American Economic Association

Estimating the Deterrent Effect of Incarceration Using Sentencing Enhancements

Author(s): David S. Abrams

Source: *American Economic Journal: Applied Economics*, Vol. 4, No. 4 (October 2012), pp. 32-56
Published by: American Economic Association
Stable URL: http://www.jstor.org/stable/23269741
Accessed: 11-08-2016 18:17 UTC

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at
http://about.jstor.org/terms

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

American Economic Association is collaborating with JSTOR to digitize, preserve and extend access to *American Economic Journal: Applied Economics*

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

American Economic Journal: Applied Economics 2012, 4(4): 32–56
http://dx.doi.org/10.1257/app.4.4.32

Estimating the Deterrent Effect of Incarceration Using Sentencing Enhancements†

By DAVID S. ABRAMS*

Increasing criminal sanctions may reduce crime through two primary mechanisms: deterrence and incapacitation. Disentangling their effects is crucial for optimal policy setting. I use sentence enhancements due to the introduction of state add-on gun laws to isolate the deterrent effect of incarceration. Using cross-state variation in the timing of law passage dates, I find that the average add-on gun law results in a roughly 5 percent decline in gun robberies within the first 3 years. This result is robust to a number of specification tests and does not appear to be associated with large spillovers to other types of crime. (*JEL* K14, K42)

How much does the threat of incarceration deter crime? The answer to this question is of crucial importance in formulating criminal sentencing policies. An increase in sentence length for any given crime may reduce the incidence of criminal acts by deterring potential offenders, but it also increases the length of time offenders are incarcerated and are hence unable to commit additional offenses. Each effect has different implications for our crime prevention and punishment system. Distinguishing between these two effects—the deterrence effect and the incapacitation effect—is one of the most challenging problems in the economics of crime. This paper seeks to isolate the deterrent effect of sentencing by exploiting variation in penalties induced by add-on gun laws. This approach adds to previous deterrence research and is the first to analyze a repeated natural experiment on a national scale.

Understanding the impact of incarceration has grown more important over time as incarceration rates in the United States have grown by over 250 percent between 1980 and 2008.[1] The total US incarcerated population in 2008 stood at 2.4 million, with the US having the highest incarceration rate worldwide (Walmsley 2009). The relative impact of incapacitation and deterrence are of first-order importance in understanding how to effectively reduce crime. If deterrence is very small, increasing sentence lengths would only reduce crime by taking potential offenders off the

* University of Pennsylvania Law School and Department of Business Economics and Public Policy, Wharton School, 3501 Sansom Street Philadelphia, PA 19104-6204 (e-mail: dabrams@law.upenn.edu). The author would like to thank Liz Ananat, Jessica Cohen, Claudia Goldin, Michael Greenstone, Justin McCrary, Guy Michaels, Sendhil Mullainathan, Sarah Siegel, Jeffrey Smith, Catherine Thomas, and participants of the MIT Labor Lunch, University of Maryland Criminology and Economics Workshop, University of Chicago Applied Microeconomics Lunch, University of Chicago Crime and Punishment Workshop, and Harvard Law and Economics Seminar for very helpful comments. Trevor Gallen, Jon Gillam, and Kathy Qian provided excellent research assistance.

† To comment on this article in the online discussion forum, or to view additional materials, visit the article page at http://dx.doi.org/10.1257/app.4.4.32.

[1] See the Bureau of Justice Statistics (BJS) website for recent data on incarceration rates: http://bjs.ojp.usdoj.gov/index.cfm.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

streets for longer periods of time. This is a very expensive proposition, with jailing costs around $100/day (see e.g., DiIulio and Piehl 1991; Waldfogel 1993; Levitt 1996). Alternatively, if deterrence is substantial, then increasing sentences offers a relatively low cost means of reducing the incidence of crime. This offers the possibility of reducing crime without bearing the cost of enforcing the penalties, something particularly appealing in a time of tight budgets.

The aim of this paper is to empirically estimate the magnitude of deterrence more precisely than has previously been possible by using add-on gun laws. Add-on gun laws stipulate sentence enhancements for defendants convicted of possessing a firearm while committing a crime. This type of law grew popular in the United States in the 1970s and 1980s, with 30 states adopting one of these laws by 1996 (Vernick and Hepburn 2003). Add-on gun laws provide a unique set of natural experiments that can be used to distinguish the deterrent effect of incarceration from the incapacitative effect. The key to the approach in this paper 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.

The use of the short-term impact of a sentencing enhancement to separate deterrence from incapacitation was pioneered by Kessler and Levitt (1999). They used the passage of Proposition 8 in California in 1982 as a natural experiment that enhanced sentences for certain types of crimes (and offenders) and not others. Using a differences-in-differences and triple difference approach, they found evidence for a modest, but significant deterrent effect of 8 percent within 3 years of the law change. Owens (2009) examines the effect of a Maryland law change that reduces the adult sentences of some former juvenile delinquents. Unlike Kessler and Levitt (1999), she uses the law change to estimate the incapacitation effect, which she finds to be substantially smaller than most previous estimates. Other recent papers use sentencing enhancements and disenhancements as well. Helland and Tabarrok (2007) investigate the effects of California's three strikes law and find a decrease in arrests of around 20 percent among felons with two strikes. Drago, Galbiati, and Vertova (2009) use a natural experiment in Italy that induced individual-level variation in sentencing to estimate a deterrence effect. Marvell and Moody (1995) estimate the combined effect of deterrence and incapacitation due to firearm sentencing enhancements, using time series variation.

The strategy in this paper for estimating the impact of increased sentence length follows similar lines to some of the aforementioned studies, but makes several advances. Unlike previous studies that focus on an individual state, the fact that add-on gun laws were passed in a majority of states allows for more easily generalizable results of the analysis. The sample in this study is extremely representative of the country as a whole, since most states passed an add-on gun law at some point in the period investigated. It also uses a time series almost 40 years long, which lends strength to the belief that the findings are not location and time specific.

The substantial previous literature on deterrence has come to mixed conclusions.[2] Part of this may be due to the fact that there are well-known data errors in the

[2]There is an extensive literature empirically testing various aspects of economic models of crime going back decades to Ehrlich's work on the death penalty (Ehrlich 1973, 1975, 1981). A full review of the literature on

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

most commonly used crime data, the Uniform Crime Reports (UCR) (Maltz and Targonski 2004). This study makes use of hand-cleaned data to address this problem. In addition, a number of alternate specifications are reported in the online Appendix to attempt to fully explore the sensitivity of the findings to choices of specification.[3]

The additional specifications and robustness checks are all consistent with the main finding of evidence of a deterrent effect of sentence enhancements. The preferred specification yields a statistically significant point estimate of a 5 percent reduction in gun robberies within three years of the add-ons. In order to account for potential contemporaneous law enforcement changes that occur with add-on gun laws, I run a triple difference specification, which supports the main finding. I also examine the impact of add-on laws on other crimes besides gun robberies. Gun assaults show a small and statistically insignificant effect of the add-on laws. This is in keeping with some findings that assaults tend to be less about pecuniary motives and perhaps less subject to deterrence (Gould, Weinberg, and Mustard 2002; Silverman 2004).

There are two possible predictions for the impact of an add-on gun law on crimes associated with gun robberies. Criminals may shift toward a lower penalty substitute crime when gun penalties increase. However, if potential criminals are generalists and tend to commit a set of related crimes, they may shift to the legitimate sector, and some individuals may choose not to become criminals in the first place. Using a dataset with extensive criminal histories, I identify nongun robberies and larcenies as the two most likely crimes committed by a gun robber. There is a decrease in these crimes following add-on gun laws, and a larger decrease in those regions with the greatest share of gun robberies. I also find no impact of gun laws on rapes and murders, the two types of crimes least associated with gun robberies.[4] Together this evidence supports the generalist criminal theory and suggests that some potential criminals likely "go straight" in response to enhanced penalties for gun crimes.

deterrence has been the subject of a number of review articles, with 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. Doob and Webster (2003) review a large number of papers by criminologists and a handful by economists and conclude that the lack of strong evidence for deterrence is widespread enough to conclude that there is a null effect. These coauthors along with Frank Zimring (Webster, Doob, and Zimring 2006) take a skeptical view of Kessler and Levitt's 1999 paper, and its evidence for deterrence. Robinson and Darley (2004) take a somewhat more nuanced view that there are circumstances where increased sentences may deter, although they believe the magnitude is insufficient to influence policy decisions. Levitt and Miles (2007), in a wide-ranging piece, point to some of the economic studies that suggest there is evidence for deterrence, but conclude that more research on the topic is needed.

Several papers have used the discontinuity in sentencing at the age of majority to identify deterrence effects. Levitt (1998b) uses cross-state differences in the relative harshness of adult sanctions relative to those for juveniles. He finds that those states with larger jumps in punishment tend to have larger decreases in adult crime relative to juvenile. Hjalmarsson (2009) finds that offender perceptions of penalties change far less than actual changes at the age of majority, and finds little evidence of deterrence in self-reported data. Lee and McCrary (2011) use high frequency data from Florida to search for a discontinuity in offending around the 18th birthday. They find a drop in crime of 2 percent around this discontinuity and suggest that part of the low response might be due to myopic behavior. Two other recent papers of note look not at sentence length, but rather prison conditions and find evidence for deterrence (Katz, Levitt, and Shustorovich 2003; Chen and Shapiro 2007).

[3] Carefully checking that estimations are robust is of particular importance in the crime literature where the data is often noisy, clear experiments are rare, and confounds are plentiful. Also of concern in any study examining one-time rule changes is calculating correct standard errors (Bertrand, Duflo, and Mullainathan 2004). I do so by constructing placebo laws and estimating standard errors using a Monte Carlo simulation and report these results in the online Appendix as well.

[4] Among crimes reported in the UCR.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

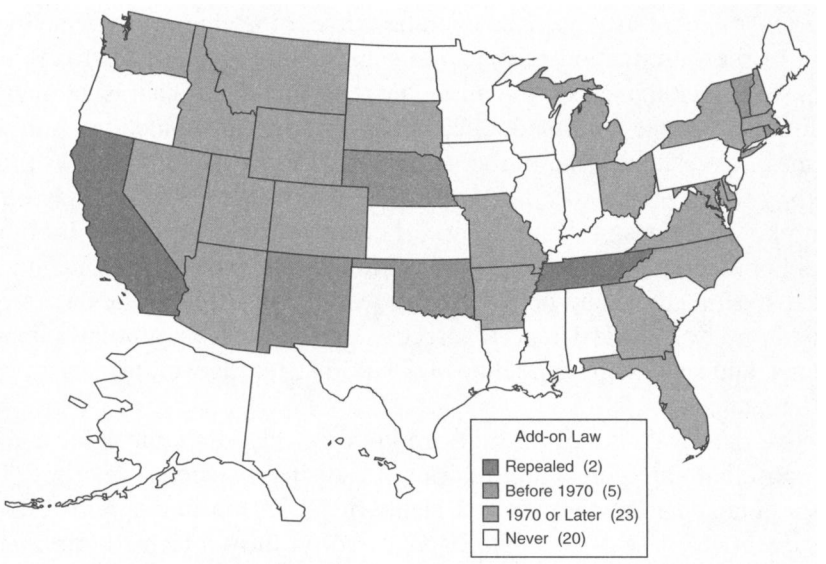

FIGURE 1. ADD-ON GUN LAWS BY DATE OF ENACTMENT

The rest of the paper proceeds as follows. The next section provides a brief background on firearm sentencing enhancements and a description of the data. Section II presents the main specifications along with a discussion of potential interpretations and confounds. Section III presents the main empirical results. In Section IV, I discuss a number of additional specifications that further test the central findings. Section V concludes.

I. The History of Add-on Gun Laws and Data Description

An add-on gun law, as used in this paper, is a state law which mandates enhanced prison sentences for defendants convicted of a felony who are further found to have used or been in possession of a firearm in the commission of the felony. These types of laws became popular in the 1970s with the aim of reducing armed crimes. Over 25 states currently have add-on gun laws in their statutes, with most states adopting the laws in the 1970s and 1980s. Figure 1 shows the geographical distribution of the timing of add-on law adoption across states.

Add-on gun laws are a prominent example of legislative efforts to reduce the incidence of crime beginning in the 1960s.[5] Legislation leading to increased penalties was introduced in an attempt to deter potential criminals and incapacitate potential recidivists. Closely related to add-on gun laws, and also designed to curb crimes involving firearms, are mandatory minimum laws. These laws are distinct from add-on laws because they do not necessarily increase the sentence length for a given defendant, but only provide a lower bound on his or her sentence length. Identifying

[5] Around the same time, sentencing guidelines were introduced around the country. Their purpose was to standardize sentence lengths, but they also led to more severe sentencing in many cases.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

the deterrent effect of increased incarceration time using changes in behavior around the time of the introduction of mandatory minimums poses a relatively complex problem since, in many cases, the minimum does not bind.[6] That is, in many states, the minimum sentence for armed robbery is longer than the mandatory minimum for gun crimes. Nevertheless, the introduction of mandatory minimum laws provides a good proxy for any state-specific unobserved characteristics which may be associated with both changes in the incidence of crime and the decision to introduce gun add-ons. For this reason, the introduction of mandatory minimums is included in the empirical specifications and permits improved identification of the deterrent effect of the add-on laws. Table 1 reports effective dates of add-on gun laws, mandatory minimums, and sentencing guidelines for all 50 states, as well as dates of passage for add-on gun laws.

Effective date and date of passage are both included in Table 1 for add-on gun laws because both are potentially relevant in causing a response in crime. The first uses data obtained from Vernick and Hepburn (2003) on the date the add-on law became effective.[7] Use of this date is premised on fully-informed criminals rationally responding to changes in penalties precisely when they occur. Alternatively, it is possible that the publicity and debate surrounding an imminent change in the law or uncertainty about the law's effective date had an impact on potential criminal behavior before the change actually occurred. The add-on law dates of passage were collected by the author from state criminal codes and state legislative journals and are reported in column 2 of Table 1.

The Uniform Crime Reports (UCR), compiled by the FBI, contain the longest and broadest dataset on crime in the United States.[8] Important to this study is the fact that, since 1965, counts of robberies and assaults have been distinguished by type of weapon involved. The latest reports consist of data collected from nearly 17,000 law reporting agencies, a number that has increased substantially over time. An agency is a local law enforcement jurisdiction, often a city.[9] The increase in reporting agencies within each state poses an empirical challenge. Including all agencies reporting in a given time period will lead to a substantially unbalanced panel: later dates would receive much more weight. Thus for the main specifications, I construct a set of the 500 most populous agencies that report data for the full sample range (1965–2002). Twenty-one agencies are added to this dataset for a total of 521 to ensure that every state is represented by at least three agencies.[10]

The UCR are known to contain substantial numbers of data errors, particularly at the agency level (Maltz and Targonski 2004). Part of the data cleaning process

[6] I run specifications using mandatory minimum gun laws alone, but find no significant effect. The coefficients on the mandatory minimum dummy variables are provided in Table 3.

[7] This data is largely based on Marvell and Moody (1995) with a number of updates and corrections.

[8] Another substantial dataset frequently used to study the impact of criminal legislation is the National Crime Victimization Survey. This dataset has some advantages over the UCR in that it may capture crimes that go unreported to the police. However, the data does not include geographic identification and thus cannot be used in the current research. "State codes are not available in the National Sample because of confidentiality restrictions" (BJS 1998). City level files are available for 26 major cities for the years 1972–1975. These were not used due to the short time span available.

[9] This is used as the basic unit of analysis because it is the smallest unit for which data is collected nationally.

[10] There is one exception to this rule. There were only two districts in Vermont that reported for the full time period.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 1—STATE CRIMINAL LAW CHANGES

State	Add-on passage date	Add-on effective date	Mandatory minimum	Sentencing guidelines
Alabama	None	None	5/27/81	1/1/04
Alaska	None	None	1/1/80	1/1/80
Arizona	5/13/74	8/9/74	8/9/74	None
Arkansas	2/27/81	6/16/81	6/16/81	1/1/94
California	<1970	9/9/1953*	1/1/1976*	None
Colorado	5/10/76	7/1/76	7/1/76	None
Connecticut	6/8/93	10/1/93	10/1/81	6/21/10
Delaware	3/29/73	7/1/73	7/1/73	10/1/87
Florida	7/3/74	7/1/75	10/1/75	10/1/83
Georgia	4/7/76	7/1/76	None	None
Hawaii	None	None	6/7/76	None
Idaho	2/25/77	7/1/77	None	None
Illinois	None	None	2/1/78	8/25/09
Indiana	None	None	< 1970	None
Iowa	None	None	1/1/78	None
Kansas	None	None	7/1/76	7/1/93
Kentucky	None	None	6/19/76	None
Louisiana	None	None	9/11/81	1/1/92
Maine	None	None	9/23/71	None
Maryland	3/27/72	6/1/72	6/1/72	7/1/83
Massachusetts	8/13/74	4/1/75	4/1/75	4/1/96
Michigan	2/11/76	1/1/77	1/1/77	1/1/84
Minnesota	None	None	8/1/79	5/1/80
Mississippi	None	None	None	None
Missouri	6/24/76	8/13/76	8/13/76	3/1/97
Montana	5/13/77	1/1/78	1/1/78	None
Nebraska	<1970	<1970	None	None
Nevada	5/3/73	5/3/73	7/1/79	None
New Hampshire	7/5/77	9/3/77	9/3/77	None
New Jersey	None	None	2/12/81	1/1/04
New Mexico	<1970	<1970	<1970	7/1/03
New York	9/17/96	11/1/76	None	None
North Carolina	3/26/94	3/26/94	None	10/1/94
North Dakota	None	None	7/1/77	None
Ohio	10/5/82	1/5/83	1/5/83	7/1/96
Oklahoma	<1970	<1970	None	None
Oregon	None	None	10/2/79	11/1/89
Pennsylvania	None	None	6/6/82	7/1/82
Rhode Island	<1970	<1970	None	None
South Carolina	None	None	6/3/86	None
South Dakota	3/14/85	4/3/85	None	None
Tennessee	3/29/1976**	7/1/76	7/1/1976**	11/1/89
Texas	None	None	8/29/77	None
Utah	2/11/76	5/1/76	None	1/1/79
Vermont	<1970	<1970	None	None
Virginia	3/24/75	10/1/75	10/1/75	1/1/91
Washington	3/27/84	7/1/84	7/1/84	7/1/84
West Virginia	None	None	6/8/79	None
Wisconsin	None	None	3/1/80	4/25/84
Wyoming	3/8/79	5/25/79	None	None

Notes: Effective 6/29/1977, California add-on law no longer in force for gun robberies. Effective date reported for mandatory minimums. Guideline date is effective date except for Connecticut, Illinois, New Jersey, New Mexico, and Wisconsin for which date of passage is reported.

**Tennessee's add-on and mandatory minimum statute repealed effective 11/11/1989.

Sources: Vernick and Hepburn (2003), Marvell and Moody (1995), Kauder and Ostrom (2008), Frase (2005) and author's own research in state statutes and legislative histories

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

required examination and correction of the data by hand, which necessitated limiting the data to the most populous agencies as described above. The dataset used covers approximately 40 percent of the contemporary US population.[11]

There are several different types of data within the UCR including reported offenses, unfounded offenses, offenses cleared (cases in which arrests are made), and juvenile offenses cleared. In this study, I use reported offenses rather than arrests as the primary measure of the incidence of crime. This choice is made to try to address the concern that policing might be modified to focus on gun crimes in response to or contemporaneous with the introduction of add-on gun laws. A modification in policing behavior in response to legislative changes would be reflected in the number of offenses cleared and complicates the task of isolating the responsiveness of criminal activity to the new law. There may also be a change in crime reporting behavior in response to a law change or the ensuing publicity. To address this concern, this paper makes use of reported crimes rather than arrests. This way, even if there are contemporaneous policing changes, the impact on reported crimes should presumably be less sensitive to law changes than policies.

Table 2 reports summary statistics from the UCR data by type of offense.[12] The first six columns summarize the data in states that have add-on gun laws and the last two columns are for states that never had one. For the add-on states, the mean and standard deviation of each crime is reported before and after the date the add-on law became effective. The first two columns report all years of data, the second two is a balanced panel where the number of reported crimes is restricted seven years prior to, and 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 and ensures that each agency has the same number of observations. The third pair of columns is a balanced panel as well, but the data is restricted to post-1974. This is due to the fact that there is a discontinuity in several variables in a large number of agencies in 1974 in the UCR data.[13]

An inspection of the mean number of crimes in Table 2 foreshadows the main results that are reported in Section III. Gun robberies drop substantially after add-on gun laws, although it is possible that some of this is due to overall time trends (as can be seen in Figure 2). The full event study specifications will control for these, as well as state-specific time trends. In addition to a substantial drop in gun robberies, we see a smaller but substantial drop in nongun robberies, as well. Rapes and murders increase and decrease, respectively, after the law change, but not to a great extent. Gun assaults and larcenies increase in some subsets of the data and decrease in others after the add-on law.

[11] In the online Appendix, I also report results from an alternate specification where data is aggregated to the state level. This has the advantage of being somewhat less noisy, but the difficulty that the number of agencies encompassed by a state varies over time.

[12] Definitions from the Uniform Crime Reports: Robbery—The taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or putting the victim in fear. Separate counts are included for Firearm Robbery (i.e., any firearm is used as a weapon or employed as a means of force to threaten the victim or put him in fear); Assault—An unlawful attack by one person upon another; and Firearm Assault includes all assaults wherein a firearm of any type (e.g., revolver, automatic pistol, shotgun, zip gun, rifle, etc.) is used or its use is threatened.

[13] Staff members at the National Archive of Criminal Justice Data, which houses the publicly available UCR dataset, were unable to account for this break in the data.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 2—REPORTED CRIME RATES

	Add-on states						Non-add-on states	
	All years		Balanced panel		Post-1974 balanced panel			
Crime category	Before	After	Before	After	Before	After	All years	Post-1974
Gun robbery	189.0	139.70	218.6	130.7	226.2	128.3	151.6	153.0
	(188.4)	(148.5)	(204.8)	(133)	(200.6)	(130.2)	(151.2)	(153.5)
Nongun robbery	257.8	196.7	273.6	239.7	362.8	237.8	198.1	228.9
	(259.7)	(200.3)	(275)	(204.3)	(300.2)	(202.7)	(201.8)	(214.3)
Murder	13.37	11.94	15.77	13.21	15.71	13.08	14.09	14.64
	(9.99)	(11.6)	(11.7)	(11.9)	(11.4)	(11.8)	(10.98)	(11.6)
Rape	39.99	54.87	42.82	51.13	46.41	51.18	46.26	53.14
	(28.4)	(56.62)	(26.2)	(34.6)	(27.4)	(34.8)	(47.3)	(52.3)
Gun assault	98.32	122.5	108.5	98.13	120.4	97.85	104.8	120.7
	(91.5)	(133.2)	(89.2)	(90.6)	(99.8)	(90.8)	(107.5)	(116.8)
Larceny	2,957	3,789	3,332	3,550	3,540	3,555	3,502	3,876
	(1,327)	(1,627)	(1,379)	(1,724)	(1,447)	(1,729)	(1,604)	(1,583)
Observations	6,453	6,884	1,864	1,412	917	1,399	6,271	4,691

Notes: Values in the tables are reported crimes per 100,000 residents. Data is from the FBI's Uniform Crime Reports.

Figure 2 documents the well-known sharp run up in crimes in the 1960s and 1970s and later a decline beginning in the 1990s. The trends for gun robbery are similar to overall crime trends, and the trends are very similar for add-on and non-add-on states. One point to note from the figure is that both gun robbery and overall crime rates are higher for add-on states in the first half of the data, but this reverses in the last decade. This may be due to the impact of the add-ons or to other cross-state variation. I now introduce the framework that I will use later in the paper to distinguish between these possibilities.

II. Methodology

The empirical challenge is to isolate the effect of add-on gun laws, estimate their impacts, and try to minimize the possibility that estimates result from something other than deterrent effects of the laws. With this in mind I adopt an event study methodology for most specifications, which takes advantage of the variation in timing of the law change across states.[14] I test several different outcomes: gun robbery, gun assault, nongun robbery, larceny, murder, and rape. In all specifications, I control for lagged prison population data, police population share, as well as economic and demographic measures.

The goal here is to identify the deterrent effect of incarceration, separate from incapacitation. Thus, it is necessary to distinguish between changes in crime rates

[14] In the online Appendix, I report various specification checks, including testing the date of law adoption versus the effective date, adding further controls, and restricting the dataset, as well as a falsification test using placebo dates for the law change. In addition, I test several models allowing for variation in the immediacy of impact of the law.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

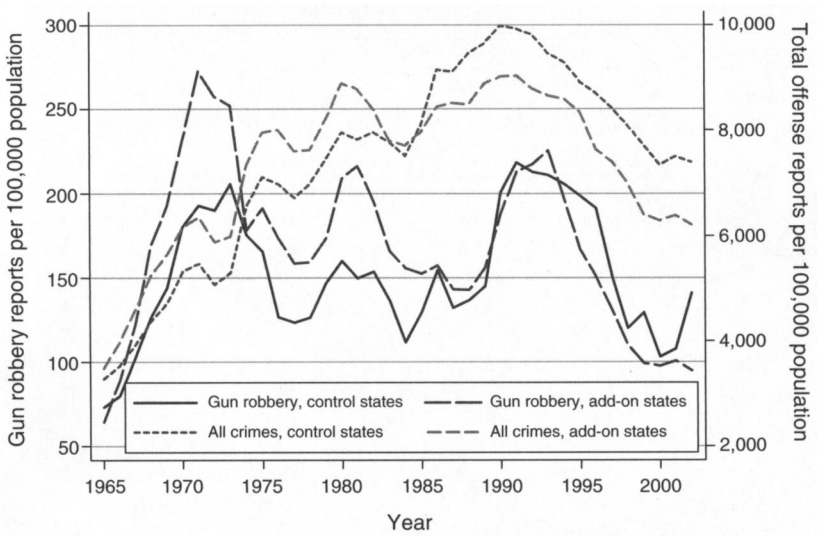

FIGURE 2. CRIMES RATES, 1965–2002

Source: FBI Uniform Crime Reports

following the introduction of add-on laws caused by increased spells of incarceration from crime rate changes due to the fact that some potential offenders may have been deterred. This is done by restricting attention to crime rates within a short period immediately following the introduction of the add-on law.

The logic is as follows. Assume the minimum sentence for the underlying crime prior to the add-on was x years and the add-on increases it by y additional years. Within the first x years after the law change there will be no effective change to incapacitation. All offenders sentenced in this period after the law change would have been incapacitated under the old law as well. Thus, any change in crime rates in the first x years cannot be due to incapacitation, and may be interpreted as a deterrent effect.

An important question, therefore, is of the appropriate value of x, the previous minimum sentence for the underlying crime. An estimate of three years was found as follows. Data on the minimum and maximum sentence for first degree robbery (or its equivalent) was collected from state statutes for 47 of the 50 states (Figure 3). The mean minimum sentence length is 5.5 years, and the median is 5 years. For the maximum sentence length the mean and median are 16.5 years and 13.5 years, respectively. It is possible that some defendants serve less than the minimum time, receiving time off for good behavior (although truth-in-sentencing laws have reduced the likelihood of this happening over time). Ideally, one would prefer an empirical distribution of time served by state, but no such dataset exists for the required years. The best empirical data on actual time served comes from the National Corrections Reporting Program, which is consistent with the three year figure. A three-year time span was hence chosen as a conservative estimate of the time during which those prisoners prevented from reoffending by incarceration

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

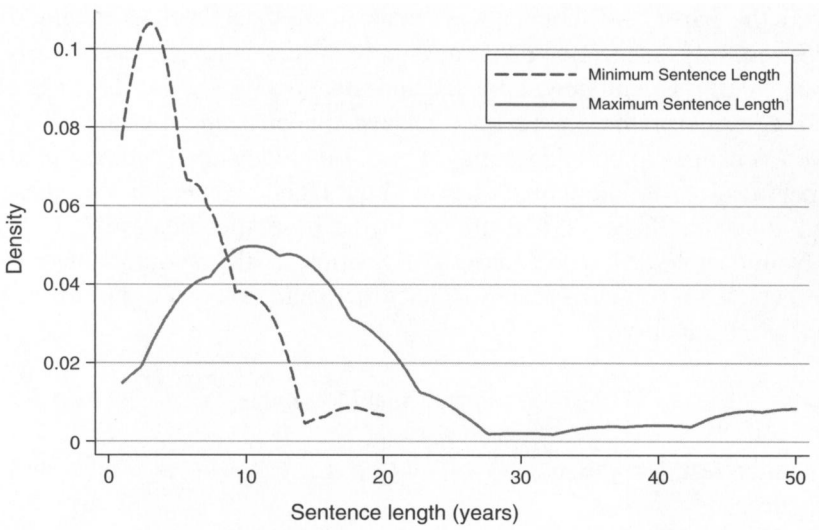

FIGURE 3. GUN ROBBERY SENTENCING DISTRIBUTION ACROSS ALL STATES

Source: Data collected from state statutes by the author

would have been removed from the set of potential offenders independent of the introduction of an add-on gun law.[15]

The key identifying assumption in this paper is that add-on gun law adoptions are exogenous. Although most add-on gun laws were enacted in the 1970s, due to the previously discussed national trends, the particular timing in a state is to a large degree random due to the vagaries of the political process within each state. This assumption plays a critical role in allowing the empirical tests to distinguish between general trends in crime rates in a given state and changes in behavior that are attributable to the introduction of the add-on law.

In order to further investigate the plausibility of this assumption, I collected additional data from newspapers. While it is difficult to establish complete randomness in the timing of law changes, there is some evidence to this point. Often legislative activity on crime-related issues is spurred by idiosyncratic events, like a particularly notorious crime. Newspaper data was collected in order to investigate whether this was a frequent impetus for add-on gun laws. There is scant digitally searchable newspaper data available before the 1990s, yielding only eight newspapers from six states with articles within a year of the law change. Although the small sample size makes it difficult to draw strong conclusions, it is informative to note that articles in two of the six states point to specific, notorious crimes as spurring the introduction of legislation.

Beyond newspaper evidence about the randomness in timing of the law change, one may still have concerns about the timing or about concomitant changes in enforcement. There are at least three factors that should reduce this concern. First, while these law changes are made at the state level, policing decisions (and the

[15] To be even more conservative, most of the analysis is also performed for one and two year time spans.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

analysis in this paper) are almost always made at the local level. Crime rates within a state will certainly be correlated, but not likely to such a degree that all jurisdictions will change enforcement identically and simultaneously. Second (as noted above), I use reported crimes rather than arrests because this measure of crime should be less sensitive to changes in policing policy. Third, I include state-specific time trends in some specifications to allow for concerns about legislative response to these trends, and find substantially similar results as in the base specification.[16] I also report results from a triple difference analysis in Section IV that should isolate the deterrence effect even if concerns remain about other changes contemporaneous with the add-on law introduction.

A. *Central Specification*

The initial test for the impact of add-on gun laws is a simple differences-in-differences,

$$\mathbf{y}_{at} = \beta \mathbf{Addon}_{st} + \boldsymbol{\lambda}_s + \boldsymbol{\gamma}_t + \mathbf{x}_{st} + \mathbf{mm}_{st} + \boldsymbol{\varepsilon}_{at}. \tag{1}$$

Here, $\mathbf{y}_{at}$ is the outcome of interest, namely a log per capita crime rate measured at the agency level. The variable $\mathbf{Addon}_{st}$ is a dummy that is one in states with an add-on gun law in force, within n years of the add-on date (where n varies across different specifications), and zero otherwise.[17] $\boldsymbol{\lambda}_s$ allows for permanent differences across states in crime rates (state fixed effects). Any national trends in crime will be absorbed into the year dummies ($\boldsymbol{\gamma}_t$). Potentially important time-varying state characteristics are controlled for with the vector $\mathbf{x}_{st}$. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population.[18] The controls also include a dummy for whether the state has a mandatory minimum law in force ($\mathbf{mm}_{st}$). This can be seen as controlling for the direct effect of mandatory minimum laws and also to proxy for other characteristics of states that passed both laws. The coefficient β signifies the impact of the add-on gun law. Errors ($\boldsymbol{\varepsilon}_{at}$) are allowed to be heteroskedastic and correlated within states. Thus, robust standard errors are clustered at the state level and population-weighted.[19]

One potential shortcoming of the specification in (1) is that it doesn't allow for state-specific trends in crime that could impact a state's likelihood of adoption of an

[16] One may also be concerned that due to prosecutorial discretion (see e.g., Bjerk 2005) the law changes may be undone by lack of enforcement. While this is an important consideration for a number of law changes, add-on gun laws were of such large magnitude, so pervasive and popular that it would have been practically difficult for them to be completely nullified by prosecutors. Nonetheless, the estimates presented here should thus be interpreted as a lower bound of the full effect of a sentencing change.

[17] Since there are only two examples of repeals of add-on gun laws (California in 1977 and Tennessee in 1989), there will be tremendous autocorrelation in this variable. This makes standard errors prone to potential underestimation, as discussed in Bertrand, Duflo, and Mullainathan (2004). Both clustering standard errors by state and estimating standard errors using placebo laws (reported in the online Appendix) are used to correct this problem.

[18] Data for control variables were kindly made available by John Donohue.

[19] Regression estimates are population-weighted because the true impact should be at the level of an individual. Thus, larger agencies have more impact on the overall point estimates than smaller ones. The online Appendix includes unweighted estimates.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

add-on gun law. Adding these trends reduces the burden of exogeneity of the add-on laws. Now, the timing must simply be exogenous once state-specific crime trends are accounted for. The following specification adds the state-time trends ($\omega_s t$):

$$\mathbf{y}_{at} = \beta \mathbf{Addon}_{st} + \boldsymbol{\lambda}_s + \boldsymbol{\gamma}_t + \boldsymbol{\omega}_s t + \mathbf{x}_{st} + \mathbf{mm}_{st} + \boldsymbol{\varepsilon}_{at}. \tag{2}$$

This specification is also estimated using robust, population-weighted errors.

B. *Event Study*

To obtain a more precise understanding of the impact that add-on gun laws have year-by-year after their effective dates, it is useful to group agencies together according to the time period relative to the add-on date in their state. This results in an event study methodology similar to that employed by Jacobson, Lalonde, and Sullivan (1993) in order to identify earnings losses of displaced workers:

$$\mathbf{y}_{at} = \sum_{i \geq -n} \boldsymbol{\beta}_i \mathbf{D}^i_{st} + \boldsymbol{\lambda}_s + \boldsymbol{\gamma}_t + \boldsymbol{\omega}_s t + \mathbf{x}_{st} + \boldsymbol{\varepsilon}_{at}. \tag{3}$$

The outcome, as before, is a measure of the crime rate at the agency level, and $\boldsymbol{\lambda}_s$, $\boldsymbol{\gamma}_t$, $\boldsymbol{\omega}_s$, $\mathbf{x}_{st}$, and $\boldsymbol{\varepsilon}_{at}$ are as described above. The major distinction is that now there are multiple variables of interest, the $\boldsymbol{\beta}_i$, which indicate the impact of the add-on gun law at various different times relative to the law's effective date. The $\mathbf{D}^i_{st}$ are dummy variables that are one in state s if period t is exactly i periods after the effective date in that state, and zero otherwise. For example, in Arkansas the add-on year is 1981, so the $i = 3$ dummy will be 1 in 1984. The relative time index, i, may take on negative values to allow for any potential effects prior to the add-on date. This methodology is powerful because it conveys a lot of information about the dynamics of the response to the add-on gun laws. The results from these regressions are reported in Figures 4 and 5.[20]

III. Empirical Findings

A. *Main Results*

The first empirical results are from a regression of reported log gun robberies per capita on post add-on dummies, using the specification in Equation (1). Table 3 presents the results, with each column representing a separate regression. "Balanced panel" has the same meaning as in the summary statistics: 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. 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

[20] Several other specifications are described and their results reported in the online Appendix. These include ones that allow for lagged dependent variables, changes in slope, and triple-difference using the magnitude of the add-on gun law penalty in a state as the third dimension.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 3—IMPACT OF ADD-ON GUN LAWS ON GUN ROBBERY RATES

Dependent variable: Log reported gun robberies per 100,000 residents	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
One year impact								
After add-on law effective date	−0.0447	−0.0610	−0.0477	−0.0223	−0.0613	−0.0661	−0.0435	−0.0125
	(0.0591)	(0.0386)	(0.0310)	(0.0193)	(0.0654)	(0.0460)	(0.0267)	(0.0217)
After MM law effective date	−0.0350	0.00293	−0.119	−0.128	0.00896	−0.0697	−0.0704	−0.0994
	(0.0680)	(0.0765)	(0.0991)	(0.0919)	(0.0621)	(0.0695)	(0.158)	(0.103)
Two year impact								
After add-on law effective date	−0.116	−0.141**	−0.110**	−0.0782**	−0.136	−0.148**	−0.0958**	−0.0578*
	(0.0656)	(0.0413)	(0.0336)	(0.0191)	(0.0733)	(0.0485)	(0.0247)	(0.0221)
After MM law effective date	−0.0262	0.0221	−0.0981	−0.105	0.0226	−0.0512	−0.0489	−0.0930
	(0.0673)	(0.0730)	(0.0963)	(0.0910)	(0.0606)	(0.0650)	(0.152)	(0.104)
Three year impact								
After add-on law effective date	−0.142	−0.173**	−0.127**	−0.0903**	−0.168	−0.179**	−0.108**	−0.0541*
	(0.0776)	(0.0524)	(0.0406)	(0.0245)	(0.0859)	(0.0612)	(0.0300)	(0.0244)
After MM law effective date	−0.0191	0.0392	−0.0956	−0.103	0.0350	−0.0358	−0.0435	−0.0964
	(0.0661)	(0.0676)	(0.0958)	(0.0907)	(0.0582)	(0.0585)	(0.148)	(0.105)
State-specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
Balanced panel	No	No	Yes	Yes	No	No	Yes	Yes
Restrict to post-1974	No	No	No	No	Yes	Yes	Yes	Yes
Observations	15,516	15,516	2,975	2,975	12,979	12,979	2,234	2,234
R^2	0.168	0.179	0.182	0.190	0.165	0.175	0.182	0.187

Notes: This table reports the impact of add-on gun laws on gun robbery rates within one, two, or three years of the law change. 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. All specifications include state and year fixed effects. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

the first two years following the add-on law effective date, and zero otherwise. All errors reported allow for intra-state correlation and are weighted by state population. All specifications included state and year effects, and the controls discussed in Section II.

Log per capita crime rate is the preferred dependent variable in this paper and this preference may be illustrated by the following example. Assume Miami has a pre-gun law level of 200 gun robberies per 100,000 residents and Phoenix 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.[21]

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 percent level after 2 or 3 years. The coefficients in

[21] I also run regressions using per capita crime data as the dependent variable. These results are reported in the online Appendix.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

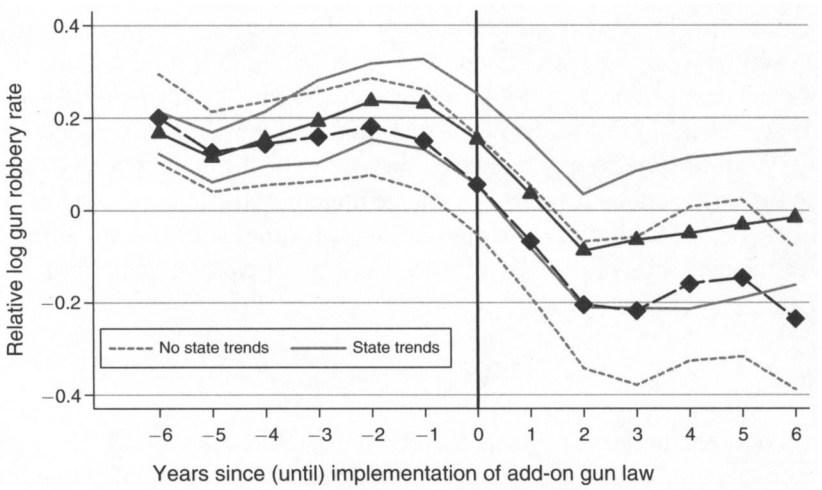

FIGURE 4. GUN ROBBERY RATE RELATIVE TO ADD-ON GUN LAW EFFECTIVE DATE

Notes: All regressions control for state and year fixed effects. For details, see tables.

Table 3 yield an estimate of the magnitude of the impact of the impact. Although the coefficients vary somewhat across specifications, there is a decline of 6–14 percent within the first 2 years and 5–18 percent within the first 3 years of introduction of the law. The preferred specification is the most conservative, with a balanced-panel restricted to post-1974 data and including state-specific time trends.[22] For this specification there is an impact which seems to level off to 6 percent within 2 years, and 5 percent within 3 years.

In order to gain more information on the timing of the impact of the law change, I estimate equation (3) using log per capita gun robberies as the dependent variable. The results, reported in Figure 4, support the findings discussed above.[23] Gun robbery 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 three years and then level out.[24] One surprising feature of Figure 4 is that it appears that the downward trend may begin slightly *before* the effective date. I discuss the timing of the impact of the law in Section IVB.

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 belie this interpretation, and they are addressed at

[22] Note that the post-1974 specification is identified off of 20 state law changes that were made after that year. I ran several regressions using each of the control variables used in the main specifications (poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population) to test whether they differed systematically from other states and found no significant difference.

[23] The absolute values on the y-axis of this and other figures are not meaningful in themselves (since they come from regressions that include a number of regressors with nonzero means), but the changes are.

[24] In additional specifications (available from the author), I examine the impact of the law change lasting up to six years, which would presumably include incapacitation effects as well. I find the overall decline in crime grows to about 10 percent in the most conservative specification, indicating that the importance of the incapacitation effect is of the same order as deterrence.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

length in Section IV. 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, I have attempted 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.[25] 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.

B. *Gun Assaults*

If the economic model of crime is correct, one should observe a deterrent effect of add-on gun laws on all types of gun crimes. The other category of crime for which weapon type is reported in the UCR is assault. Assaults are often considered to be "crimes of passion," and thus may not be as well described by the economic model of crime.[26] Nevertheless, one might expect that some fraction of assaults do have an indirect economic motive, or at least respond to changes in penalties.

Using UCR data on reported gun assaults, I test for a deterrent effect of add-on gun laws on gun assaults, and find no significant effect. Table 4 reports results from specifications described by equations (1) and (2) for two and three years after the add-on law. The estimates are all negative, but statistically indistinguishable from zero. The coefficients are all substantially smaller than those for gun robberies. Taken together, this suggests the possibility of a weak deterrent effect for gun assaults of about 1 to 3 percent. But the current study lacks the power to confirm the statistical significance of this effect.

C. *Other Crimes*

While economic theory clearly predicts a negative relationship between the presence of add-on gun laws and gun crimes, the prediction is less clear for nongun crimes. Add-on gun laws will increase nongun crimes if guns and other weapons are good substitutes and criminals shift toward other weapons or types of crime as the cost of using a gun increases. Alternatively, add-on gun laws may reduce non-gun crimes if individuals choose whether or not to be a generalist career criminal based on the total expected returns of criminal and alternative careers. If there is some upfront investment necessary to enter the criminal sector (e.g., gang initiation) or to improve general skills one would observe a correlation across some types of crime rates. Decreased expected returns due to add-on gun laws could lead some potential

[25] Most of the coefficients are relatively stable when state-year trends are added. Remaining concern about contemporaneous policy changes affecting the results may be addressed by the triple difference specification, addressed below.

[26] See e.g., Gould, Weinberg, and Mustard (2002), and Silverman (2004), both of which note that assaults and some other types of violent crimes often have primarily nonpecuniary motives. Of course, this doesn't imply that harsher sanctions will have no effect even on these crime rates, but suggests that the effect may be smaller.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 4—IMPACT OF ADD-ON GUN LAWS ON GUN ASSAULT RATES

Dependent variable: Log reported gun assaults per 100,000 residents								
Post-law change window	Two years				Three years			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post law change	−0.0360	−0.0139	−0.0316	−0.00181	−0.0367	−0.0104	−0.0355	−0.00819
	(0.0225)	(0.0159)	(0.0201)	(0.0133)	(0.0285)	(0.0213)	(0.0248)	(0.0213)
State-specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
Restrict to post-1974	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2,964	2,964	2,223	2,223	2,964	2,964	2,223	2,223
R^2	0.368	0.372	0.395	0.398	0.368	0.372	0.395	0.398

Notes: This table reports the impact of add-on gun laws on gun assault rates within two or three years of the law change. 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. All specifications include state and year fixed effects. Each specification contains seven years of data prior to the law change. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

criminals to stay in the legitimate sector or some current criminals to shift into the legitimate sector, and thus reduce levels of all types of crime.

The degree of correlation in crime rates will vary by type of crime. In order to determine which types of crime rates are likely to covary the most and least, I analyzed data with complete criminal histories for almost 40,000 prisoners from 15 states.[27] Using this data, for each type of crime I calculated both the unconditional probability of an offender committing it and the probability of commission conditional on an offender committing a gun robbery. The ratio of these two probabilities is the strength of association between a crime and gun robbery. I find that of the UCR crime categories, nongun robberies and larcenies are by far the most likely to have been committed by individuals who have been convicted of gun robbery. In addition, I found that murder and rape are the crimes least likely to have been committed by a gun robbery (among those for which I had data). I use this information to further test the generalist career criminal model.

Data from both nongun robberies and larcenies appear to support the career criminal model over the substitution model. Table 5 reports the effect of add-on gun laws on robberies using weapons other than guns and on larcenies. There is no evidence in any of the regressions for the substitution model, as all estimates for the short-term impact of add-on gun laws on nongun robberies and larcenies are negative. Not surprisingly, the effect of nongun robberies is not as substantial (or significant) as that on gun robberies, with the preferred specification yielding a three year impact of just over 3 percent. The point estimates obtained for larcenies are similar, although slightly smaller in magnitude.

While I find an effect of add-on gun laws for nongun robberies and larcenies, the generalist criminal theory would not predict an impact on rapes or murders because they are not very associated with gun robberies (or gun assaults). Table 6 reports

[27] The data is from "Recidivism of Prisoners Released in 1994" produced by the US Bureau of Justice Statistics.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 5—IMPACT OF ADD-ON GUN LAWS ON NONGUN ROBBERY AND LARCENY RATES

	Nongun robberies				Larceny			
Type of crime	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post law change	−0.0323	−0.0416*	−0.0559**	−0.0338	−0.0287	−0.0229	−0.0291*	−0.0246
	(0.0301)	(0.0196)	(0.0150)	(0.0188)	(0.0171)	(0.0127)	(0.0111)	(0.0147)
State-specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
Restrict to post-1974	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2,911	2,911	2,209	2,209	2,971	2,971	2,230	2,230
R^2	0.252	0.258	0.232	0.232	0.259	0.268	0.256	0.260

Notes: This table reports the impact of add-on gun laws on nongun robbery and larceny rates within three years of the law change. Dependent variable is the log reported crime rate per 100,000 residents. 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. All specifications include state and year fixed effects. Each specification contains seven years of data prior to the law change. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

TABLE 6—IMPACT OF ADD-ON GUN LAWS ON RAPE AND MURDER RATES

	Rape				Murder			
Type of crime	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post law change	−0.00672	−0.00598	−0.00861	−0.000418	−0.0311	−0.0256	−0.0312	−0.0174
	(0.0215)	(0.0212)	(0.0192)	(0.0209)	(0.0248)	(0.0249)	(0.0237)	(0.0306)
State-specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
Restrict to post-1974	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2,935	2,935	2,216	2,216	2,967	2,967	2,234	2,234
R^2	0.199	0.205	0.207	0.209	0.168	0.170	0.161	0.163

Notes: This table reports the impact of add-on gun laws on rape and murder rates within three years of the law change. Dependent variable is the log reported crime rate per 100,000 residents. 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. All specifications include state and year fixed effects. Each specification contains seven years of data prior to the law change. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

results from regressions where these are the dependent variables. As expected, neither of the crimes show a statistically significant impact of the add-on gun laws within two or three years of the law change. All of the point estimates for rape are less than 1 percent. While the point estimates for murder are larger, none are statistically significant.

It appears that not only gun crimes, but other crimes that tend to be committed by gun robbers are impacted by the implementation of add-on gun laws. At the same time, uncorrelated crimes do not appear to be affected by the law change. As discussed above, these results support the career criminal hypothesis. The findings do not imply that there is no substitution away from guns to other weapons; simply that these effects are outweighed by the overall decline in robberies. There are also other potential explanations for the similar time pattern of crime reduction. For example, misclassification of some fraction of gun robberies as nongun robberies could lead to these results. Other possibilities could include a contemporaneous law enforcement

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

change (like a broad crackdown on crime) or a mean-reverting crime process with endogenous legislative implementation of add-on gun laws. These potential confounds and a number of specification checks are discussed in Section IV and in the online Appendix.

IV. Addressing Potential Concerns

A. *Crime Trends and Contemporaneous Policy Changes*

To this point, I have attempted to isolate the deterrent effect of add-on gun laws by using a long crime panel and a repeated natural experiment, which allows for the inclusion of state and time controls as well as state-specific trends. One still may be concerned that some or all of the effect that has been estimated is from contemporaneous policy changes or that the timing of the laws is not exogenous. Although the regression results indicate a significant decrease in crime after the effective date of the law change, there may be concern that states pass laws in response to a run-up in crime, and the decline is simply reflecting mean reversion in crime rates. The fact that a decline in crime subsequent to the effective date of the law persists even when state trends are included casts some doubt on this explanation. This still leaves open the possibility that laws are passed in response to changes in the crime trend or that other law changes are responsible for the detected effect.

Some states made other criminal law changes around the same time as the add-on laws were passed, most commonly mandatory minimums (in about half the states). I have attempted to control for these law changes by including a dummy variable for their presence in all of the main specifications (see also Section IVC below). Sentencing guidelines are rarely adopted within a year of add-ons, with only two states (North Carolina and Washington) having done so (Table 1). Anecdotally, add-ons are sometimes adopted as a legislative response to particularly horrific gun crimes. State legislatures do not control local law enforcement agencies, and thus changes in policing are not likely to coincide with law changes (unless both are responding to crime trends).[28]

Another way to isolate the impact of add-on gun laws is through a triple-difference specification, where the third difference is between crime rates for a gun crime and those for a nongun crime (i.e., non-gun robbery or larceny) that otherwise would have similar time trends to the gun crime. The results from these regressions are reported in Table 7 using nongun robberies and larcenies as the unaffected crime. The identifying assumption in these regressions is that gun robberies would have experienced the same time evolution as the control crimes if not for the add-on gun law.[29] Across all specifications, the triple difference shows a decline in gun

[28] I also investigate potential changes in policing by performing the main analysis but using gun robbery *arrests* rather than reports as the dependent variable. I find effects that are not significantly different from zero, but also cannot rule out that they are of the same magnitude as the effects for reported gun robberies. This implies either no change in the effect of policing or at most a small one.

[29] This assumption necessitates using control crimes that are similar to gun robberies, which is why nongun robberies and larcenies were chosen. As discussed, however, there may be a direct effect of the law on these crimes as well, in which case the triple difference will understate the magnitude of the effect of the law change.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 7—TRIPLE DIFFERENCE IMPACT OF ADD-ON LAW

	Nongun robbery				Larceny			
Control crime	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
After add-on law effective date	−0.0802*	−0.0405	−0.0565	−0.0238	−0.0914*	−0.0629*	−0.0719*	−0.0263
	(0.0346)	(0.0214)	(0.0278)	(0.0267)	(0.0334)	(0.0231)	(0.0296)	(0.0247)
State-specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
Restrict to post-1974	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2,911	2,911	2,209	2,209	2,971	2,971	2,230	2,230
R^2	0.523	0.553	0.392	0.409	0.302	0.310	0.287	0.292

Notes: This table reports the impact of add-on gun laws on the difference between rates of gun robbery and the control crime within three years of the law change. Dependent variable is the log reported crime rate per 100,000 residents. 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. All specifications include state and year fixed effects. Each specification contains seven years of data prior to the law change. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

robberies due to the law change, although one that is statistically insignificant for some of the specifications. This provides further evidence that the add-on law had a deterrent effect.

B. *Timing of the Decline in Crime*

Thus far I have presented evidence for a deterrent effect of add-on gun laws, leading to a decrease in gun robberies per capita of about 5 percent within the first 3 years of passage. One potential concern regards the timing of the decrease in crime: there is a slight (statistically insignificant) decline in gun robberies prior to the effective date of the add-on law, even when controlling for state-specific time trends (Figure 4).

One possible interpretation of the slight decrease in crime pre-law change is that potential offenders learn about the law through ongoing public debate and discussion and modify their behavior in anticipation of the law change.[30] The process by which potential offenders learn about criminal sanctions is not well studied. There is some evidence (Pogarsky, Piquero, and Paternoster 2004; Tunnell 1996) that potential criminals often have very noisy information about penalties they may face. Other work (Cook 1980) suggests that potential criminals may learn of law changes through the media and will change behavior even with imperfect knowledge of new

[30] In order to gain a better understanding of when potential offenders are likely to have learned of add-on gun laws, I collected data from local newspapers about gun legislation. This turns out to be a difficult task. For each of the 30 states that ever passed add-on gun laws, I searched for newspaper article availability for the largest newspaper in the state capital and in the largest city. Although most newspapers have digitized archives going back to the 1990s, because many of the add-on laws were passed earlier, data was only available from eight newspapers, representing six states, around the time of the add-ons. For these newspapers, searches were run with various permutations of the terms firearm, gun, add-on, mandatory minimum, law, and legislation, in order to determine which period had the greatest news coverage of the law change. There was weak evidence of more publicity around the date of passage, but insufficient power to find statistical significance.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

sanctions. Discussions the author's had with criminal defendants and public defenders indicate that at least some defendants are aware of sanctions.[31]

Imperfect knowledge of law changes may lead to a weakened overall deterrent effect, and also to a modification of the timing in the response to penalty changes. All specifications presented thus far have used the law's effective date as the key independent variable. But this date is often months or even years after the law has been debated in the legislature. New laws are likely to receive the most publicity and have the greatest effect on behavior around the date of legislative introduction or passage (see footnote 30). To test this hypothesis, I collected the dates of legislative bill introduction or passage (the former are difficult to obtain for a number of states, but the latter may be found in state codes or legislative histories) and report these in Table 1.

I replicate the regressions above using the date of bill passage instead of implementation and find a somewhat shifted time structure of the crime response, relative to the previous specifications (Figure 5). The greatest declines in gun robbery rates occur in the first two years following the date of passage of the law. The point estimates using date of passage are very similar to those reported above using the effective date of the law change. This supports the hypothesis above regarding the timing of criminal response corresponding more closely to the date of passage.

Although the add-on law is not yet effective, there are two mechanisms that could account for an immediate reduction in crime. First, a forward-looking fully rational individual considering a career in crime should respond to the knowledge of the penalty change, even though it is not yet effective, by not making investments related to a criminal career. Second, the information a potential criminal receives about the law change may be imperfect. For example, the potential offender may hear about the add-on law when it is publicized through the media, and may assume that it is effective immediately. I do not attempt to distinguish these explanations here, and continue to use the timing of the base specification for all other regressions.

C. *Impact of Mandatory Minimum Laws*

One of the most significant potential confounds of the deterrence interpretation is the possibility of other policy changes contemporaneous with add-on gun laws. The most likely candidate for such a contemporaneous change is a mandatory minimum law. Many of the states that adopted add-on gun laws also adopted another type of law aimed at reducing gun violence, mandatory minimums. These laws provide for a lower bound on sentences for crimes involving the use of a firearm. As discussed previously, since mandatory minimums are often not binding, it makes a deterrence interpretation problematic.

I test for an impact of mandatory minimum laws using the same methodology as used for add-on gun laws. Table 3 presents coefficients on mandatory minimum law dummies in regressions including add-on law dummies as well. Although a few of

[31] An example of a media source that provides information on gun laws is Don Diva, a hip-hop magazine that has run articles entitled "What are Mandatory Minimums?" and "What Every Gangster Needs to Know."

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

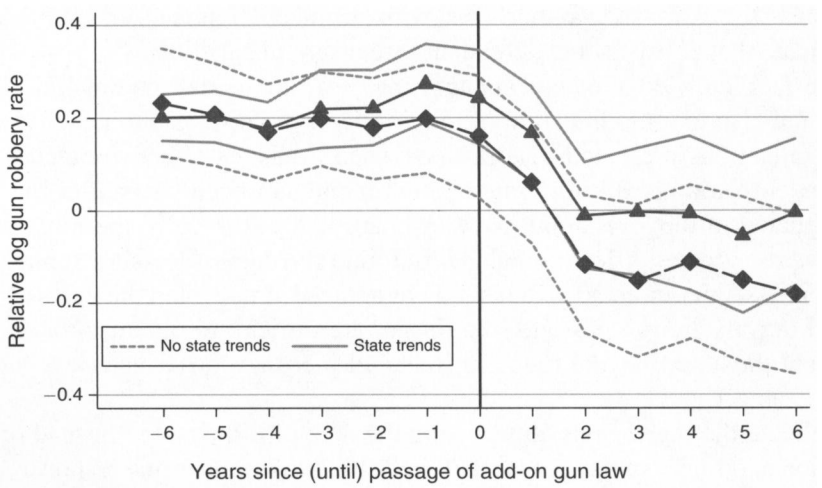

FIGURE 5. GUN ROBBERY RATE RELATIVE TO ADD-ON GUN LAW PASSAGE DATE

Notes: All regressions control for state and year fixed effects. For details, see tables.

the coefficients on the mandatory minimum dummies are large, none are statistically significant. The same results were found when running specifications including only mandatory minimum dummies, without those for add-on laws.[32] Mandatory minimums appear to have, at best, a weak effect on gun robberies, thus ruling out this policy change as the driver of the main results.

D. *Further Test of Generalist Criminal Theory*

In addition to the evidence previously presented supporting the notion of a generalist criminal, I report one additional analysis here. If gun robbers tend to commit multiple types of crimes, then those jurisdictions with the largest fraction of gun robbers should also show the greatest decrease in associated crimes. As discussed above, nongun robbery and larceny appear to be the crimes most commonly committed by gun robbers. Table 8 reports the effect of add-on gun laws on these crimes, for jurisdictions with above and below median share of gun robberies.

Panel A reports the results for nongun robberies, which in most specifications drop less in low gun robbery cities than in high, although the difference is not statistically significant. The difference is much more pronounced when examining the change in larcenies in panel B. There is a decline in larcenies of 3–6 percent in high gun robbery cities, and only 0–2 percent in low gun robbery cities. This provides further support of the notion that the impact of add-on gun laws has positive spillovers through a reduction in crimes that tend to be committed by gun robbers.

[32] Additionally, omitting the mandatory minimum dummies from regressions using the add-on dummies has a very small effect on the add-on coefficients and no impact on their statistical significance.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

TABLE 8—TEST OF GENERALIST CRIMINAL MODEL

	High				Low			
Gun robbery ratio	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Nongun robberies								
After add-on law effective date	−0.07	−0.04	−0.08**	−0.05	−0.05	−0.07*	−0.06*	−0.02
	(0.045)	(0.032)	(0.025)	(0.029)	(0.026)	(0.029)	(0.026)	(0.020)
Observations	1,442	1,442	1,066	1,066	1,469	1,469	1,143	1,143
R^2	0.383	0.396	0.368	0.370	0.473	0.477	0.420	0.422
Panel B. Larcenies								
After add-on law effective date	−0.0628**	−0.0299	−0.0513**	−0.0557**	−0.00700	−0.0237	−0.0209	−0.00175
	(0.0181)	(0.0180)	(0.0147)	(0.0169)	(0.0189)	(0.0160)	(0.0145)	(0.0159)
Observations	1,490	1,490	1,084	1,084	1,481	1,481	1,146	1,146
R^2	0.382	0.389	0.374	0.378	0.470	0.489	0.477	0.483
State-specific time trends	No	Yes	No	Yes	No	Yes	No	Yes
Restrict to post-1974	No	No	Yes	Yes	No	No	Yes	Yes

Notes: This table reports the impact of add-on gun laws on the non-gun robbery and larceny rates within three years of the law change. Dependent variable is the log reported crime rate per 100,000 residents. The data consists of agency-year level observations and is reported by the share of gun robberies in an agency relative to the median agency. Standard errors reported in parentheses are clustered at the state level to allow for intra-state correlation in error structure. All specifications include state and year fixed effects. Each specification contains seven years of data prior to the law change. Controls include poverty rate, unemployment rate, racial composition, age composition, lagged police population share, and lagged imprisoned population share.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

V. Conclusion

The question of how to best reduce crime is one of perennial importance, made even more salient during periods of budgetary strain. Incarceration is currently by far the most favored method to reduce crime in the United States, and it acts primarily through two channels: incapacitation and deterrence. Disentangling the relative contributions of the two channels is of primary significance in establishing sensible sentencing policies.

In this paper, I use the introduction of add-on gun laws to isolate the deterrent effect of incarceration. Since defendants sentenced under add-on gun laws receive sentences of several years for their underlying crime, any impact on crime within the first several years of an add-on gun law may be interpreted as due solely to its deterrent effect.

I find that this effect on gun robberies is significant, with a per capita reduction of 5 percent within 3 years of the law's effective date. This reduction in gun robberies does not seem to come at substantial expense from criminals substituting to other types of crime. Nongun robberies and larcenies display a weaker response to add-on laws, but in the same direction, supporting the notion that add-on gun laws may have positive, not negative spillovers.

While it is difficult to completely rule out that passage of add-on gun laws is endogenous, or that contemporaneous policy changes may be responsible for some of the findings, I present substantial evidence addressing these concerns. Numerous alternate specifications are explored to attempt to verify the robustness of the central findings. Contemporary newspaper data suggests that legislative action is often spurred by

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

idiosyncratic crimes. Triple differences and an analysis of related and unrelated crimes reinforce the central finding of deterrence and point toward generalist career criminals.

Previous research into deterrence has often been limited to single jurisdictions or has been unable to make use of natural experiments to establish a causal relationship. This paper should help solidify our evidence for deterrence from incarceration. While the jurisdictions vary, it is useful to compare the magnitude of the estimates found in this paper with others. The 5 percent 3-year decline in this paper is close in magnitude to the 8 percent drop found by Kessler and Levitt (1999). Since the magnitude of sentence enhancements in that paper are similar to gun add-ons, this is an encouraging result.

Other papers use sentencing changes that are substantially different from those in this paper, and so a comparison of elasticities is more illuminating. A quick back of the envelope calculation yields an elasticity of approximately -0.10 in the current paper. This magnitude is consistent with that found by Lee and McCrary (2011). They bound allowable elasticities consistent with their data and model to have a magnitude no greater than -0.13, although their preferred parameter values yield elasticities close to 0. The largest recent empirical elasticity estimates come from Drago et al. (2009) using Italian data, where they find a magnitude of -0.74 for 7 months. 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 -0.07.

The main finding in this paper is of a robust deterrent effect of incarceration. As the preceding discussion illustrates, the magnitude of the effect found here is consistent with some prior results from individual jurisdictions, although there is a wide range of estimates. In looking toward future research and implications for policy, one must recognize that understanding the magnitude of deterrence, and not just its existence, is paramount.

REFERENCES

Abrams, David S. 2012. "Estimating the Deterrent Effect of Incarceration Using Sentencing Enhancements: Dataset." *American Economic Journal: Applied Economics*. http://dx.doi.org/10.1257/app.4.4.32.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119 (1): 249–75.

Bjerk, David. 2005. "Making the Crime Fit the Penalty: The Role of Prosecutorial Discretion under Mandatory Minimum Sentencing." *Journal of Law and Economics* 48 (2): 591–625.

Chen, M. Keith, and Jesse M. Shapiro. 2007. "Do Harsher Prison Conditions Reduce Recidivism? A Discontinuity-Based Approach." *American Law and Economics Review* 9 (1): 1–29.

Cook, Philip J. 1980. "Research in Criminal Deterrence: Laying the Groundwork for the Second Decade." *Crime and Justice* 2 (1): 211–68.

DiIulio, John J., Jr., and Anne Morrison Piehl. 1991. "Does Prison Pay? The Stormy National Debate over the Cost-Effectiveness of Imprisonment." *Brookings Review* 9 (4): 28–35.

Doob, Anthony N., and Cheryl Marie Webster. 2003. "Sentence Severity and Crime: Accepting the Null Hypothesis." *Crime and Justice* 30 (1): 143–95.

Drago, Francesco, Roberto Galbiati, and Pietro Vertova. 2009. "The Deterrent Effects of Prison: Evidence from a Natural Experiment." *Journal of Political Economy* 117 (2): 257–80.

Ehrlich, Isaac. 1973. "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation." *Journal of Political Economy* 81 (3): 521–65.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

Ehrlich, Isaac. 1975. "The Deterrent Effect of Capital Punishment: A Question of Life and Death." *American Economic Review* 65 (3): 397–417.

Ehrlich, Isaac. 1981. "On the Usefulness of Controlling Individuals: An Economic Analysis of Rehabilitation, Incapacitation, and Deterrence." *American Economic Review* 71 (3): 307–22.

Gould, Eric D., Bruce A. Weinberg, and David B. Mustard. 2002. "Crime Rates and Local Labor Market Opportunities in the United States: 1979–1997." *Review of Economics and Statistics* 84 (1): 45–61.

Helland, Eric, and Alexander Tabarrok. 2004. "Using Placebo Laws to Test 'More Guns, Less Crime.'" *Advances in Economic Analysis and Policy* 4 (1): 1–7.

Helland, Eric, and Alexander Tabarrok. 2007. "Does Three Strikes Deter: A Non-Parametric Investigation." *Journal of Human Resources* 42 (2): 309–30.

Hjalmarsson, Randi. 2009. "Crime and Expected Punishment: Changes in Perceptions at the Age of Criminal Majority." *American Law and Economics Review* 11 (1): 209–48.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993. "Earnings Losses of Displaced Workers." *American Economic Review* 83 (4): 685–709.

Katz, Lawrence, Steven D. Levitt, and Ellen Shustorovich. 2003. "Prison Conditions, Capital Punishment, and Deterrence." *American Law and Economics Review* 5 (2): 318–43.

Kessler, Daniel P., and Steven D. Levitt. 1999. "Using Sentence Enhancements to Distinguish between Deterrence and Incapacitation." *Journal of Law and Economics* 42 (1): 343–63.

Kessler, Daniel P., and Anne Morrison Piehl. 1998. "The Role of Discretion in the Criminal Justice System." *Journal of Law, Economics, and Organization* 14 (2): 256–76.

Lee, David S., and Justin McCrary. 2011. "The Deterrence Effect of Prison: Dynamic Theory and Evidence." Princeton Industrial Relations Section Working Paper 550.

Levitt, Steven D. 1996. "The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation." *Quarterly Journal of Economics* 111 (2): 319–51.

Levitt, Steven D. 1998a. "Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?" *Economic Inquiry* 36 (3): 353–72.

Levitt, Steven D. 1998b. "Juvenile Crime and Punishment." *Journal of Political Economy* 106 (6): 1156–85.

Levitt, Steven D., and Thomas J. Miles. 2007. "Empirical Study of Criminal Punishment." In *Handbook of Law and Economics*, edited by A. Mitchell Polinsky and Steven Shavell, 455–95. Amsterdam: Elsevier.

Maltz, Michael D., and Joseph Targonski. 2004. "Making UCR Data Useful and Accessible." Department of Criminal Justice. Chicago, February. https://www.ncjrs.gov/pdffiles1/nij/grants/205171.pdf. (accessed March 10, 2010).

Marvell, Thomas B., and Carlisle E. Moody. 1995. "The Impact of Enhanced Prison Terms for Felonies Committed with Guns." *Criminology* 33 (2): 247–81.

Marvell, Thomas B., and Carlisle E. Moody. 2001. "The Lethal Effects of Three-Strikes Laws." *Journal of Legal Studies* 30 (1): 89–106.

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

Owens, Emily G. 2009. "More Time, Less Crime? Estimating the Incapacitative Effect of Sentence Enhancements." *Journal of Law and Economics* 52 (3): 551–79.

Pogarsky, Greg, Alex R. Piquero, and Ray Paternoster. 2004. "Modeling Change in Perceptions about Sanction Threats: The Neglected Linkage in Deterrence Theory." *Journal of Quantitative Criminology* 20 (4): 343–69.

Polinsky, A. Mitchell, and Steven Shavell. 1999. "On the Disutility and Discounting of Imprisonment and the Theory of Deterrence." *Journal of Legal Studies* 28 (1): 1–16.

Robinson, Paul H., and John M. Darley. 2004. "Does Criminal Law Deter? A Behavioural Science Investigation." *Oxford Journal of Legal Studies* 24 (2): 173–205.

Silverman, Dan. 2004. "Street Crime and Street Culture." *International Economic Review* 45 (3): 761–86.

Tonry, Michael. 1992. "Mandatory Penalties." *Crime and Justice* 16 (2): 243–73.

Tunnell, Kenneth D. 1996. "Choosing Crime: Close Your Eyes and Take Your Chances." In *Criminal Justice in America: Theory, Practice, and Policy*, edited by Barry W. Hancock and Paul M. Sharp, 38–50. Upper Saddle River: Prentice-Hall.

Vernick, Jon S., and Lisa M. Hepburn. 2003. "State and Federal Gun Laws: Trends for 1970-99." In *Evaluating Gun Policy: Effects on Crime and Violence*, edited by Jens Ludwig and Philip J. Cook, 345–402. Washington, DC: Brookings Institution Press.

Waldfogel, Joel. 1993. "Criminal Sentences as Endogenous Taxes: Are They 'Just' or 'Efficient?'" *Journal of Law and Economics* 36 (1): 139–51.

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms

Walmsley, Roy. 2009. "World Prison Population List." International Centre for Prison Studies. London, January. http://www.kcl.ac.uk/depsta/law/news/news_details.php?id=203. (accessed December 21, 2011).

Webster, Cheryl Marie, 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 (3): 417–48.

Wolfers, Justin. 2006. "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review* 96 (5): 1802–20.

US Bureau of Justice Statistics. 1994. "Recidivism of Prisoners Released in 1994." Inter-University Consortium for Political and Social Research, Ann Arbor, MI. ICPSR03355-v7. doi:10.3886/ICPSR03355.v7. (accessed March 8, 2011).

US Bureau of Justice Statistics. 1998. "National Crime Surveys: National Sample, 1973–1983 Codebook." Inter-University Consortium for Political and Social Research, Ann Arbor, MI. doi:10.3886/ICPSR07635.v6. (accessed December 31, 2011).

US Bureau of Justice Statistics. 2011a. *Correctional Populations in the United States*. US Bureau of Justice Statistics. http://bjs.ojp.usdoj.gov/index.cfm?ty=pbse&sid=5. (accessed December 31, 2011).

US Bureau of Justice Statistics. 2011b. *Sourcebook of Criminal Justice Statistics*. US Bureau of Justice Statistics. http://bjs.ojp.usdoj.gov/index.cfm?ty=pbse&sid=46. (accessed December 31, 2011).

US Department of Justice, Federal Bureau of Investigation. 1965–2002. "Uniform Crime Reporting Program Data." Inter-University Consortium for Political and Social Research, Ann Arbor, MI. ICPSR09028-v5. (accessed September 30, 2005).

This content downloaded from 130.91.144.125 on Thu, 11 Aug 2016 18:17:29 UTC
All use subject to http://about.jstor.org/terms