison Boom*, edited by Steven Raphael and Michael Stoll. New York, NY: Russell Sage Foundation.

Cohen, Mark A., and Alex R. Piquero. 2009. New Evidence on the Monetary Value of Saving a High Risk Youth. *Journal of Quantitative Criminology* 25:25-49.

Donohue, John J. 2009. Assessing the Relative Benefits of Incarceration: The Overall Change Over the Previous Decades and the Benefits on the Margin. Pp. 269-341 in *Do Prisions Make Us Safer? The Benefits and Costs of the Prison Boom*, edited by Steven Raphael and Michael Stoll. New York, NY: Russell Sage Foundation.

Doob, Anthony M., and Cheryl Webster. 2003. Sentence Severity and Crime: Accepting the Null Hypothesis. *Crime and Justice* 23:143-195.

Drago, Francesco, Roberto Galbiati, and Pietro Vertova. 2009. The Deterrent Effects of Prison: Evidence from a Natural Experiment. *Journal of Political Economy* 117:257-280.

Ehrlich, Isaac. 1981. On the Usefulness of Controlling Individuals: An Economic Analysis of Rehabilitation, Incapacitation and Deterrence. *The American Economic Review* 71:307-322.

Fehr, Ernst and Urs Fischbacher 2004. Third Party Punishment and Social Norms. *Evolution and Human Behavior* 25:63-87.

Green, Donald P., and Daniel Winik. 2010. Using Random Judge Assignments to Estimate the Effects of Incarceration and Probation on Recidivism Among Drug Offenders. *Criminology* 48:357-387.

Helland, Eric, and Alexander Tabarrok. 2007. Does Three Strikes Deter: A Non-Parametric Estimation. *Journal of Human Resources* 42(2):309-330.

Hjalmarsson, Randi. 2009. Crime and Expected Punishment: Changes in Perceptions at the Age of Criminal Majority. *American Law and Economics Review* 11:209-248.

Kessler, Daniel P., and Steven D. Levitt. 1999. Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation. *Journal of Law and Economics* 42:343-364.

Klick, Jonathan, and Alexander Tabarrok. 2005. Using Terror Alert

Levels to Estimate the Effect of Police on Crime, *The Journal of Law & Economics* 48:267-279.

——. 2010. Police, Prisons, and Punishment: The Empirical Evidence on Crime Deterrence, Pp. 127-144, in *Handbook on the Economics of Crime*, edited by Bruce L. Benson and Paul R. Zimmerman eds. Northampton, MA: Edward Elgar.

Kling, Jeffrey. 2006. Incarceration Length, Employment, and Earnings. *American Economic Review* 96:863-76.

Lee, David S., and Justin McCrary. 2009. The Deterrence Effect of Prison: Dynamic Theory and Evidence, CEPS Working Paper No.189.

Levitt, Steven D. 1998. Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?. *Economic Inquiry* 36:353-372.

Levitt, Steven D. 1998. Juvenile Crime and Punishment. *Journal of Political Economy* 106:1156-1185.

Levitt, Steven D., and Thomas J. Miles. 2007. Empirical Study of Criminal Punishment. Pp. 457-494 in vol. 1 of *Handbook of Law and Economics*, edited by A. Mitchell Polinsky and Steven Shavell. Amsterdam: Elsevier.

Loeffler, Charles. 2013. Does Imprisonment Alter the Life Course? Evidence on Crime and Employment from a Natural Experiment. *Criminology* 51: 137-166.

Loftin, Colin, and David McDowall. 1984. The Deterrent Effects of the Florida Felony Firearm Law. *The Journal of Criminal Law and Criminology* 75:250-259.

Marvell, Thomas B., and Carlisle E. Moody. 1995. The Impact of Enhanced Prison Terms for Felonies Committed with Guns. *Criminology* 33:247-281.

Maurin, Eric, and Aurelie Ouss. 2009. Sentence Reductions and Recidivism: Lessons from the Bastille Day Quasi Experiment, IZA Discussion Paper.

Nagin, Daniel. S., 1998. Criminal Deterrence Research at the Outset of the Twenty-First Century. *Crime and Justice* 23:1-42.

Nagin, Daniel S., Francis T. Cullen, and Cheryl L. Jonson. 2009. Imprisonment and Reoffending. *Crime & Justice: A Review of Research* 38:115-200.

Robinson, Paul H., and John M. Darley. 2004. Does Criminal Law Deter? A Behavioural Science Investigation. *Oxford Journal of Legal Studies* 24:173-205.

Turner, Emily. 2009. Using Random Case Assignment and Heterogeneity in Attorney Ability to Examine the Relationship Between Sentence Length, and Recidivism. Unpublished dissertation. University of Pennsylvania.

Vernick, Jon S., and Lisa M. Hepburn. 2003. State and Federal Gun Laws: Trends for 1970–99. Pp. 345-411 in *Evaluating Gun Policy: Effects on Crime and Violence*, edited by Jens Ludwig. Washington, DC: Brookings Institution Press.

Webster, Cheryl M., Anthony N. Doob, and Franklin E. Zimring. 2006. Proposition 8 and Crime Rates in California: The Case of the Disappearing Deterrent. *Criminology and Public Policy* 5:417-448.