DOES MORE CRIME MEAN MORE PRISONERS? AN INSTRUMENTAL VARIABLES APPROACH*

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ABSTRACT

This paper studies the mechanical theory of crime and incarceration—the notion that changes in imprisonment are partially determined by changes in crime rates. Previous studies found scant evidence supporting the mechanical theory. These studies, however, failed to properly control for simultaneity between incarceration rates and crime rates. While more crime may lead to larger prison populations, rising incarceration rates may deter crime. To address this bias, abortion rates in the 1970s are used as an instrument for crime in later decades. Abortion rates in the 1970s are correlated with crime in the 1990s but are unlikely to be otherwise related to incarceration or prison admissions rates in the 1990s. The instrumental variables approach finds that the estimated elasticity of prison admissions with respect to crime is approximately 1, in accord with the mechanical theory. This finding has important implications for understanding trends in the U.S. prison population.

I. INTRODUCTION

THE effect of crime rates on the prison population should be a mechanical one. As two criminologists note, “Imprisonment is a criminal sanction: its use will therefore fluctuate in direct proportion to changes in the level of the behavior to which it is designed to respond.”¹ While this theory could hardly be more straightforward, its empirical relevance is a matter of some controversy. Numerous empirical studies of the relationship between crime rates and the scale of imprisonment have found almost no evidence supporting

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¹ Franklin E. Zimring & Gordon J. Hawkins, *The Scale of Imprisonment* 121 (1991).

the hypothesis that more crime leads to higher incarceration rates.² This stark dissonance between theory and empirics has been called the “paradox in crime and incarceration.”³

The debate over the impact of crime rates on incarceration rates is part of a broader debate regarding rising incarceration rates in the United States.⁴ The prison population of the United States has burgeoned over the last 30 years, increasing almost fivefold between 1970 and 2000 and more than doubling between 1985 and 1997 (see Figure 1). Some criminologists attribute some (but certainly not all) of this massive increase in imprisonment (particularly in the 1980s) to an increase in the propensity to commit crime.⁵ Many others, however, dismiss this claim, citing the many studies that find no relationship between changes in crime rates and changes in incarceration.⁶ This debate has important policy ramifications; some commentators have pointed to the seemingly irrational relationship between imprisonment and crime as evidence of the generally perverse behavior of the prison system and have used this evidence to bolster their calls for an overhaul of the penal incarceration system in the United States.⁷ Thus, determining the validity of “mechanical theories” of imprisonment rates, such as the impact of crime rates on imprisonment rates, is a critical policy task.

There are several reasons why the effect of crime on imprisonment rates may be hard to identify, even if there is a link between the amount of crime and the number of prisoners. Most important, endogeneity bias confounds

² *Id.* at 121–25; Michael Tonry, Why Are U.S. Incarceration Rates So High? 45 *Crime & Delinq.* 421–22 (1999); Thomas B. Marvell & Carlisle E. Moody, Prison Population Growth and Crime Reduction, 10 *J. Quantitative Criminology* 109 (1994); Lee H. Bowker, Crime and the Use of Prisons in the United States: A Time-Series Analysis, 27 *Crime & Delinq.* 206 (1981); and William G. Nagel, On Behalf of a Moratorium on Prison Construction, 23 *Crime & Delinq.* 154, 160, 172–74 (1977), for examples of studies that find no effect of crime on imprisonment. Studies such as David Biles, Crime and the Use of Prisons, 43 *Fed. Probation* 39 (1979); Gary Sykes, Gennaro E. Vito, & Karen McElrath, Jail Populations and Crime Rates: An Exploratory Analysis, 15 *J. Police Sci. & Admin.* 72 (1987); Patrick A. Langan, America’s Soaring Prison Population, 251 *Science* 1568 (1991); and Marc Ouimet & Pierre Tremblay, A Normative Theory of the Relationship between Crime Rates and Imprisonment Rates: An Analysis of the Penal Behavior of U.S. States from 1972 to 1992, 33 *J. Res. Crime & Delinq.* 109, 114–24 (1996), find some (very weak) impact of crime on imprisonment. See also the literature review in a survey article by the Florida Corrections Commission, which concludes that “most studies found no relationship between the two rates [crime and imprisonment]; when a relationship is found, the results are mixed and weak.” Florida Corrections Commission, *The Relationship between Crime Rates and Incarceration Rates* (December 1994) (<http://www.fcc.state.fl.us/fcc/reports/rates/rateind.html>).

³ National Center for Policy Analysis, *Paradox in Crime and Incarceration* (1998) (<http://www.ncpa.org/pi/crime/jan98e.html>).

⁴ See Zimring & Hawkins, *supra* note 1, at 117–20; and Tonry, *supra* note 2, at 419–34, for overviews of this debate.

⁵ Steven D. Levitt, The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation, 111 *Q. J. Econ.* 319 (1996).

⁶ See Zimring & Hawkins, *supra* note 1, chap. 5, and the sources cited there.

⁷ See Nagel, *supra* note 2, at 174; and Zimring & Hawkins, *supra* note 1, chap. 5.

FIGURE 1.—Incarceration rates and crime rates

attempts to estimate the response of incarceration rates to crime rates. Since the number of prisoners affects the amount of crime, treating crime as an exogenous variable in a regression of imprisonment rates on crime rates is an inappropriate assumption. In addition, the relationship between the number of prisoners (or even the change in the number of prisoners) and the amount of crime is complex and dynamic.⁸ Since convicted criminals often spend more than 1 year in prison, the number of individuals in prison in any given year depends not only on the crime rate for that year but also on many lags of the crime rate.

This paper employs two strategies to confront the empirical complications that plague previous studies of the impact of crime on incarceration. The endogeneity biases are addressed using an instrumental variables approach. This paper uses abortion rates in the 1970s as an instrument for crime rates. John Donohue and Steven Levitt's seminal paper on abortion and crime demonstrated that abortion rates in the 1970s (which varied widely across states) are systematically related to changes in crime rates in the 1990s.⁹ Moreover, it seems quite plausible that, after controlling for state fixed effects,

⁸ See Bowker, *supra* note 2; and Marvell & Moody *supra* note 2, for time-series treatments of crime and imprisonment.

⁹ John J. Donohue III & Steven D. Levitt, *The Impact of Legalized Abortion on Crime*, 116 Q. J. Econ. 379 (2001).

abortion rates in the 1970s will be unrelated to incarceration rates in the 1990s (except through abortion's impact on crime).

To mitigate the difficulties associated with estimating the dynamic relationship between crime and incarceration, this paper focuses on admissions to prison rather than on the aggregate prison population. Unlike the total prison population, admissions to prison should depend primarily on crime rates in the present or recent past, thus moderating the need to examine the impacts of lagged values of crime on incarceration.¹⁰ While admissions to prison are clearly an incomplete element of the response of imprisonment to crime rates, they do constitute at least a partial test of a mechanical relationship between crime and imprisonment. Indeed, Patrick Langan notes that "prison population growth since 1973 has been driven by increases in prison admissions."¹¹

Using prison admissions data (but not instrumenting) rather than total imprisonment rates, this paper finds a significant relationship between crime rates and entrances to prison. Ordinary least squares (OLS) estimates of this relationship find an elasticity of about .5, which suggests that a 1 percent increase in crime is associated with a .5 percent increase in the number of prison admissions. While significant, this elasticity falls far short of the elasticity of 1 that is implied by a strictly mechanical relationship between crime and imprisonment.

The results obtained after instrumenting to control for endogeneity strengthen the hypothesis that crime rates mechanically influence incarceration rates. The instrumental variables estimates (which have high standard errors, particularly after adjustments to correct for serial correlation) find an elasticity of prison admissions with respect to crime of approximately 1; a 1 percent change in crime leads to a corresponding 1 percent change in admissions to prison. These results suggest that endogeneity bias is an important obstacle that plagued previous studies of this issue.

Intriguingly, an elasticity of 1 is implied by the simplest version of the mechanical hypothesis. If arrest rates and the probability of incarceration per arrest are insensitive to changes in the crime rate, then incarceration rates should move in direct proportion to crime rates, a prediction corroborated by the IV results found here.

These findings imply that the mechanical theory of imprisonment, so alluringly intuitive, has some empirical validity, contrary to the claims of some observers.¹² It is not quite a "theory in search of facts."¹³ *Ceteris paribus*, an

¹⁰ The advantages of focusing on admissions, however, come with a cost. Admissions to prison do not tell the full story of how prison populations change—since releases from prison are an equally important element. This paper will address the issue of releases below, although releases are subject to the questions about lagged crime rates.

¹¹ Langan, *supra* note 2, at 1572.

¹² See Tonry, *supra* note 2, at 421.

¹³ Zimring & Hawkins, *supra* note 1, at 119.

exogenous increase in the amount of crime will lead to a large increase in the number of people admitted to prison.

The United States probably did experience an exogenous increase in crime during the period from 1970 to 1997 as the number of crimes increased substantially in spite of large increases in incarceration rates and police forces.¹⁴ According to the results presented here, the increase in crime experienced from 1970 to 1997 should have led to an 80 percent increase in incarceration. This does not suggest, however, that the mechanical theory offers a complete explanation for the secular rise in imprisonment in the United States over the past 30 years. Incarceration rates increased almost fivefold, rather than by 80 percent. Instead, mechanical increases in imprisonment may constitute one of many important causes of the extraordinary rise in incarceration.

The paper proceeds as follows. Section II provides an informal framework for evaluating the relationship between crime and incarceration. Section III surveys and describes the data used in the analysis. In Section IV, simple OLS regressions estimating the impact of crime rates on incarceration are presented. Section V discusses the use of abortion as an instrument for crime and presents the instrumental variables estimates of the impact of crime rates on abortion rates. In Section VI, various aspects of the relationship between crime and incarceration are discussed to help determine how changes in crime rates affect releases from prison and overall incarceration rates. Section VII concludes.

II. ANALYTICAL FRAMEWORK

The notion that changes in crime rates should lead to changes in incarceration rates is an intuitive one. If the probability that a criminal is placed in prison is independent of the total number of crimes committed, then the number of prisoners admitted to prison should be a simple function of the amount of crime. For example, if a machine observed all crimes and randomly selected a certain proportion of them for incarceration, then imprisonment would rise in direct proportion to crime.

There are a number of reasons to think that the probability of a criminal being placed in prison should not be independent of the number of crimes, however. Moreover, even if the mechanical theory is generally true, the dynamic relationship between imprisonment and crime makes obtaining evidence regarding the mechanical theory exceedingly difficult. This section discusses these issues in turn.

Several factors might lead to a less than directly proportional relationship between imprisonment and crime. If the number of prison cells is always

¹⁴ See Federal Bureau of Investigation (FBI), Uniform Crime Reports for the United States (1973–98).

filled to capacity, for example, then the number of prisoners (in the short term) should be independent of the amount of crime. Similarly, if law enforcement operates more efficiently during periods of low crime rates than during crime waves, one might expect a less than directly proportional relationship between crime and incarceration. Finally, if police or prosecutors seek to fill a quota for the number of criminals caught or punished, then the number of criminals sent to prison would be independent of the amount of crime (an elasticity of zero).

Other factors may also lead to a partially nonmechanical relationship between crime levels and imprisonment. (Throughout the text, partially mechanical relationships between prisons and crime will refer to positive relationships different from direct proportionality, while a strictly mechanical relationship implies that the prison population responds in direct proportion to crime.) For example, if the marginal crime is less severe (and thus less likely to result in incarceration) than the average crime, then the elasticity of imprisonment with respect to crime should be less than 1 (and more than 1 if the marginal crime is more severe than the average crime).¹⁵ If law enforcement responds to period of high crime rates by "cracking down" on crime (and sending a higher proportion of offenders to prison), however, then the elasticity of imprisonment with respect to crime may be greater than 1.

Even if none of the aforementioned factors are significant, the dynamic nature of the relationship between crime and incarceration rates makes estimating the elasticity of crime with respect to incarceration quite difficult. Since convicted criminals often spend more than 1 year in prison, the number of individuals in prison in any given year depends not only on the crime rate for that year but also on many lags of the crime rate. Moreover, the relationship between lagged crime rates and current incarceration rates is ambiguous. High crime rates 5 years ago, for example, might lead to high incarceration rates today if most of the prisoners convicted 5 years ago remain in prison in the present. If many criminals serve only 5-year sentences, by contrast, then the incarceration rate may drop in the present even if the contemporaneous crime rate changes little from the previous year. Finally, crime rates in a given locale are highly autocorrelated, which makes estimation of coefficients on various lags of crime subject to increased standard errors. Thus, disentangling the effects of various lags of the crime rate on the incarceration rate may be nearly impossible.

In total, these factors (and many others) may cause theoretical and empirical deviations from a strictly mechanical (one-to-one) relationship between imprisonment and crime. Moreover, the net impact of these factors will be ambiguous. As a result, the mechanical theory should be viewed

¹⁵ Note that in the empirical specifications below, this possibility is partially controlled for through the separation of crimes into property and violent crimes, which have different propensities to lead to incarceration.

TABLE 1
SUMMARY STATISTICS

Variable	Mean	Standard Deviation	Within-State Standard Deviation
Property crime per 1,000 residents	44.4	11.0	3.7
Violent crime per 1,000 residents	5.0	2.6	.80
Weighted total crime per 1,000 residents	18.8	5.1	1.6
Prison admissions per 1,000 residents	1.41	.69	.42
Prison releases per 1,000 population	1.23	.64	.39
Prisoners per 1,000 residents	2.5	1.2	.72
Police per 1,000 residents	2.66	.54	.24
Income per capita (\$1997)	21,877	3,653	1,493
Unemployment	.059	.017	.012
Poverty rate	13.4	4.03	1.8
Effective abortion rate per 1,000 live births, violent crime	53	63	50
Effective abortion rate per 1,000 live births, property crime	93	92	73

NOTE.—All values are unweighted averages of state-level data for the 48 continental U.S. states for the period 1985–97 (624 observations). The data sources are described in the text. Effective abortion rates are defined in the text (equation (2)).

more as a benchmark for analysis than as a definitive empirical prediction. Nevertheless, the overwhelming lack of support for the mechanical theory in the literature is striking, even after consideration of these mitigating factors. In the following sections, the effectiveness of the mechanical theory as a benchmark is tested by using a number of techniques that hopefully mitigate the empirical complications that plague other studies.

III. DATA AND DESCRIPTIVE STATISTICS

For this study, I will use panel data on the 48 continental U.S. states from the years 1985–97.¹⁶ These years are chosen because they span the period during which abortion begins to have an impact on crime rates.¹⁷ Table 1 presents unweighted state averages of many of the variables used in this paper.

Data on crime are taken from the *Uniform Crime Reports* published by the Federal Bureau of Investigation (FBI).¹⁸ The FBI collects data on several types of “index crimes” that are known to police in a given state in a given year. Violent index crimes include murder and nonnegligent manslaughter, forcible rape, assault, and robbery.¹⁹ The crimes that comprise the property

¹⁶ I thank John Donohue and Steven Levitt for graciously sharing much of the data used in this paper.

¹⁷ See Donohue & Levitt, *supra* note 9.

¹⁸ See FBI, *supra* note 14.

¹⁹ Definitions for violent index crimes are available at <http://www.ci.chi.il.us/CommunityPolicing/Statistics/IndexCrimeDefs.html>.

index are burglary, larceny, and motor vehicle theft.²⁰ Table 1 also presents figures for "weighted total crime." This figure is an average of property crime and total crime and is weighted to reflect the fact that the two types of crimes make up differing proportions of prison admissions.²¹ Since violent crimes have a greater chance of being punished with imprisonment than do property crimes, violent index crimes receive a higher weighting on the average (violent crimes have a weight of approximately .65, while property crimes get a weighting of .35).²²

Table 1 also presents data on several statistics concerning the prison population of the United States.²³ The data on prisoners come from the *Correctional Populations in the United States* survey published by the Bureau of Justice Statistics.²⁴ Over the period in question, the average state incarcerated approximately 2.5 prisoners per 1,000 residents. This incarceration rate in the United States is considerably higher than that of any other Western nation.²⁵ The source for data on the number of admissions to and releases from prison (by state and year) is the National Prisoner Statistics data series,

²⁰ It should be noted that the *Uniform Crime Reports* includes only crimes that are known to the police. Crimes that are not reported are not included in the data. Victimization surveys tend to reveal that there is a considerable amount of crime that goes unreported to police. See John DiIulio, Jr., Help Wanted: Economists, Crime and Public Policy, 10 J. Econ. Persp. 3, 6–12 (1996). This raises some questions concerning measurement error, although there is some reason to believe that the error will be constant. See Steven D. Levitt, Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error? 36 Econ. Inquiry 353 (1998), for a full discussion.

²¹ Note that the proportion of crimes is taken from the year 1985 to minimize endogeneity issues. In addition, note that simply weighting by the proportion of property and violent criminals in prison is inaccurate, since the violent criminals spend longer periods in jail per crime. Thus, property criminals tend to make up a higher proportion of admitted criminals then would be assumed from their proportion of the prison population. Property criminals make up about 30 percent of prison admissions, while violent criminals make up about 45 percent. Other crimes, such as drug-related crimes, make up the remainder. Bureau of Justice Statistics, *Correctional Populations in the United States (1980–98)* (<http://ojp.usdoj.gov/bjs/abstract/cpusst.htm>; checked in 2001).

²² Note that changes in the weights make little difference to the regression results below.

²³ Note that this paper studies only prison populations, which do not include jail inmates. (Jail inmates inhabit county facilities and include individuals held in pretrial detention as well as those serving very short sentences.) Including jail inmates would complicate the empirical work to a great degree because jail inmates often serve less than 1 year in prison, making the problem of lags between crime and incarceration even more intractable. Moreover, the number of jail inmates in the United States increased at almost the exact same rate as the number of prisoners during the period 1985–97 (an approximately 120 percent increase for jail inmates during this period and a 130 percent increase for prisoners; see Bureau of Justice Statistics, *supra* note 21). This trend, along with the fact that the prison population is more than twice as large as the jail population, suggests (but does not prove) that the results presented below apply to general U.S. incarceration policies.

²⁴ Bureau of Justice Statistics, *supra* note 21.

²⁵ Tonry, *supra* note 2, at 419.

collected annually by the Bureau of Justice Statistics.²⁶ The mean admission rate is higher than the mean release rate, which reflects the secular rise in total imprisonment that occurred from 1985 to 1997. Moreover, it is important to note that the increase in imprisonment rates has not simply been the result of a policy of imprisoning drug offenders. Indeed, the proportion of inmates imprisoned for drug offenses in state prisons has declined over the past decade.²⁷

Figure 1 graphs prison admissions, total prison population, and violent and property index crime rates by year. Note the distinct patterns in the prison data and the crime data. Both prison admissions and total prison population climb continuously from 1985 to 1997. Indeed, prison admissions and populations more than doubled over this 12-year period. Crime rates, by contrast, rise until approximately 1992 and then begin to decline. These conflicting patterns highlight the argument that incarceration rates are independent of crime rates; the incarceration figures rise at almost the same rate when crime rates rise as when they fall.

Data on the number of police in each state, once-lagged to minimize endogeneity, are also obtained from the *Uniform Crime Reports*. Data for other controls, such as state per capita income, state unemployment rates, and state poverty rates, are obtained from the *Statistical Abstract of the United States*.²⁸ A discussion of the data on abortion can be found below.

IV. ORDINARY LEAST SQUARES ESTIMATES OF THE ELASTICITY OF PRISON ADMISSIONS WITH RESPECT TO CRIME

This section tests the mechanical theory of prison admissions using OLS regressions on the data just described. According to this theory, the number of admissions should change in proportion to a change in crime rates.

To test this theory, I estimate the following equation:

$$p_{st} = \beta c_{st} + \theta_s + \lambda_t + X'\delta + \varepsilon_{st}, \quad (1)$$

where s denotes states and t corresponds to years. The variable p_{st} is the log of the per capita prison admissions rate, c_{st} is the log of the per capita crime rate, θ_s and λ_t are state and year fixed effects, respectively, X is a vector of control variables, and ε_{st} is an error term. According to the strict mechanical theory of incarceration rates, $\beta = 1$; if crime goes up by 1 percent, the number of admissions to prison should also rise by the same percentage.

There are several important specification issues worth noting. The coef-

²⁶ Note that the National Prisoner Statistics data set records admissions to prison. Admissions to prison are admissions of convicted and sentenced criminals and do not include individuals being held for other reasons, such as indicted criminals awaiting trial.

²⁷ See Bureau of Justice Statistics, *supra* note 21, at 50 (data from 1997 edition).

²⁸ FBI, *supra* note 14; Bureau of the Census, *Statistical Abstract of the United States* (various years).

ficients are estimated using unweighted regressions. Since the policy response to crime is the variable of interest, there is no reason to lend more weight to larger states.²⁹ In addition, equation (1) includes controls for year effects. The rising trend in prison population will be reflected in the year effects (the λ_t) and thus will not impact the coefficients of primary interest. Prison populations have been rising for many reasons.³⁰ To isolate the impacts of changes in crime on the prison population, it is necessary to control for these other factors, even if this entails forgoing direct explanation of the rise in imprisonment.³¹

Another concern with the specification is the fact that criminals who commit crimes that occur in year t will often not be admitted to prison until year $t + 1$. Indeed, according to a survey of jurisdictions by the National Center for State Courts,³² the median case-processing time from arrest to disposition was 125 days. This suggests that lags of almost half-a-year (or sometimes even longer) between crime and admission to prison may be quite common. As a result, almost half of the prisoners admitted to prison in 1 year (p_{st}) will have committed the crime for which they are sentenced in the previous year ($c_{s,t-1}$).

To address this issue, this paper will employ a moving average approach. Since the relevant crime rate that influences the number of prisoners admitted to prison in year t is not c_{st} but rather a combination of c_{st} and $c_{s,t-1}$, the estimations will use a weighted average (with weights of approximately .6 and .4, respectively) of these two crime figures in place of the c_{st} term that appears in equation (1). (Using alternative weighting schemes does not change the results of the study appreciably.)

Finally, an important recent paper by Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan suggests that serial correlation in error terms may bias estimates of standard errors in studies employing differences-in-differences methodologies over a panel of states, even after controlling for state fixed effects.³³ Since the research design described here shares some similarities with the differences-in-differences methodology, the tables below will

²⁹ Weighting the results by state population reduces the OLS estimates by approximately 15 percent. Note also that the data for a larger state are not estimated any more accurately than the observations for smaller states. For each state, there is only one observation per variable per year.

³⁰ See Section VIB for discussion.

³¹ It will be possible to use the results described here to obtain an indirect estimate of the impact of rising crime rates (from 1973 to 1997) on the prison population. See Section VI *infra*.

³² Brian J. Ostrom & Neil B. Kauder, *Examining the Work of State Courts, 1998: A National Perspective from the Court Statistics Project* (1999).

³³ See Marianne Bertrand, Esther Duflo, & Sendhil Mullainathan, *How Much Should We Trust Differences-in-Differences Estimates?* (Working Paper No. w8841, Nat'l Bur. Econ. Res., March 2002). The authors show how autocorrelation in both error terms and in a treatment variable can bias downward standard error estimates in differences-in-differences studies.

TABLE 2
ORDINARY LEAST SQUARES (OLS) AND INSTRUMENTAL
VARIABLES (IV) REGRESSION RESULTS

VARIABLE	ln(Prison Admissions per Capita)			
	OLS		IV	
	(1)	(2)	(3)	(4)
ln(total crime per capita) ^a	.59 (.13)** [.26]*	.59 (.13)** [.23]*	1.16 (.28)** [.68]	1.07 (.30)** [.70]
Unemployment rate		-.0052 (.010) [.013]		-.0053 (.0010) [.014]
ln(income per capita)		-1.060 (.48)* [.69]		-1.32 (.51)* [.81]
Poverty rate		-.013 (.006)* [.012]		-.010 (.006) [.012]
ln(police per capita)		-.039 (.11) [.20]		-.138 (.12) [.25]

NOTE.—Standard errors are in parentheses. Serial-correlation corrected standard errors are in square brackets. The dependent variable is the log of the number of prison admissions per capita. All regressions include state and year fixed effects. The data set includes annual state-level observations from the 48 continental U.S. states from 1985 to 1997. Estimation is robust to heteroskedasticity. There are 624 observations in all equations. The police variables are once-lagged to minimize endogeneity bias. Data sources are described in the text.

^a Two-year moving average.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

report two estimates of standard errors for the parameter estimates of the impact of crime on imprisonment. Below each parameter estimate, the number appearing in parentheses is the traditional fixed-effects estimate of the standard error, which is directly comparable to the estimates of previous studies. Below this standard error there appears another standard error estimate in brackets. This estimate attempts to address the serial-correlation concern by allowing for within-state serial correlation of the error terms, even after controlling for state fixed effects.³⁴

The results of the fixed-effects, panel data regressions (equation (1)) are presented in columns 1 and 2 of Table 2. Column 1 presents results without controls aside from year and state fixed effects, while column 2 includes control variables. The coefficients for the control variables estimated in column 2 are generally consistent with the estimates presented in previous

³⁴ Standard errors are adjusted using the robust cluster command in Stata. Note that while Bertrand, Duflo, and Mullainathan advocate this option for differences-in-differences studies, its effectiveness in IV studies such as this one is unknown (see below).

estimates of similar specifications.³⁵ Note that higher per capita income and higher poverty rates are negatively associated with incarceration.

In the regressions presented here, the elasticity of prison admissions with respect to crime (β) is approximately .59. A 1 percent increase in crime is associated with a .59 percent increase in the number criminals admitted to prison (column 2). This result is robust to several different specifications, including changes in the weighting of property and violent crimes as well as changes in the weighting of present-year versus previous-year crime rates.

The elasticities of prison admissions with respect to crime that are presented here are considerably higher than the estimates suggested by the previous studies, which often find little or no relationship between changes in crime rates and changes in incarceration rates.³⁶

There are two primary reasons for this distinction. First, the choice of prison admissions (as opposed to overall prison population) as the dependent variable allows this specification to hone in on the contemporaneous relationship between incarceration and crime. Other studies are confounded by the dynamic relationship between overall incarceration rates and crime.

The specification presented here also includes controls for year effects. This helps disentangle the impacts of changes in crime on incarceration from the other trends that affect imprisonment rates during the period in question.³⁷ Indeed, the year effects are all positive (the excluded year is 1985), which reveals a secular increase in prison admissions rates that is (at least partially) independent of changes in crime (see Table 3, columns 1 and 2). Some of the explanations that have been offered (among many others) to explain this trend in imprisonment include the rising rate of drug-related arrests, decreasing prison costs, an increasing tendency toward incarceration ("getting tough on crime"), and the interaction with U.S. political trends. Many scholars, however, reject these explanations (with the possible exception of the latter) as inadequate³⁸ and view the startling rise of incarceration in the United States as one of the greatest mysteries of U.S. public policy over the last 2 decades.³⁹

While the estimates of the elasticity of prison admissions with respect to crime presented here are higher than existing estimates, they are considerably

³⁵ See Levitt, *supra* note 5, table 5.

³⁶ See note 2 *supra* for a list of citations.

³⁷ Including year effects, however, precludes direct explanations of this trend. In Section VI, however, I will attempt to use the indirect evidence obtained from this paper to study how changes in crime rates might have affected incarceration rates.

³⁸ For example, the real per-inmate prison cost increased by approximately 20 percent from 1986 to 1996. See Bureau of Justice Statistics, *State Prison Expenditures, 1996* (1999). Moreover, it is important to note that the increase in imprisonment rates has not simply been the result of a policy of imprisoning drug offenders. Indeed, the proportion of inmates imprisoned for drug offenses in state prisons has declined over the past decade. See Bureau of Justice Statistics, *supra* note 21.

³⁹ See Zimring & Hawkins, *supra* note 1; and Tonry, *supra* note 2.

TABLE 3
YEAR EFFECTS FROM ORDINARY LEAST SQUARES (OLS) AND
INSTRUMENTAL VARIABLES (IV) REGRESSIONS

YEAR	ln(Prison Admissions per Capita)			
	OLS		IV	
	(1)	(2)	(3)	(4)
1986	.033 (.04)	.075 (.041)	.011 (.042)	.058 (.043)
1987	.084 (.041)*	.132 (.043)**	.048 (.044)	.109 (.045)*
1988	.156 (.041)**	.219 (.046)**	.115 (.045)**	.196 (.049)**
1989	.335 (.041)**	.421 (.050)**	.288 (.046)**	.394 (.053)**
1990	.372 (.042)**	.471 (.051)**	.315 (.048)**	.435 (.056)**
1991	.381 (.042)**	.481 (.049)**	.314 (.051)**	.435 (.057)**
1992	.425 (.042)**	.556 (.053)**	.368 (.048)**	.517 (.059)**
1993	.445 (.041)**	.583 (.054)**	.404 (.045)**	.555 (.057)**
1994	.46 (.041)**	.62 (.061)**	.425 (.044)**	.6 (.062)**
1995	.492 (.041)**	.706 (.073)**	.457 (.044)**	.685 (.074)**
1996	.522 (.040)**	.767 (.080)**	.5 (.042)**	.756 (.081)**
1997	.579 (.040)**	.815 (.080)**	.572 (.041)**	.818 (.081)**

NOTE.—This table presents the value of the year-effect controls included in the regressions presented in Table 2. The omitted year is 1985. Standard errors are in parentheses. The dependent variable is the log of the number of prison admissions per capita. All regressions include state and year fixed effects. The data set includes annual state-level observations from the 48 continental U.S. states from 1985 to 1997. There are 624 observations in all equations. Estimation is robust to heteroskedasticity across states. The police variables are once-lagged to minimize endogeneity bias. Data sources are described in the text.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

lower than the prediction made by the strict mechanical theory of incarceration, which would predict an elasticity (β) of 1. Indeed, a t -test of the elasticity coefficients being equal to 1 is rejected at the 99 percent confidence level. As briefly discussed in Section I, however, there is good reason to suspect that endogeneity biases the estimate of β downward. The next section will attempt to control for this endogeneity using an instrumental variables (IV) approach.

V. INSTRUMENTAL VARIABLES ESTIMATION

It is easy to imagine an endogenous relationship between crime and incarceration rates. While crime rates should help determine incarceration rates through the mechanical relationship described above, crime rates are themselves partially determined by incarceration rates. High incarceration rates may deter crime—if criminals perceive that there is a higher probability of going to prison, they will be less likely to commit crimes. In addition, high incarceration rates can lower crime through an incapacitation effect. If po-

tential criminals are in prison rather than at large, then the population of criminals goes down and there may be fewer crimes.⁴⁰

Several empirical studies, including those of Thomas Marvell and Carlisle Moody and particularly of Levitt,⁴¹ obtain results suggesting that higher incarceration rates lower crime. Levitt's paper finds an estimated elasticity of crime with respect to prisoners of around $-.4$. The empirical evidence clearly shows that incarceration rates help determine crime rates, which suggests that studies of the impact of crime rates on incarceration must control for endogeneity.⁴²

A. *Abortion and Crime Rates*

Controlling for this endogeneity requires a source of exogenous variation in crime rates. For this study, I will use abortion rates during the 1970s as the source of exogenous variation.⁴³ As Donohue and Levitt show in their seminal paper,⁴⁴ abortion rates in the 1970s (which varied widely) are important determinants of changes in crime rates in the 1990s.

Donohue and Levitt employ an intuitive approach to find a connection between abortion and crime. They note that abortion rates in the 1970s varied greatly among states. Five states (Alaska, California, Hawaii, New York, and Washington) were early legalizers (from 1967 to 1970) rather than legalizers after the Supreme Court's *Roe v. Wade* decision in early 1973. These states obviously had higher legal abortion rates than other states before 1973. Moreover, since abortion infrastructures do not become reality overnight, it took all states some time to reach steady state abortion rates. Abortion rates in all states were higher in 1979, for example, than in 1973.⁴⁵ In addition, different states may have very different steady-state levels of abortion. These differences stem from a number of factors, including demographic differences among state populations, attitudes of the public toward abortion, distances between abortion centers, and so on.⁴⁶

⁴⁰ See Robert D. Cooter & Thomas Ulen, *Law and Economics*, chaps. 11 & 12 (2000), for discussions of deterrence and incapacitation.

⁴¹ Marvell & Moody, *supra* note 2; Levitt, *supra* note 5.

⁴² In statistical terms, the discussion presented here implies that the error term (ϵ_{it}) from equation (1) negatively affects crime, so $\text{cov}(\epsilon_{it}, c_{it}) < 0$. Since with endogeneity bias

$$\hat{\beta}_{\text{ols}} - \beta = \text{cov}(\epsilon_{it}, c_{it}) / \text{var}(c_{it}),$$

the OLS coefficient for β estimated in the previous section will be biased downward. See Fumio Hayashi, *Econometrics* 188 (2000).

⁴³ Please note that the use of abortion rates in this paper is strictly for scientific purposes and does not imply any position on the contentious issue of the legality and availability of abortion.

⁴⁴ See Donohue & Levitt, *supra* note 9.

⁴⁵ *Id.*, figure 1.

⁴⁶ See *Abortion Factbook: Readings, Trends, and State and Local Data to 1988* (Stanley K. Henshaw & Jennifer Van Vort eds. 1992).

Since criminals are disproportionately young,⁴⁷ abortion in the 1970s should reduce the size of the criminal population in the late 1980s and 1990s by reducing the size of the cohort that commits the majority of crime. Furthermore, Donohue and Levitt claim (and bring supporting evidence) that the decrease in the size of the criminal population is even greater than the decrease in the size of the cohort since abortion rates tend to be high in high-crime-rate demographic categories (such as children of teenage mothers). Since abortion rates in the 1970s varied widely among states, Donohue and Levitt utilize cross-state variation in abortion rates and crime to identify the impact of abortion on crime. They find that "crime was almost 15–25 percent lower in 1997 than it would have been absent legalized abortion."⁴⁸

Donohue and Levitt's findings have been incisively critiqued in some new research.⁴⁹ Using more "cohort-based" analytical techniques, these researchers find a greatly reduced (or even zero) impact of abortion on crime. A number of the specifications in these papers, however, do find some (reduced) impact of abortion on crime, although the primary impact of abortion would appear to be through its cohort-size-reducing effects rather than through selectively greater impacts on high-crime-rate populations.⁵⁰ As one survey of the controversy over Donohue and Levitt's article noted, "The same reviewers who believe the study has probably overstated the effect of abortion liberalization agree that the authors most likely have uncovered an important mechanism contributing to the lower crime rates."⁵¹ For the purposes of this study, even a reduced impact of abortion on crime is sufficient for the specification presented below.

After controlling for state fixed effects, it is difficult to envision how abortion rates in the 1970s could be directly determining imprisonment rates in the 1990s (other than through crime). There is no doubt that the abortion rate of a state is not random. Large, urban states such as California and New York tend to have higher rates of abortion than smaller states. After controlling for state fixed effects, however, the nonrandomness of abortion within a state should be of much less concern.

One might also be concerned that different steady-state levels of abortion in different states reflect underlying sentiments that also were manifested in

⁴⁷ According to Cooter & Ulen, *supra* note 40, chap. 12, about two-thirds of all street crime in the United States is committed by persons aged 15–24.

⁴⁸ Donohue & Levitt, *supra* note 9, at 418.

⁴⁹ See Ted Joyce, Did Legalized Abortion Lower Crime? (Working Paper No. 8319, Nat'l Bur. Econ. Res., June 2001); and John R. Lott, Jr., & John E. Whitley, Abortion and Crime: Unwanted Children and Out-of-Wedlock Births (Working Paper No. 254, Yale L. Sch. Program Stud. L., Econ., & Pub. Pol'y 2001).

⁵⁰ See, for example, table 2 in Joyce, *supra* note 49; and Phillip B. Levine *et al.*, *Roe v. Wade* and American Fertility, 89 Am. J. Pub. Health 199 (1999).

⁵¹ Sasha Abramsky, Did *Roe v. Wade* Abort Crime? Am. Prospect, January 1, 2001, at 26. Note that including region-year interaction terms in my regressions, which partially addresses some of the critiques, does not materially address the results presented here.

social programs in the 1970s that might have affected at-risk children. For example, if high abortion rates are correlated with high welfare payments, then it is possible that it is the welfare payments that are causing the reduction in crime rather than abortion. For the purposes of this study, however, this distinction is not of critical importance. Whether it was abortion laws themselves or the programs with which these laws are correlated that are responsible for the decrease in crime in the 1990s does not change the validity of the IV specification. So long as all these programs are not correlated with the unobservable determinants of prison admissions in the 1990s, the specification is valid.⁵²

Abortion's impact on crime will be felt only gradually. As each year passes, the fraction of the population that is affected by abortion increases. In 1990, for example, only cohorts aged 17 and younger were affected by *Roe v. Wade*. By 1997, cohorts aged 24 and younger were impacted by *Roe v. Wade* (which was decided in early 1973). Since the percentage of criminals aged 24 and younger is much higher than the percentage aged 17 and younger, abortion rates in the 1970s should have a larger impact on crime in 1997 than in 1990.⁵³

To apply this idea empirically, this paper employs the "effective abortion rate" (a_{st}) as defined by Donohue and Levitt:

$$a_{st} = \sum_{\text{all ages}} \text{abortion}_{t-\text{age}} (\text{arrests}_{\text{age}} / \text{arrests}_{\text{total}}). \quad (2)$$

The "importance" of a lagged abortion rate (in year $t - \text{age}$) for determining the crime rate in year t depends on the proportion of the total criminal population (for a particular crime) that is of that age.⁵⁴ The effective abortion rate aggregates these lagged abortion rates into one figure.

The bottom of Table 1 presents descriptive statistics for the effective abortion rates for violent and property crimes. The data for abortion are taken from the *Statistical Abstract of the United States*.⁵⁵ Note the large standard deviations for these figures, both overall and even within states. This reflects the large differences in abortion rates across states as well as the lag between the legalization of abortion and the time at which the steady-state abortion rate for a particular state is reached. In addition, the effective abortion rates

⁵² This argument can also be applied to some of the critiques of the Donohue-Levitt analysis described above. These critiques suggest that the supposed correlation between abortion and crime is the result of state-specific period effects that moved across the nation at different times. If these effects are uncorrelated with the unobservable determinants of prison admissions (and given the idiosyncratic nature of these state-specific period effects, this does not seem to be an unreasonable suspicion), then abortion would still serve as a valid instrument for crime, even if the critiques of the Donohue-Levitt paper are entirely valid.

⁵³ See Donohue & Levitt, *supra* note 9, at 394.

⁵⁴ The arrest proportions were determined using 1985 data to minimize potential endogeneity bias.

⁵⁵ Bureau of the Census, *supra* note 28.

for violent crimes are lower than those for property crimes (53 versus 93 per 1,000 live births). This reflects the fact that violent criminals tend to be older than property criminals. As a result, it will take longer for abortion rates to have an impact on violent crimes.⁵⁶

This paper now attempts to estimate the response of prison admissions to crime through an IV approach by using the effective abortion rate as an instrument. The specification is

$$p_{st} = \beta \hat{c}_{st} + \theta_s + \lambda_t + X'_{st} \delta + \varepsilon_{st}, \quad (3)$$

where θ_s and λ_t are state and year fixed effects, X_{st} is a vector of control variables, ε_{st} is an error term, and $\hat{c}_{st}$ (predicted crime) is estimated using

$$c_{st} = \rho a_{st} + \mu_s + \tau_t + X'_{st} \xi + \eta_{st}, \quad (4)$$

where a_{st} is the effective abortion rate in state s at time t , μ_s and τ_t are state and year fixed effects, X_{st} is a vector of control variables, and η_{st} is an error term for the first stage. Note that c_{st} and a_{st} are both 2-year moving averages rather than 1-year totals to take account of the fact that many criminals are sent to jail in the year after they commit a crime (as discussed above).

B. Results of Instrumental Variables Estimation

Table 4 presents results from the first-stage regression (equation (4)) without and with control variables (columns 1 and 2). As shown by the results in Donohue and Levitt's paper, abortion is an important determinant of crime. In the specifications with and without controls, the effective abortion rate is significant at the 99 percent confidence level, with a t -statistic greater than 11. The magnitude of the coefficient is also substantial. A 1-standard-deviation difference in the effective abortion rate in 1997 was associated with an approximately 7 percent change in crime—a state with an effective abortion rate in 1997 that was slightly less than 1 standard deviation above the mean (100 effective abortions per 1,000 live births above the mean) would have a 7 percent lower crime rate, *ceteris paribus*.⁵⁷

The parameter estimates of the control variables (presented in column 2 of Table 4) included in this regression are measured imprecisely and generally have insignificant impacts on crime. These results are consistent with many

⁵⁶ Donohue & Levitt, *supra* note 9, at 394.

⁵⁷ Note that although this regression is similar in spirit to those presented in Donohue & Levitt, *supra* note 9, there are two salient differences. In addition to the use of 2-year moving averages rather than 1-year figures for crime and abortion, these regressions are unweighted. While Donohue and Levitt are primarily interested in estimating the impact of abortion on overall crime levels in the United States (and therefore use weighted regression), this study focuses on policy choices by states, and therefore the regressions are unweighted. Because of these differences, the impacts of abortion found here are lower than the more direct estimates presented in Donohue & Levitt (*id.*). The standard errors of the coefficients, however, are considerably smaller.

TABLE 4
ESTIMATES OF THE RELATIONSHIP BETWEEN
ABORTION RATES AND CRIME

VARIABLE	ln(Weighted Average of Crime Rates)	
	(1)	(2)
"Effective" abortion rate $\times$ 100	-.068 (.009)** [.006]**	-.079 (.009)** [.006]**
State unemployment rate		.01 (.003)** [.003]**
Poverty rate		-.0020 (.002) [.0016]
ln(police per capita) once lagged		.16 (.36) [.36]
ln(income per capita)		.14 (.20) [.14]

NOTE.—Standard errors are in parentheses. The dependent variable is the log in the weighted per capita crime rate (an average of property and violent crime rates in which violent crime rates are given extra weight since they are more frequently punished by imprisonment). All regressions include state and year fixed effects. Estimation is robust to heteroskedasticity across states and years. All variables are 2-year averages. The data set includes annual state-level observations from the 48 continental U.S. states from 1985 to 1997. There are 624 observations in both equations. The police variables are once-lagged to minimize endogeneity bias. Data sources are described in the text.

** Significant at the 1 percent level.

studies of crime, which often find that crime has few significant determinants.⁵⁸ The exception is the unemployment rate, which has a significantly positive impact on crime.⁵⁹

A glance at Table 4 reveals that abortion rates in the 1970s are strongly correlated with crime. As argued above, it is also plausible that abortion rates should be uncorrelated with the error term (ϵ_{st}) from equation (3). Consequently, I now utilize abortion rates in the 1970s as an instrument for crime to obtain unbiased estimates of the β coefficient from equation (3).

Columns 3 and 4 in Table 2 present estimates of equation (3) without and with control variables, respectively. The results are striking. The instrumental

⁵⁸ For examples of studies that find almost no significant determinants of crime in a first-differences model, see Kristin F. Butcher & Anne M. Piehl, *Cross-City Evidence on the Relationship between Immigration and Crime*, 17 J. Pol'y Analysis & Mgmt. 457 (1998); and Steven D. Levitt, *Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime*, 87 Am. Econ. Rev. 270 (1997).

⁵⁹ See Cooter & Ulen, *supra* note 40, at 415–17, for a thorough discussion of the connection between unemployment and crime.

variable estimates of the elasticity of prison admissions with respect to crime rates (β) are nearly double the OLS estimates (although the standard error also increases significantly). Indeed, the IV estimates of the elasticity are quite near (just slightly above) an elasticity of 1; a 1 percent change in crime is expected to lead to a 1 percent change in the number of prisoners admitted to prison (that is, $\beta = 1$). This is exactly what the strict mechanical theory of prison admissions predicts; *ceteris paribus*, the number of individuals sent to prison fluctuates in direct proportion to the amount of crime.

The serial-correlation-corrected standard errors (in brackets) for these estimations are extremely high (more than double the traditionally estimated standard errors), which suggests that these results must be treated with more than the usual "grain of salt." Because incarceration figures are highly correlated within state, it may be difficult to obtain precise (unbiased) estimates of the coefficients of interest.

When the regression data are weighted by state population, however, the IV estimate of the elasticity is reduced to approximately .3 (with standard errors approximately 40 percent larger than the unweighted estimates). This implies that small states have considerably more flexible policy responses to changes in crime rates than do larger states.⁶⁰ Note, however, that the weigh-

ted regression results are dominated by the results of California. When California is omitted from the weighted regression, the estimate moves to approximately .7.

The divergence of results of the IV estimation from the OLS estimation suggests that endogeneity bias is an important factor that obfuscates previous studies of the relationship between imprisonment and crime. When the impact of imprisonment on crime is "controlled" by using an instrument, the estimate of the elasticity of prison admissions with respect to crime doubles, implying that there is a large bias in OLS estimates of the impact of crime on imprisonment.

The model's other parameters are generally imprecisely estimated. After controlling for crime rates, I find that unemployment, poverty rates, and the number of police have insignificant impacts on imprisonment. As in the OLS specification, per capita income has a significantly negative impact on imprisonment. Note again that the year effects (displayed in Table 3) for these regressions (the excluded year is the first year, 1985) are invariably positive, which reflects the upward trend in prison admissions. As mentioned above, the estimate of the elasticity of imprisonment with respect to crime that is presented here does not undermine the proposed importance of many other factors that drive the secular increase in prison admissions during this period.

⁶⁰ Thus the average "marginal" criminal in the United States is not as likely to go to prison as the average criminal.

VI. IMPRISONMENT AND CRIME

The previous section documented that the mechanical theory of imprisonment and crime performed well with respect to admissions. After controlling for other trends, prison admissions fluctuated in direct proportion to crime rates. This section attempts to interpret how this finding relates to overall imprisonment trends and their relationship to crime rates.⁶¹ Can the relationship uncovered between prison admissions and crime be applied more generally to the response of imprisonment to crime, or is there some countervailing factor (such as release rates) that prevents the mechanical relationship between prison admissions and crime from translating into an equivalent relationship between overall imprisonment and crime?

A. *Releases from Prison and Crime Rates*

Perhaps the most obvious countervailing factor negating the impact of an increase in prison admissions on the prison population would be prison releases. Even if prison admissions do change in direct proportion to crime, if prison release rates are adjusted to take into account the number of admissions, then the number of prisoners would not change overall. According to the mechanical theory, by contrast, releases from prison should be independent of contemporaneous crime rates, since the prisoners released from prison in the present period would have committed their crimes in a previous period.

Table 5 presents results from the following regression:

$$r_{st} = \tau c_{st} + \theta_s + \lambda_t + X'\delta + \varepsilon_{st}, \quad (5)$$

where r_{st} is the number of prison releases in state s at time t , θ_s and λ_t are state and year effects, respectively, and X is a vector of control variables. According to the mechanical theory, τ should be near 0; crime in the present period should have only a slight impact on the number of releases. If all criminals spend more than 1 year in prison, then releases should be independent of the contemporaneous crime rate. Since some criminals spend less than 1 year in prison, however, releases will be slightly dependent on contemporaneous crime rates.

Columns 1 and 2 of Table 5 present OLS estimates of equation (5) that reveal that this prediction is not supported by OLS estimates. The parameter estimates in these columns suggest that a 1 percent change in crime will be accompanied by a corresponding .35 percent change in the number of releases

⁶¹ As described above, the number and change in the number of prisoners depend on past crime rates as well as on crime in the present period. Since criminals often serve long prison terms, disentangling the impact of various lags in the crime rate on overall imprisonment becomes exceedingly difficult. As a result, the results presented in this section must inevitably be more speculative than the results presented earlier.

TABLE 5
ORDINARY LEAST SQUARES (OLS) AND INSTRUMENTAL VARIABLES (IV)
REGRESSIONS OF RELEASES FROM PRISON ON THE AMOUNT OF CRIME

VARIABLE	ANNUAL PRISON RELEASES PER CAPITA			
	OLS		IV	
	(1)	(2)	(3)	(4)
ln(total crime per capita)	.331 (.12)** [.26]	.39 (.12)** [.23]	.109 (.27) [.68]	.251 (.29) [.64]
Unemployment rate		2.304 (.974)* [1.84]		2.294 (.977)* [1.86]
Poverty rate		-.015 (.006)* [.011]		-.016 (.006)** [.011]
ln(income per capita)		-.182 (.406) [.823]		-.162 (.426) [.941]
ln(police per capita)		-.256 (.131) [.276]		-.217 (.14) [.286]

NOTE.—Standard errors are in parentheses. The dependent variable is the log of the number of prison releases per capita. All regressions include state and year fixed effects. Estimation is robust to heteroskedasticity across states and years. The data set includes annual state-level observations from the 48 continental U.S. states from 1985 to 1997. There are 624 observations in all equations. The police variables are once lagged to minimize endogeneity bias. Data sources are described in the text.

* Significant at the 5 percent level.

** Significant at the 1 percent level.

from prison. These estimates are significantly different from the zero value predicted by the mechanical theory.

As with the OLS regression discussed earlier (equation (5)), however, there is reason to believe that OLS estimates of α will be biased. If states decide to “crack down” on crime, and this phenomenon is unobserved but negatively correlated with releases from prison, then the OLS estimate of α should be biased upward. Columns 3 and 4 of Table 5 show that this is the case. When crime rates are instrumented by abortion rates (using a specification analogous to equations (3) and (4)), the elasticity of prison releases with respect to crime goes down considerably and becomes insignificant at the 5 percent level (although it remains positive and the standard errors are large). This evidence suggests that, after controlling for endogeneity, releases may not respond dramatically to changes in crime rates. Even if the OLS estimates are accurate, however, it is clear that releases from prison are much less sensitive to changes in crime rates than are admissions. Consequently, a change in crime should be associated with a corresponding change in overall imprisonment, *ceteris paribus*.

B. Trends in Imprisonment and Crime

If prison admissions go up with crime and prison releases are not significantly affected by contemporaneous crime rates, then at least part (but certainly not all) of the rise in incarceration in the United States may be attributable to changes in the underlying propensity to commit crime and a mechanical response in imprisonment. The results presented here suggest that the number of prisoners should go up in a mechanical fashion with the amount of crime, other things being equal. Thus, even had there been no change in the propensity to incarcerate between 1970 and 1997, the increase in crime that occurred during those 27 years would have led to an approximately 80 percent increase⁶² in the U.S. incarceration rate. While this is a far cry from the nearly fivefold increase that actually occurred during this period, it does suggest that the mechanical theory is a valid and important part of the theory relating crime rates and incarceration rates, particularly when combined with other trends such as rising sentence lengths or other time-variant heterogeneities.⁶³

This conclusion is supported by an analysis of overall imprisonment trends in states with high versus low abortion rates in the 1990s. As Figure 1 shows, overall imprisonment has been increasing in the United States in the 1990s, in spite of the decrease in crime rates. Clearly, there has been a general push toward greater incarceration that is independent of crime rates. Nevertheless, a glance at Table 6 reveals that changes in crime rates do have an impact on the number of individuals in prison. In Table 6, the 48 continental U.S. states are divided into quarters on the basis of their effective abortion rate in 1997. For each quartile, the percentage change in the number of prisoners (as well as the percentage change in crime rates) is given from 1985 to 1991 and from 1991 to 1997. From 1985 to 1991, before abortion began to have an important impact on imprisonment rates (because the effective abortion rate was relatively low and imprisonment responds with a long lag to changes in crime), the high-abortion-rate states witnessed the highest average increase in prison population among the three groups by a large margin. In the second half of the period, by contrast (when abortion begins to have an important

⁶² This figure was calculated by comparing the violent index crime rate per capita in the 1970s with that in 1997 and applying the elasticity of 1 between imprisonment and crime that is found here. Note that if prison deters crime, then the enormous rise in incarceration should be deterring some crime, which suggests that the underlying propensity to commit crime has risen by greater than 80 percent. Thus, the estimate presented here is a lower bound. The National Crime Victimization survey conducted by the Bureau of Justice, however, does not show a rise in crime of the same magnitude during this period. See DiIulio, *supra* note 20, for a discussion.

⁶³ Note that if incarceration would have increased by 80 percent because of mechanical factors, the underlying propensity to incarcerate did not increase fivefold between 1970 and 1997, but rather by only 275 percent ($500/180 \approx 2.75$). Note that the rise in incarceration during the 1970s and 1980s is even more attributable to mechanical factors since these years witnessed an even greater increase in crime rates.

TABLE 6
CHANGES IN INCARCERATION (1985-97) AS A FUNCTION OF ABORTION RATES

ABORTION FREQUENCY	CHANGE IN INCARCERATION RATES (%)		CHANGE IN VIOLENT CRIME RATES (%)	
	1985-91	1991-97	1985-91	1991-97
Lowest (Q1)	38	38	30	0
Medium-low (Q2)	37	37	18	-8
Medium-high (Q3)	40	34	25	-14
Highest (Q4)	44	31	22	-21

NOTE.—The 48 continental U.S. states are ranked into quartiles on the basis of effective abortion rate in 1997 (see equation (1)). Each cell presents the unweighted mean change of the variable in question for the states in the given quartile during the years noted. Imprisonment rates are taken from Bureau of Justice Statistics, *Correctional Populations in the United States* (1980-98), abortion data from the Bureau of the Census, *Statistical Abstract of the United States* (various years), and crime data from the Federal Bureau of Investigation, *Uniform Crime Reports for the United States* (1973-93).

impact on crime), the situation reverses. The high-abortion-rate group experienced the largest decrease in crime among the four groups, and it also had the smallest increase in prison population after having the largest imprisonment increase in the first period. Similarly, the third quartile of effective abortion rates also demonstrates a relatively large drop in crime and small increase in the number of prisoners. These results suggest that, although there is clearly a trend toward greater imprisonment within the United States during this period, less crime does lead to fewer prisoners, all other things being equal.⁶⁴

VII. CONCLUSION

As the previous section noted, this paper has demonstrated that the mechanical theory of crime and imprisonment rates has some empirical support, all things being equal. This is not to claim that all things have been equal. Clearly, there has been a significant increase in the propensity to imprison in the United States, as the time effects in the regressions above reveal (see Table 3). Nevertheless, the claim that the mechanical theory of crime and incarceration has "virtually no validity"⁶⁵ is likely overstated. Previous papers showing no relationship between changes in crime rates and changes in incarceration failed to address the endogeneity or the dynamic relationship between crime and incarceration. Once this endogeneity is addressed by instrumenting, the mechanical theory performs quite well in predicting incarceration rates (controlling for trend), which suggests that incarceration policies are not as paradoxical as they are sometimes portrayed. As a result,

⁶⁴ Note that because of endogeneity issues and the long time lags required, an IV regression approach to estimating the impact of crime on imprisonment rates would be extremely imprecise.

⁶⁵ Tonry, *supra* note 2, at 421.

some of the critiques of the penal system in the United States⁶⁶ that rely on the failure of the mechanical theory must be viewed with greater skepticism.

There is reason to suspect that the mechanical theory of crime and incarceration may begin to play a more obvious role in influencing U.S. incarceration rates. Since there is a lag between changes in crime rates and changes in incarceration rates, the 1990s decline in crime may only now be affecting the size of the prison population through the mechanical path. Indeed, recent incarceration patterns in the United States suggest that the trend of rising incarceration rates may have crested. The year 2000 witnessed the smallest annual growth rate in prison population in 29 years.⁶⁷ Moreover, in states with high abortion rates that have witnessed large decreases in crime, such as New York and California, the prison population has even begun to decline.⁶⁸ Such developments imply that the mechanical theory may be a critical (and more obvious) determinant of imprisonment trends over the next decade.

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⁶⁶ See note 7 *supra* for examples of such studies.

⁶⁷ Alan Beck & Jennifer C. Karberg, *Prison and Jail Inmates at Midyear 2000* (2001).

⁶⁸ David Rohde, *After Long Climb, New York State's Prison Population Starts to Drop*, N.Y. Times, February 2, 2001, late edition at A1; and David Firestone, *U.S. Figures Show Prison Population Is Now Stabilizing*, N.Y. Times, June 9, 2001, at A1. Of the 12 states that experienced declines in prison population in 2000 (listed in Firestone, *supra*), 10 have effective abortion rates above the median and six have effective abortion rates in the highest quartile.

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