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How Much Crime Reduction Does the Marginal Prisoner Buy?

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Abstract

We present new evidence on the effect of aggregate changes in incarceration on changes in crime that accounts for the potential simultaneous relationship between incarceration and crime. Our principal innovation is that we develop an instrument for future changes in incarceration rates based on the theoretically predicted dynamic adjustment path of the aggregate incarceration rate in response to a shock (from whatever source) to prison entrance or exit transition probabilities. Given that incarceration rates adjust to permanent changes in behavior with a dynamic lag (given that only a fraction of offenders are apprehended in any one period), one can identify variation in incarceration that is not contaminated by contemporary changes in criminal behavior. We isolate this variation and use it to tease out the causal effect of incarceration on crime. Using state level data for the United States covering the period from 1978 to 2004, we find crime-prison elasticities that are considerably larger than those implied by OLS estimates. For the entire time period, we find average crime-prison effects with implied elasticities of between -0.06 and -0.11 for violent crime and between -0.15 and -0.21 for property crime. We also present results for two sub-periods of our panel: 1978 to 1990 and 1991 to 2004. Our IV estimates for the earlier time period suggest much larger crime-prison effects, with elasticity estimates consistent with those presented in Levitt (1996) who analyzes a similar time period yet with an entirely different identification strategy. For the latter time period, however, the effects of changes in prison on crime are much smaller. Our results indicate that recent increases in incarceration have generated much less bang-per-buck in terms of crime reduction.

1. Introduction

Between 1980 and 2008 the number of inmates in U.S. state and federal prisons increased from approximately 320,000 to over 1.6 million. This corresponds to a change in the incarceration rate from 139 to 505 prisoners per 100,000 residents. Not surprisingly, expenditures on corrections increased in tandem as states built new prisons, expanded corrections employment, and incurred the additional costs of housing and supervising greater numbers of inmates.[1]

This rapid increase in incarceration rates and corrections expenditures has led many to ask whether, on the margin, the benefits of incarceration exceed the costs (for example, see Dilulio and Piehl 1991, Donohue and Siegelman 1998, Levitt 1996, Jacobson 2005). Presumably, the chief benefit of additional incarceration is the crime avoided via the incapacitation of the criminally active as well as the crime prevented via the general deterrence of the potentially criminally active. The larger are such incapacitation and deterrence effects, the more likely that the value of incarcerating one more offender exceeds the explicit outlays as well as the more difficult to measure social costs of incarceration.

However, there is considerable disagreement over the size of such effects and, for most recent offenders, whether incapacitation and deterrence effects exist at all. Those who argue for small crime-incarceration effects note the lack of a strong correlation between aggregate crime and incarceration rates (Jacobson 2005, Western 2006) and the likelihood that the crime-reducing effects of incarceration are likely to be declining as the prison population increases. Researchers finding larger effects (Levitt 1996) emphasize a fundamental identification problem

[1] Over this time period, nominal expenditures on corrections increased from approximately nine billion in 1982 to 68 billion dollars in 2006. Adjusted for inflation, this represents a three-fold increase in corrections expenditures.

that is likely to bias estimates of prison-crime effects towards zero; namely, changes in behavior that increase criminal activity will simultaneously increase incarceration rates.

In this paper, we present new evidence on the effect of aggregate changes in incarceration on changes in crime that accounts for the potential simultaneous relationship between incarceration and crime. Our principal innovation is that we develop an instrument for future changes in incarceration rates based on the theoretically predicted dynamic adjustment path of the aggregate incarceration rate in response to a shock (from whatever source) to prison entrance or exit transition probabilities. Given that incarceration rates adjust to permanent changes in behavior with a dynamic lag (given that only a fraction of offenders are apprehended in any one period), one can identify variation in incarceration that is not contaminated by contemporary changes in criminal behavior. We isolate this variation and use it to tease out the causal effect of incarceration on crime.

We present a simple model of incarceration and crime where steady-state incarceration rates are determined by the transition probabilities between the incarcerated and non-incarcerated population. We use this model to derive a prediction regarding the lead one-period change in incarceration rates based on the current disparity between the actual incarceration rate and the steady-state incarceration rate implied by the current period transition probabilities describing movements into and out of prison. This predicted change serves as our instrument for actual future increases in incarceration. Absent controls, our instrument explains nearly one-fifth of the variance in one-year changes in incarceration rates.

Using state level data for the United States covering the period from 1978 to 2004, we find crime-prison elasticities that are considerably larger than those implied by OLS estimates. For the entire time period, we find average crime-prison effects with implied elasticities of between -0.06 and -0.11 for violent crime and between -0.15 and -0.21 for property crime. We also present results for two sub-periods of our panel: 1978 to 1990 and 1991 to 2004. Our IV estimates for the earlier time period suggest much larger crime-prison effects, with elasticity estimates consistent with those presented in Levitt (1996) who analyzes a similar time period yet with an entirely different identification strategy. For the latter time period, however, the effects of changes in prison on crime are much smaller. Our results indicate that recent increases in incarceration have generated much less bang-per-buck in terms of crime reduction.

2. Incarceration and Crime: A Review of the Existing Research

Incarceration may impact the overall level of crime through a number of causal channels. To start, incarceration mechanically incapacitates the criminally active. In addition, the threat of incarceration may deter potential criminal offenders from committing a crime in the first place; a causal path referred to as general deterrence. Over the longer term, prior prison experience may either reduce criminal activity among former inmates who do not wish to return to prison (referred to as specific deterrence) or perhaps enhance criminality if a prior incarceration increases the relative returns to crime.

Criminological research on the effect of incarceration on crime has focused mainly on the potential incapacitation effects of prison. Much of this research indirectly estimates incapacitation effects by interviewing inmates regarding their criminal activity prior to their most recent arrest and then imputing the amount of crime that inmates would have committed from their retrospective responses. Results from this research vary considerably across studies (often by a factor of ten), a fact often attributable to a few respondents who report incredibly large amounts of criminal activity. The most careful reviews of this research suggest that, on average, each additional prison year served results in 10 to 20 fewer serious felony offenses (see the discussion in Marvell and Moody 1994 and the extensive analysis in Spelman 1994, 2000).[2]

By construction, these incapacitation studies provide only a partial estimate of the effect of incarceration on crime since they are unable to detect whether potential offenders are deterred by the threat of incarceration.[3] Moreover, the likely unreliability of inmate self reports and the large cross-study variation in results suggests the need for alternative strategies. Given these limitations, several scholars have attempted to estimate the overall effect of incarceration using aggregate crime and prison data. However, these studies must address an alternative methodological challenge: namely, the fact that unobserved determinants of crime are likely to create a simultaneous relationship between incarceration and crime.

Marvell and Moody (1994) are perhaps the first to estimate the overall incarceration effect using state-level panel regressions. The authors use a series of Granger causality tests and conclude that after first-differencing the data, within-state variation in incarceration is

[2] More recent research in this vein attempts to directly estimate incapacitation effects by observing the criminal behavior of former inmates after release. Owens (2009) exploits a sentence dis-enhancement to estimate the effect of shorter sentences on overall crime. In 2003, the state of Maryland discontinued the practice of consideration of one's juvenile record in the determination of sentences for adult offenders between 23 and 25 years of age. Owens estimates that this change in sentencing procedures reduced the time served by 200 to 400 days for adult offenders in the effected age range with prior juvenile convictions. By observing arrests during the period when they would have been incarcerated had they been sentenced under the prior sentencing regime, Owens estimates that the sentence dis-enhancement increases the number of serious offenses by roughly 2 to 3 index crimes per offender per year of street time.

[3] There is a separate and growing literature attempting to estimate general deterrence effects of incarceration. Kessler and Levitt (1999) estimate the effect of sentence enhancements for violent crime on overall offending arguing that the crimes receiving the enhancement would have resulted in incarceration regardless and thus any short term effect of the enhancement on crime is attributable to pure deterrence. Webster, Doob, Zimring (2006), however, argue that the deterrence estimates in Kessler and Levitt are driven by crime rates that were already trending downwards and thus are spurious. A separate set of studies attempts to estimate general deterrence effects by exploiting the discontinuous increase in sentences for offenses occurred at 18 years of age. Levitt (1998) finds a decrease in offending when youth reach the age of majority while Lee and McCrary (2005) find no evidence of such an effect. More recently, Drago et. al. (2009) exploit a unique feature of a 2006 Italian mass pardon to identify general deterrent effects. The Italian pardon released most inmates with three years or less remaining on their sentence. Those who re-offended post release faced an enhanced sentence through the adding on of the remainder of one's un-served time to whatever new sentence it meted out for the new post-release offense. The authors find that those inmates who faced a longer sentence enhancement (conditional on observables) were less likely to reoffend after being released.

exogenous. Marvell and Moody subsequently estimate the effect of incarceration on crime using a first-difference model with an error correction component to account for the co-integration of the crime and prison time series. The authors estimate an overall crime-prison elasticity of -0.16.

Levitt (1996) also estimates the effect of incarceration on crime using a state-level panel data model. Unlike Marvell and Moody, however, Levitt explicitly corrects for the potential endogeneity of variation in incarceration rates. Levitt exploits the fact that in years when states are under a court order to relieve prisoner overcrowding, state prison populations grow at a significantly slower rate relative to states that are not under such court orders. Using a series of variables measuring the status of prisoner overcrowding lawsuits as instruments for state level incarceration rates, Levitt finds 2SLS estimates of crime-prison elasticities that are considerably larger than comparable estimates from OLS with a corrected property crime-prison elasticity of -0.3 and a violent crime-prison elasticity of -0.4.

To be sure, the relatively large estimates in Levitt (1996) have been criticized based on the choice of instruments. For example, Western (2006) argues that most of the states being placed under court order are southern states during the 1980s and early 1990s. To the extent that Levitt is capturing a local average treatment effect specific to the South during this time period, the large estimates may not generalize to the country as a whole. Donohue and Siegelman (1998) argue that Levitt's chosen instruments may themselves be endogenous, as states that have had unusually large increases in prison populations are more likely to come under court order to relieve overcrowding.

In our assessment, Levitt is surely correct in arguing that OLS estimates of the prison-crime effect are likely to be biased towards zero by a reverse causal effect crime on incarceration. Moreover, the first-stage relationship between incarceration and his law suit variables is strong, well documented, and well argued. Given the paucity of estimates that correct for the endogeneity of prison, however, as well as the quite large estimates presented by Levitt, further research on the bias in simple first-differenced or fixed effect crime models is necessary. In what follows, we present an alternative identification strategy.

3. Methodological Framework

In this paper, we follow Marvell and Moody (1994) and Levitt (1996) in estimating the overall crime-prison effects using state-level panel data regressions. Our principal innovation is that we derive an instrument for future increases in incarceration rates based on the predicted dynamic adjustment of incarceration rates to changes in the underlying transition probabilities describing the incarceration stochastic process. Among the benefits of our strategy, the principal benefit is that regardless of the source of the shock (e.g., change in underlying criminal behavior, increased enforcement, longer sentences), the dynamic adjustment of the incarceration rate to any permanent shock provides exogenous variation in future incarceration changes that can be used to identify the crime-incarceration effect. A second benefit concerns the fact that the instrument can be defined for all time periods and states, and thus we are able to explore whether the marginal incarceration effects are changing over time.

To illustrate our strategy, we first present a simple aggregate model of incarceration and crime where we assume an exogenous shock to the underlying criminality of the populace and where we assume further that criminal activity is not responsive to variation in the contemporaneous incarceration rate or enforcement (i.e., there is no general deterrent effect). We subsequently discuss how the model is altered by the incorporation of behavioral responses to changes in incarceration rates.

A. A simple model of incapacitation

Suppose that at any given time the members of a population can be described by the current state, *i,* where *i=1* corresponds to not being incarcerated and *i=2* corresponds to being incarcerated. Define the vector $S'_t = [S_{1,t} \quad S_{2,t}]$, where $S_{i,t}$ is the proportion of the population in state *i* in period *t*. Assume that the periodic probability that any individual commits a crime is giving by the constant parameter *c*. Assume further that only the non-incarcerated can commit crime -- i.e., incarceration mechanically incapacitates potential offenders. We also assume that the likelihood of being caught and sent to prison conditional on committing a crime is given by the parameter *p*. Taken together, these assumptions indicate that the transition probability from non-incarceration to incarceration is simply *cp*, while the fraction flowing into prison between periods *t* and *t+1* is given by $cpS_{1,t}$. Finally, we assume that the periodic probability of being released from prison is given by the parameter θ for all inmates and time periods.

For any period *t* the population distribution across the two states is determined by an equation relating current population state shares to last period's population shares,

(1) $$S_t' = S_{t-1}'T$$

where $T=[T_{ij}]$ is a transition probability matrix with each element representing the likelihood that a person in state *i* transitions to state *j*. Given our assumptions, the transition matrix is given by

(2) $$T = \begin{bmatrix} 1-cp & cp \\ \theta & 1-\theta \end{bmatrix}$$

where the first row provides the survival and hazard functions (respectively) for the non-incarcerated, while the second row of the matrix provides the hazard and survival functions for the incarcerated.

Equation (1) gives the relationship between the distribution of persons across states in period *t* and the comparable distribution in period *t-1*, and suggests that this distribution changes over time according to the elements of *T*. Given enough time periods, however, the proportions not incarcerated and incarcerated will eventually settle to steady-state values. Assuming stability in the elements of T, the steady state is defined by the equation

(3) $$S^{*\prime} = S^{*\prime}T,$$

where we have dropped the time subscript and added an asterisk to indicate the steady-state population share vector. When combined with the constraint $S^*_1 + S^*_2 = 1$, the steady-state population shares can be expressed as

(4) $$S^*_1 = \frac{\theta}{cp+\theta}$$
$$S^*_2 = \frac{cp}{cp+\theta}.$$

With these specific values, the equilibrium crime rate can be derived by multiplying the proportion not-incarcerated by the probability that someone commits a crime. This yields

(5) $$Crime^* = cS^*_1 = c(1 - S^*_2) = \frac{c\theta}{cp+\theta}.$$

A comparative static analysis of the steady-state shares in equation (4) and the equilibrium crime rate in equation (5) can be used to highlight the fundamental identification problem faced by empirical studies of the crime incarceration effect that make use of aggregate data. Within the context of the simple mechanical model derived here, such studies seek to uncover the individual propensity to commit crime – i.e., the parameter c. Negative one times this parameter gives the reduction in crime that would occur through increased incapacitation with a one-person increase in the incarceration rate. The aggregate analysis seeks to uncover this parameter by empirically estimating the effect of a change in the incarceration rate on crime rates, or $\frac{\partial Crime^*}{\partial S^*{}_2}$. Whether this aggregate relationship reveals the parameter, c, will depend on which of the three underlying parameters is driving the change in incarceration and crime.

For example, suppose that a sentence enhancement reduces the value of the parameter θ (effectively increasing sentence length). In this instance, the empirically-observed change in crime co-occurring with the observed change in incarceration would be given by $\frac{\partial Crime^*}{\partial S^*{}_2} = \frac{\partial Crime^* / \partial \theta}{\partial Crime^* / \partial \theta} = -c$. As this is the parameter we seek to estimate, the aggregate analysis in this instance would yield an unbiased estimate. Similarly, when variation in incarceration and crime is driven by an exogenous shock to the apprehension parameter, p, the empirical estimate of the change in crime caused by a change in the incarceration rate will also equal $-c$. That is to say, exogenous policy variation in the incarceration rate identifies the parameter of interest and reveals the crime reduction caused by an increase in incarceration.

However, a change in incarceration caused by an exogenous change in criminal behavior will not uncover the criminality parameter. An increase in criminal behavior (operationalized as an increase in the parameter c), will cause both an increase in incarceration rates as well as an increase in crime rates, --i.e., $\frac{\partial S^*{}_2}{\partial c}, \quad \frac{\partial Crime*}{\partial c} \quad > \quad 0$. The ratio of the latter derivative to the first (corresponding the naïve empirical estimate of $\frac{\partial Crime^*}{\partial S^*{}_2}$) yields the solution $\frac{\theta}{p}$. As both of these parameters are positive, variation in incarceration and crime caused by exogenous shocks to criminal behavior may create the false impression that higher incarceration leads to higher crime. At a minimum, the dependence of crime and incarceration on underlying variation in criminal propensities will positively bias empirical estimates of $\frac{\partial Crime^*}{\partial S^*{}_2}$ that do not account for this simultaneity problem.

Most panel data studies of crime and incarceration do not regress changes in steady-state crime rates on changes in steady-state incarceration rates, since shocks to the underlying parameters of the two variables will induce multi-period adjustment processes towards new steady-state values. To the extent that observed temporal variation in crime and incarceration are at a relatively high frequency (annual data for instance), changes in these variables will reflect both responses to contemporaneous changes in underlying transition parameters as well as the dynamic adjustment between equilibrium caused by past changes in the underlying parameters. The innovation we highlight here exploits variation in incarceration caused by past changes in the various transition probabilities that is arguably independent of contemporaneous and subsequent changes in parameter values. As we show below, variation in incarceration associated with such longer term adjustment to earlier shocks is plausibly exogenous and can be used to identify the crime-prison effect.

To illustrate this point, we derive the adjustment paths of crime and incarceration to a permanent change in the propensity to commit crime and show that variation along this adjustment path *beyond the first period response* can be used to identify the parameter of interest *c*. As the preceding comparative-static analysis demonstrated, this is the worse-case-scenario source of variation for the purpose of estimating the crime-prison elasticity.[4] With dynamically-lagged responses of incarceration to this change, however, good variation in incarceration can be isolated to measure the underlying causal relationship.

Suppose the system is initially in steady-state with a value for the criminality parameter equal to c_0 at time $t=0$. The propensity to commit crime then increases at $t=1$ from c_0 to c_1. For any period $t > 0$, the proportion incarcerated is given by

(6)
$$S_{2,t} = S_{1,t-1}c_1 p + S_{2,t-1}(1-\theta),$$

where we have now re-introduced the time subscript since it is no longer presumed that we are in the steady state at any point in time. Substituting ($1-S_{2,t-1}$) for the share not incarcerated in period t-1 and rearranging yields the expression

(7)
$$S_{2,t} + S_{2,t-1}(c_1 p + \theta - 1) = c_1 p$$

which is in the form of a simple linear difference equation. To derive an explicit description of the dynamic path of incarceration as a function of time and the underlying transition parameters, we need to define the initial condition at t=0 for the incarceration rate. Since the system was in steady state before the shock, the incarceration rate at time t=0 is, $S^*{}_{2,t=0} = \dfrac{c_0 p}{c_0 p + \theta}$. With this initial condition, solving equation (7) as a function of the parameters and time gives the expression

$$S_{2,t} = \left[\frac{c_o p}{c_o p + \theta} - \frac{c_1 p}{c_1 p + \theta}\right][1 - c_1 p - \theta]^t + \frac{c_1 p}{c_1 p + \theta}$$

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4 Similar arguments apply to permanent changes in the composite apprehension and conviction probability or the release from prison probability. In fact, the instruments in Levitt (1996) are most likely operating through an exogenous shift in θ. Here we focus on the adjustment to changes in the criminality parameter *c* since is thought by most to be the most likely contaminating omitted factor. Nonetheless, the ensuing argument and instrumental variables strategy applies to permanent changes in any of the parameters of the transition probability matrix.

which can be rewritten as

(8) $$S_{2,t} = (S^*_{2,t=0} - S^*_{2,t>0})(1 - cp - \theta)^t + S^*_{2,t>0}$$

where $S^*_{2,t=0} = \dfrac{c_0 p}{c_0 p + \theta}$ is the old steady-state incarceration rate prior to the change in criminality while $S^*_{2,t>0} = \dfrac{c_1 p}{c_1 p + \theta}$ is the new steady-state incarceration rate that will eventually be reached given stability in the parameters and enough time.

Equation (8) shows that the incarceration rate at any time t>0 is equal to the new steady-state incarceration rate (the second term on the right) plus a proportion of the disparity between the old and new steady-state rates. Since the first term is negative and since 0< (1-cp-θ) < 1 for typical values of these parameters,[5] Equation (8) depicts a stable process whereby the incarceration rate approaches the new steady-state from below. The adjustment path is depicted in Figure 1. Note, incarceration increases between t=0 and t=1 due to the increase in the criminality parameter. Subsequent increases, however, are not driven by further changes in c, since we have assumed a one-time permanent shock to criminality. Rather, subsequent increases reflect the dynamic multi-period adjustment of incarceration towards its new equilibrium rate. For annual data for U.S. states, a typical value for θ is roughly 0.5 while a typical value for cp (the flow rate into prison) is at most 0.01. These values combined with equation (8) suggest that the incarceration rate becomes quite close to its new equilibrium value after five or six years.

[5] In theory, it is plausible that the term (1-cp-θ) could fall below zero. This would require a very high release probability and a very large inflow rate into prison. If this were the case and if -1 < 1-cp-θ < 0, then the incarceration would still converge asymptotically to the higher steady-state value. However, rather than approaching the steady state from below, the adjustment path would oscillate above and below the long run steady-state, with the oscillation variance diminishing with time. In practice, the prison releases rate in the U.S. hovers around 0.5 and the transition into prison, cp, is consistently below 0.01. Hence, for practical purposes it's safe to assume that 0 < 1-cp-θ < 1.

We can derive a similar adjustment path for the crime rate. Substituting the time path for incarceration into equation (5) and rearranging yields the expression,

(9) $$Crime_t = c_t(S^*_{2,t>0} - S^*_{2,t=0})(1-cp-\theta)^t + c_t(1-S^*_{2,t>0})$$

where crime at time *t*>0 consists of two components: the new steady-state crime rate (the second terms on the right side of equation (9)), and the deviation from the new steady-state associated with the dynamics adjustment (the first term). Here, the adjustment term is positive and approached zero as *t* increases, implying that crime approaches its new equilibrium from above. Given that the new steady-state crime rate will exceed the old steady-state crime rate, equation (9) indicates that in response to a permanent increase in criminality, crime increases discretely and then declines to the new equilibrium over time. The time path for this variable is also depicted in Figure 1.

The patterns observed in Figure 1 hint at our identification strategy. Between periods *t*=0 and *t*=1, both crime and incarceration increase as a results of the discrete increase in the criminality parameter from c_0 to c_1. Clearly, the positive covariance between the two variables for this first difference (both crime and incarceration increase) is driven by the change in criminality. Thus a regression of a series of such first-period changes in crime against first-period change in incarceration will yield a spurious positive coefficient.

This is not the case, however, for subsequent changes in these series. For all changes beyond the first, the criminality parameter is held constant yet the incarceration rate increases as it approaches its new equilibrium rate. With regards to crime, following the initial discrete increase, subsequent increases in incarceration decrease the crime rate by incapacitating a greater proportion of the population. In other words, the decline in crime along the adjustment path beyond the change between periods 0 and 1 is driven by the increase in incarceration. In fact, the ratio of the changes in crime to the change in incarceration for any of these subsequent periods would yield negative one times the criminality parameter –i.e., the incapacitation effect that we seek to estimate. Thus, if one could discard the variation associated with the initial shock and isolate variation in subsequent movements associated with the dynamics adjustment to the shock, one could identify the incapacitation effect of marginal increases in incarceration.

The exogeneity of subsequent changes in incarceration in this model is best illustrated by deriving explicit expressions for the change in incarceration and crime following permanent shocks to criminality. Let $\Delta S_{2,t} = S_{2,t+1} - S_{2,t}$ and $\Delta C_t = C_{t+1} - C_t$. From equation (8), explicit expressions for one period changes in incarceration rates for t=0, t=1, and t>1 are

(10)

$$\Delta S_{2,0} = (S^*{}_{2,t>0} - S^*{}_{2,t=0})(c_1 p + \theta)$$

$$\Delta S_{2,1} = (S^*{}_{2,t>0} - S^*{}_{2,t=0})(c_1 p + \theta)(1 - c_1 p - \theta)$$

$$\Delta S_{2,t} = (S^*{}_{2,t>0} - S^*{}_{2,t=0})(c_1 p + \theta)(1 - c_1 p - \theta)^t$$

Given that (1-c_1p-θ) <1, the first change in incarceration rate is the largest and each subsequent change diminishes in size.

By equation (9), an explicit expression for the first-period change in crime is

(11)
$$\Delta Crime_o = -c_1 \Delta S_{2,0} + (c_1 - c_0)(1 - S^*{}_{2,t=0}),$$

which has two components, one of which we are interested in uncovering. The second term on the right side of (11) gives the change in crime associated with the increase in criminality holding the incarceration rate at its equilibrium in period t=0. This component is positive and drives the initial spike in crime. The first term on the right side of (11) shows the decline in crime association with the first-period increase in incarceration. Thus, the discrete increase in crime in Figure 1 entails the sum of two effects: the effect of an increase in criminality (the larger of the two) and the partially offsetting effect of the contemporaneous increase in incarceration.

In practice, we observe the total change in crime and the change in incarceration and wish to estimate the coefficient $-c_1$ associated with the first component in equation (11). We do not observe the second term on the right side of (11) and thus it is swept into the residual of an OLS regression. Given that the contemporaneous change in criminality will be positively correlated with the contemporaneous change in incarceration, an OLS regression of $\Delta Crime_0$ on $\Delta S_{2,0}$ will yield a positively biased estimate of $-c_1$. This argument is similar to the identification problem that we highlighted in the comparative static analysis.

However, changes subsequent to the first-period change will not suffer from this bias. To see this, the explicit expression for the next change in crime is given by

(12) $$\Delta Crime_1 = -c_1 \Delta S_{2,1}.$$

Here the crime change is a function of the change in incarceration alone. This follows from the fact that we are modeling a one-time permanent increase in criminality, and thus, the contaminating second term in (11) drops out for all subsequent changes in crime until the crime rate reaches its new steady state level. Most importantly, taking the ratio of equation (12) to the second line of equation (10) yields the parameter of interest $-c_1$.

Together, equations (10) and (12) provide the heart of our identification strategy. The second line of equation (10) provides a prediction for the change in incarceration rates between periods 1 and 2 associated with an increase in criminality between periods 0 and 1. Since this predicted increase is not driven by contemporaneous changes in criminal behavior between periods 1 and 2, a variable constructed from the second line of equation (10) could serve as an instrument for actual changes in incarceration. Our principal strategy is to use this prediction to instrument changes in incarceration in a series of crime models where the incarceration rate is the principal explanatory variable of interest. In other words, we estimate the difference equation (12) where the actual change in incarceration is instrumented with the corresponding predicted change in incarceration from equation (10). In essence, the strategy identifies variation in incarceration that "would have occurred anyway" and uses this variation to identify various crime-incarceration effects.

While we have not performed a similar analysis of the dynamic adjustment processes of crime and incarceration to underlying shocks to the policy parameters, p and θ, it should be noted that to the extent that such shocks are causing variation in crime and incarceration a simple OLS regression of crime rates on incarceration rates is indeed identified. Specifically, making reference to the change in crime in equation (11), the contaminating second term occurs due to the exogenous shock to criminality. If changes in incarceration and crime are being driven by shocks to parameters other than the criminality parameter, this contaminating factor drops out of the equation. Hence, crime responds to a change in p or θ only through the impact of these parameters on the incarceration rate, and thus the OLS regression would provide an unbiased estimate of the pure incapacitation effect.

B. Behavioral Responses to Changes in Criminality and Enforcement

Thus far, we have assumed that the underlying transition parameters in our aggregate model of crime and incarceration do not respond to one another, either instantaneously or with a dynamic lag. Clearly this assumption is unrealistic as we would expect policy makers to respond to changes in the prevalence of criminal behavior and potential criminals to perhaps respond to changes in policy. For our narrow purposes, we are particularly interested in whether incorporating behavioral responses compromises our identification strategy.

In appendix A, we provide a detailed discussion that sequentially relaxes some of the behavioral assumptions that we have implicitly made. Here, we highlight how the incorporation of such behavioral responses is likely to impact our first-stage prediction, the potential exogeneity of our instrument, and the interpretation of our empirical results. While we work through quite specific responses and the dynamic structure below and in the appendix, the following general points can be gleaned from this exercise.

First, allowing for endogenous responses of the policy parameters to an underlying criminality shock will influence the strength of our first stage (2SLS model), but does not compromise the exogeneity of our instrument. For several possible policy responses, we show that the one-lead change in incarceration rates can be decomposed into a component equal to our prediction assuming no behavioral response (via equation (10) above) as well as additional components associated with the second-order responsive changes in p and θ. Since these parameters only influence crime through the incapacitation and deterrence effects of incarceration, we can still identify a causal effect as long as our instrument predicts significant variation in the change in incarceration.

Second, when one allows for a reciprocally-responsive relationship between criminality and enforcement, we can no longer interpret the coefficient from OLS as a pure incapacitation (as in our simple model above). Specifically, increases in enforcement caused by an exogenous shock to behavior may induce any increase in criminality to be subsequently dulled by the higher incarceration risk. When this is the case, the empirical association between incarceration and crime will be driven by incapacitation as well as deterrence. In the context of our instrumental variable strategy, our estimates will yield a biased estimate of the pure incapacitation effect. However, our estimate of the total effect of prison on crime is unbiased as long as we interpret the estimate more broadly.

Finally, there are certain conditions under which the identification strategy proposed here would fail. The most obvious would be when changes in the criminality parameter exhibits negative serial correlation for reasons that are independent of policy. For example, if a wave of drug-related crime today leads to the emergence of informal social controls that are external to the criminal justice system that subsequently reduce criminality, the predicted future increase in incarceration may be spuriously inversely related to future crime declines.

In the appendix, we provide a more detailed analysis of the consequences of allowing for the following behavioral relationships.

- **Allowing enforcement to respond to changes in criminality – i.e., $p=p(c)$.** One might hypothesize that p may either by increasing or decreasing in the degree of criminality. If policy makers increase enforcement in response to an increase in c, an elevated propensity to commit crime may be matched by an elevated incarceration risk. On the other hand, an increase in c may dilute enforcement resources and reduce the risk of incarceration. Such a policy reaction should not compromise the exogeneity of our instrument, although the timing of the response may impact the strength of our first-stage prediction. If p responds instantaneously to changes in c, the behavioral response will either speed up or slow down the adjustment processes of crime and incarceration to their new equilibrium values (depending on the sign of dp/dc). If p responds with a lag, our instrument will either over or under-predict the actual change in incarceration. However, the proposed instrument is still orthogonal to the second-stage error term.
- **Allowing sentence severity to respond to changes in criminality – i.e., $\theta=\theta(c)$.** Assessing the effect of a change in sentence length on our identification strategy by necessity requires a dynamic analysis since a change in sentence length today will not impact incarceration rates until today's cohort of admitted inmates reaches their counterfactual release dates under the prior sentencing regime. Assuming a one-period lag in the response of θ to a change in c, an increase in sentence length (operationalized as a decrease in θ) in response to an increase in c implies that our instrument will under-predict the change in incarceration following the initial behavioral shock. While this introduces error into our first-stage prediction, the instrument is still exogenous to the unobserved determinants of future changes in crime.
- **Allowing criminality and enforcement to reciprocally react – i.e., $p=p(c)$, $c=c(p)$.** The implications of allowing simultaneous determination of the criminality and the

incarceration risk parameters for our identification strategy will depend on whether these adjustments will occur instantaneously or over time. Moreover, if the reaction processes are dynamic, the timing and sequencing of the reactions are important in assessing how such behavioral responses would impact the interpretation of our empirical results. If we assume that criminality responds instantaneously to change in *p*, while enforcement responds with a lag to changes in *c*, our proposed instrumental variables strategy will yield a biased estimate of a pure incapacitation effect. This bias is driven by the fact that the error term in the second-stage equation relating changes in crime to changes in incarceration will include a component reflecting the behavioral response of criminal behavior to enhanced enforcement (a term which will be negatively correlated with our predicted change in incarceration). However, since this component is essentially a general deterrent effect, the structural estimate of the effect of incarceration on crime still represents a causal effect, as long as this estimate is interpreted as the overall impact of a change in incarceration (incapacitation plus deterrence). With regard to the first-stage prediction, reciprocal reactionary responses between *p* and *c* imply that future increases in incarceration in response to a change in *c* may either be smaller or larger than a non-behavioral model would predict, since the effect of enhanced enforcement on incarceration is offset by subsequent deterrence-induced declines in criminality.

- **Allowing future change in criminality to respond to previous change in criminality.** Suppose that current increases in criminal behavior cause subsequent decreases in criminality due to a revulsion on the part of those likely to commit crime in response to the consequences of an initial crime spike. Such an effect would induce negative serial correlation in changes in the criminality parameter and would likely induce a spurious negative correlation between our instrument (which is increasing in the past periods increase in criminality behavior) and future changes in crime rates (which would be negatively impacted by the hypothesized reactive behavior). Of course, if the periodicity of our data is such that the one-period lead prediction we employ as an instrument predicts changes in incarceration before such revulsion wells up and impacts crime, our IV strategy would still be valid.

To summarize, with the exception of the final possibility, incorporating behavior into our model may impact the precision of our first-stage prediction but does not compromise the exogeneity of the proposed instrument. If we allow criminal behavior and corrections policy to simultaneously respond to one another, we need to interpret the IV results as an overall effect of incarceration on crime, rather than as an estimate of a pure incapacitation effect. Nonetheless, the IV estimates still carry a causal interpretation.

Our strategy would not be suitable if criminal behavior reacts negatively to previous increases in criminal behavior (through channels not mediated through a change in enforcement or sentencing). Above we offer the example of the emergence of informal social controls intended to mitigate an increase in criminality. However, there are strong reasons to believe that such responses would likely take more than one year (the time frame of our prediction), and thus are unlikely to compromise our results. Nonetheless, we acknowledge this potential weakness.

4. Data Description and Documentation of the First-Stage Relationship

Implementing our identification strategy requires that we obtain information on the transition probabilities between incarceration and non-incarceration by state and year. Our strategy also requires that we identify permanent changes in underlying transition probabilities. Finally, there are very few states and time periods where changes in incarceration fit the model of a one-time increase in criminality with a delayed dynamic adjustment. In fact, over the last twenty plus years, most states have experienced repeated increases in prison admission rates. Thus, we must adapt our strategy to incorporate these serial shocks. In this section, we describe the data for this project and the manner in which we use these data to implement our identification strategy.

Our first task is to estimate the transition probabilities by state and year, since our proposed instrument requires information on cp and θ. Doing so requires four pieces of information: aggregate annual flows into prison, aggregate annual flows out of prison, the stock of prisoners for a given year, and an estimate of the total state population. We obtain data on aggregate flows into an out of prison by state and year from the National Prison Statistics (NPS) data base. These data provide the total admissions and total releases from prison within a calendar year. Data on the stock of prison inmates under each state's jurisdiction come from the Bureau of Justice Statistics and measure the stock of inmates as of December 31st of the stated calendar year. State level population data come from the U.S. Census Bureau. We estimate the transition probability from non-incarceration to incarceration by dividing total prison admissions by the total state population less the inmate population. We calculate the incarceration/non-incarceration transition probability by dividing total annual prison releases by the stock of prison inmates.

The next task involves adapting our instrumental variable to the fact that states are subject to serial shocks in transition probabilities rather than single shocks. Our manner of doing so is illustrated for three years for the state of New York in Table 1. The first row of the table provides current incarceration rates for four years (1979 through 1982) while the second and third rows provide our estimates of the non-incarceration-to-incarceration and incarceration-to-non-incarceration transition probabilities. For the latter three years, the next row presents the equilibrium incarceration rate implied by the empirical transition probabilities for each year. Note, in each year the equilibrium values exceed the actual incarceration rate. For any given year, we designate t=0 as corresponding to the year previous. For example, for the purpose of predicting the increase in incarceration rates between 1980 and 1981, we designate 1980 as t=1 and 1979 as t=0. In predicting the change between 1981 and 1982, we set t=1 in 1981 and t=0 in 1980, and so on. Thus, the starting value for the dynamic adjustment for any given year is always defined as the one-period lagged incarceration rate. These starting values (incarceration at t=0) are presented in the fifth row of the table.

The sixth row of the table displays the predicted change in incarceration between t=1 and t=2 for each year based on all of the values that are already determined by time period t=1 (with the exception of the initial incarceration rate which is determined by t=0) using the second expression equation (10). Finally, the last row presents the actual change in incarceration rates between t=1 and t=2. Our identification strategy uses the variable displayed in the second-to-last row as an instrument for the first-differences in incarceration rates displayed in the last row.

Finally, our identification strategy requires that we identify permanent changes to the underlying transition probabilities (which may subsequently be enhanced or diminished by future changes in these probabilities). To the extent that the observed changes in empirical admission and release hazards reflect temporary rather than permanent changes, our instrument will serve as a poor predictor of future actual change in incarceration rates.[6] To minimize the influence of temporary shocks to the transition probabilities, we first smooth the transition probability time series for each state and use the smoothed series to construct the predicted change in incarceration as illustrated in Table 1. For each state, we estimate a simple regression where a given transition probability is regressed on an eighth-order polynomial time trend. We then calculate the predicted value for the transition probability from the estimated regression function. We estimate this model for each of the 50 states plus D.C. for the admission hazard as well as the release hazard (102 models in all). These predicted transition probabilities are then used to construct our instrumental variable.[7]

Our panel data set covers the period from 1978 to 2004 and covers the 50 states and D.C.[8] Figure 2 presents a scatter plot (weighted by state level population counts) of the actual annual changes in incarceration rates for the entire panel against the predicted changes from

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[6] Moreover, if temporary increases in criminality cause subsequent increases in incarceration rates due to time lags between arrest and incarceration or a spurt of criminal activity at the end of the year, temporary shocks to the transition probabilities may induce a spurious negative relationship between our instrument and crime.

[7] Constructing the instrument using the smoothed transition probabilities rather than the raw numbers improves the fit of the first stage. For example, a weighted regression of the changes in incarceration rates on the predicted changes using the raw transition probabilities yields an R^2 of 0.137. The comparable value using the smoothed transition probabilities is 0.164. Moreover, the structural estimates using the instrument based on the smoothed transition probabilities tend to be slightly smaller than those using the raw transition probabilities, with fairly large disparities in specifications where the first stage is the weakest. These additional results are available from the authors upon request.

[8] There are a few missing observations for Alaska and two missing observations at the end of the time series for Washington D.C., when the metropolitan area abandoned its prison system.

equation (10) (based on the smoothed state-level transition probabilities). Several notable patterns stand out. First, there is a strong correlation between our instrument and actual changes in incarceration (approximately 0.40), with the instrument explaining roughly 16 percent of the variation in annual changes in incarceration rates. Second, the lion's share of predicted increases in incarceration are positive (roughly 86 percent), suggesting that for most states and time periods the observed incarceration rate is below the steady-state rate implied by the value of their respective transition probabilities. Finally, the coefficient on the instrument is substantially less than one (0.61), suggesting that the instrument is over-predicting the actual change in incarceration. Note, this is consistent with a reciprocal responsiveness between criminal propensity and enforcement, with subsequent declines in criminality in response to enhanced enforcement moderating the impact of a crime shock on incarceration (see Appendix A for a detailed discussion of this possibility).

Table 2 assesses the robustness of this first-stage relationship to the inclusion of year fixed effects, state fixed effects and a series of state-level controls for changes in the age structure, the percent minority, the percent poor, the state unemployment rate, and state per-capita income.[9] Adding year effects removes the influence of any factor impacting incarceration over time that are constant across states. Since the dependent variable (as well as all of the explanatory variables) is specified in first-differences, adding a complete set of state-fixed effects adjusts for state-specific linear time trends in incarceration after adjusting for common national year-to-year changes. All of the models in Table 2, as well as all of the models presented throughout the paper, are weighted by state-level population counts. The first column

[9] Data on the percent of a state's residents within a given age group, the percent black, and the percent poor come from the U.S. Census Bureau. State level unemployment rates come from the Bureau of Labor Statistics while data on per-capita income come the Bureau of Economics analysis. All of the data used in this project are available upon request.

repeats the simple bivariate regression depicted in Figure 2. The second regression adds observable covariates. Regression (3) adds year effects to the specification in regression (2), while regression (4) adds state effects. While the coefficient on the predicted change in incarceration diminishes slightly across specifications and the standard error on the point estimate increases, the instrument is highly significant in all specifications. The F-statistics from a simple test of the significance of the instrument are all above 160. Thus, the first-stage relationship is quite strong for the overall panel and persists with the inclusion of observable covariates.

We use the first-stage models in Table 2 to identify the effect of changes in incarceration on changes in crime rates. With regards to our dependent variables, we test for effects on all of the seven part 1 felony offenses included in the Federal Bureau of Investigation Uniform Crime Reports (UCR). The UCR counts all serious felony offenses reported to the police by state and year. Averages crime rates for individual offenses and overall violent crime (the aggregation of murder, rape, robbery and assault) and overall property crime (the aggregation of burglary, larceny, and motor vehicle theft) are presented in Table 3, along with the average state-level incarceration rate. All rates are expressed as the number of incidents per 100,000 state residents (or inmates per 100,000 for the incarceration rate). The table also presents descriptive statistics for two sub-periods (1978 to 1990 and 1991 to 2004) which we use later to stratify the time period in order to estimate period-specific incarceration effects.

The means by sub-period suggest that overall violent crime rate increased slightly while overall property crime declined considerably in the latter period relative to the former. Data from victimization surveys indicate that both property as well as violent crime rates declined substantially over these time periods. The higher violent crime rates in the latter period most likely reflects increased reporting of violent crime to police,[10] a problem in the UCR data frequently noted in past research (Donohue and Siegleman (1998), Levitt (1996), Spelman (2000)). In light of this problem, all of the crime models that we estimate below include year fixed effects.

5. Empirical Results Using the Entire Sample Period

Table 4 presents a series of regression model estimates where the dependent variable is either the annual change in the overall violent crime rate or the annual change in the overall property crime rate. For each dependent variable, we present the results from four model specifications. First, we estimate an OLS model that regresses the annual change in the crime rate on the corresponding change in the incarceration rate, with controls for changes in socioeconomic and demographic variables (same set of controls variables included in the specification in Table 2), and a complete set of year fixed effects. Next, we re-estimate this model employing our predicted change in incarceration from equation (10) as an instrument for the actual change. We then re-estimate the OLS and IV models using the initial specification plus a complete set of state-level fixed effects. Note, the first-stage models for the IV results correspond to specifications (3) and (4) in Table 2.

Beginning with the results for violent crime, in both OLS models the coefficient on the change in incarceration is small, positive, and statistically insignificant. Instrumenting with the predicted change in incarceration turns the coefficient negative, and for the final specification, statistically significant at the 10 percent level of confidence. The most complete specification suggests that each additional inmate reduces the annual number of violent crimes by 0.23

[10] In fact, analysis by the Bureau of Justice Statistics shows that the crime reporting rates have increased over the past two to three decades. See “Facts at a Glance” http://www.ojp.usdoj.gov/bjs/glance.htm (accessed on November 30, 2006).

incidents. This is a considerably larger effect than the small positive impact (0.038) suggested by the corresponding OLS model. Thus, correcting for simultaneity greatly increases effect sizes.

Turning to the results for overall property crime, there is more consistent evidence of a significant negative effect of incarceration on crime in all models. Beginning with the models omitting state fixed effects, a one-person increase in the incarceration rate is predicted to reduce the property crime rate by approximately one. The IV estimate, however, indicates a much larger effect, with a coefficient on the change of incarceration of -2.315. Both estimates are statistically significant at the one-percent level of confidence and statistically distinguishable from one another. Adding state fixed effects increases these estimates, marginally for the OLS model (to -1.109) but appreciably for the IV model (to -3.272). Similar to the results for violent crime, the juxtaposition of the OLS and IV estimation results strongly suggests that OLS estimates are biased towards zero by the simultaneous determination of incarceration and crime.

To facilitate comparison with previous research, the last row of the table presents the implied crime-prison elasticities for each specification.[11] For violent crime, conversion of our IV estimates to elasticities yield effect sizes of -0.06 to 0-.11. For property crime, the corresponding elasticity estimates range from -0.15 to -0.21. Levitt (1996) reports violent crime-prison elasticities between -0.38 and -0.42 and property crime-prison elasticities of -0.26 to -0.32. Marvell and Moody (1994) find a total crime-prison elasticity of -0.16.[12] Thus, our results imply substantially smaller effects than those reported in Levitt and results comparable to those of

[11] We calculate elasticities in the following manner. We divide the coefficient estimate by the average crime rate for the entire sample. We then divide this ratio by one divided by the average incarceration rate. Thus, the elasticity can be interpreted as the average scale independent effect at the means of the sample.

[12] Marvell and Moody do not report separate elasticity estimates for overall violent and property crime. However, given the greater frequency of property crime, their overall effect is roughly consistent with the elasticity estimates that we find using our entire panel data set.

Marvell and Moody. These comparisons are misleading however, as our departure from Levitt's results and our accordance with those in Marvell and Moody are driven largely by the difference in time periods analyzed. While the results in Table 4 are based on a panel data set spanning 1978 to 2004, Levitt's analysis is based on panel data spanning the 1971 to 1993 while Marvell and Moody analyze the time period 1971 to 1989. As we show below, we find considerably larger effects when we restrict our sample to an earlier time period. Moreover, unlike Marvell and Moody we consistently find strong evidence that the crime-prison elasticities estimated by OLS are severely biased towards zero.

Table 5 presents comparable estimates for each of the seven individual felony offenses listed in Table 3. Again we report results from four separate models. Here however, we only report the coefficients on the change in incarceration rates to conserve space. With the exception of the assault rate models, instrumenting the change in incarceration rates with our predicted change in incarceration rates yields more negative effects of incarceration on crime, with most point estimates substantially larger and statistically distinguishable from the OLS results. None of the coefficients in the murder rate models are statistically significant, though all are negative. The IV results for rape suggest much larger effects (roughly six times) of changes in incarceration on the rape rate than those using the OLS specification. We find similar results for robbery, burglary, larceny, and auto theft, but no measurable effect for assault. Finally, incorporating state fixed effects in the model specification increases the magnitude of the point estimates in nearly all cases.

The results in Table 5 can be used to compare our estimation results to those from the pure incapacitation effect literature that attempts to gauge crimes avoided through retrospective inmate surveys. Recall, while the range of estimates in this literature spans from 10 crimes prevented per additional inmate to over 100 crimes, the most careful assessments of this research suggest a range of estimates between 10 and 20 incidents per year of prison served. Since both the dependent and independent variables used in the models in Tables 4 and 5 are expressed per 100,000, the coefficients on the change in the incarceration rate can be interpreted as the average effect of putting one more person in prison for a year. Thus summing the coefficients for the seven crime categories in Table 5 provides an estimate of the number of part 1 felony offenses prevented by putting one more person in prison.

One problem with this estimate concerns the fact that the UCR data are based on crimes reported to the police, and with the exception of murder, reporting rates are considerably lower than one for all crimes. However, with crime-specific data on reporting rates, one can easily inflate the point estimates by dividing by the proportion of incidents reported to the police.

Using the IV estimates in the final column of Table 5 and accounting for the under-reporting of most crimes,[13] we find that, on average, a one-person increase in the prison population prevents 0.005 murders, 0.1 rapes, .04 robberies, no assaults, 2.1 burglaries, 6.3 larcenies, and 0.6 motor vehicle thefts. In total, this constitutes 9.4 fewer part 1 felony offenses for each additional inmate. Thus, our model estimates using the entire sample are on the low end of the range of estimates from the pure incapacitation literature.[14]

[13] Rennison (2001) presents estimates from the National Criminal Victimization Survey for the years 1993 through 2000 on the proportion of offenses reported to the policy by crime victims. Averaging her eight annual estimates yield average reporting rates of 0.325 for rape, 0.572 for robbery, 0.553 for aggravated assault, 0.502 for burglary, 0.262 for larceny, and 0.788 for motor vehicle theft. In the calculations above, we use these reporting rates to inflate the marginal effects of incarceration. We assume that the police are aware of all murders.

[14] Note, the recent incapacitation research by Owens (2009) suggests even smaller effects (on the order of 2 to 3 crimes per year).

6. Estimation Results by Sub-Period

The estimates in the previous section constrain the average effect of incarceration on crime to be constant across all time periods and states. Given the substantial increase in U.S. incarceration rates since the mid 1970s, this specification choice is likely to be too restrictive. Assuming that the most criminally active are incarcerated first either through the deliberate targeting of resources by the criminal justice system or through the most active being caught first, one would expect that the marginal crime-abating effect of an additional inmate would be lower in later years (when the incarceration rate is higher) relative to earlier years.

In this section, we assess whether the crime-prison effects vary by time period. To do so, we stratify our panel into two sub-panels covering the periods 1978 to 1990 and 1991 to 2004, and estimate separate models for each period. We begin with an analysis of the strength of the first-stage relationship between our predicted change in incarceration and the actual change for these sub-periods. Table 6 presents the results from several first-stage models with regression specifications corresponding to those employed in Table 2. To conserve space, we only report the coefficient on the predicted change in incarceration rates. Specifications (3) and (4) correspond to the actual first-stage models that we will subsequently use in the crime-prison models. In all four specifications and for both time periods, the predicted change in incarceration has a positive and significant effect on the actual change in incarceration. We find a better first-stage fit for the latter time period relative to the former time period, suggesting greater temporary variation in the underlying determinants of incarceration in the early period. Moreover, the degree to which the prediction over-estimates the actual change is greater in the earlier period. Nonetheless, in specifications (3) and (4) the instrument is significant at the one-percent level of confidence in both models and for both time periods.

Table 7 presents OLS and IV estimates of the effect of changes in incarceration on changes in violent and property crime rates for each time period. The specifications correspond exactly to those used in Table 4, although here we only report the coefficients on incarceration. The table also reports the implied elasticities at the sample means. Beginning with the results for violent crime, in both periods the IV estimates are more negative and generally statistically significant relative to the OLS estimates (which are all near zero and statistically insignificant). However, the IV estimates for the earlier time period are considerably larger than those for the latter period. Including year effects for the 1978 to 1990 model, the results suggest that each additional prison year served prevents roughly ½ of a violent crime, while including state effects yields the much larger estimate of 2.5 violent crimes avoided for each year served. These point estimates correspond to violent crime-prison elasticities of -0.166 and -0.794. Regarding previous research, the violent crime-prison elasticity of -0.4 estimated in Levitt (1996) using a very similar time period is within the 95 percent confidence interval of both IV estimates presents in Table 7.

In contrast, the violent crime results for the latter time period are considerably more modest and significant only when state effects are included in the specification. In model (2), the IV estimates suggest that each prison year served prevents 0.3 violent crimes, which corresponds to an elasticity estimate of -0.2.

We observe very similar patterns for property crime. Again, IV estimates are generally much more negative than the OLS estimates, although for property crimes all of the OLS as well as the IV estimates are statistically significant at the one-percent level of confidence. For the earlier time period, the IV estimated effects suggest that each prison year served prevents between 4 and 11 property crimes, corresponding to elasticity estimates of -0.157 and -0.432.

Again, this range includes the elasticity estimates in Levitt (1996) (-0.26 to -0.32). The point estimates are considerably smaller for the latter period.

Table 8 presents corresponding crime-specific results. In nearly all cases, IV estimates are more negative than OLS estimates and the estimated effect sizes are larger for the earlier time period relative to the latter time period. To summarize these results, we again estimate the total number of crimes avoided by an additional prison year served, adjusted for crime-specific under-reporting rates in the UCR data. To do so, we make us of the estimation results in the final column of Table 8 that uses the most liberal specification of the IV model. The results indicate that for the period 1978 to 1990, each additional inmate prevented approximately 30 part 1 felony offenses. The comparable figure for the period from 1991 to 2004 is 8.3. Thus, the marginal crime-fighting effect of an additional inmate has indeed declined substantially in recent years.

7. Conclusion

There are several important findings and contributions of this study. First, to isolate exogenous variation in incarceration, we use the dynamic adjustment path of aggregate incarceration rates induced by shocks to underlying determinants of incarceration rates. Our theoretical prediction regarding subsequent one-year changes in incarceration rates provides a strong instrument for actual changes in incarceration rates. Moreover, the strategy permits us to estimate separate effects for the U.S. stratified by time period, as the instrument does not depend on variation in specific states or time periods.

Second, we find that breaking the simultaneity between incarceration and crime yields substantially larger estimates of the effects of incarceration on crime than those estimated in simple OLS models. When restricted to earlier time periods, our corrected estimates are in accord with those from Levitt (1996), who analyses a similar time period yet uses an entirely different identification strategy.

Third, we find that the effect of incarcerating one more inmate on crime rates has declined drastically over the last quarter century. When we split the sample into two equal time periods, we find crime-prison effects for the latter period that are less than one-third the size of those for the earlier period. For 1978 to 1990, we estimate that each additional prison year served prevented approximately 30 index crimes. For the period from 1991 to 2004, the comparable figure is eight. Moreover, this decline in level effects corresponds to substantial declines in crime-prison elasticities, suggesting that the constant-elasticity specification often used in previous research under-estimates the degree to which the crime-abating effects of incarceration decreases with scale.

This large decline in the marginal effect of an inmate suggests that the most recent increases in incarceration have been driven by the institutionalization of many inmates who, relative to previous periods, pose less of a threat to society. Indeed, given the much lower crime-abating effects for the most recent period, it is likely the case that for many recent inmates, the benefits to society in terms of crime reduction are unlikely to outweigh the explicit monetary costs of housing and maintaining an additional inmate. Moreover, once one accounts for the additional external costs of incarceration, such as the adverse effects on the families of inmates, the effects on victimizations behind bars, the effects on additional HIV/AIDS infections (Johnson and Raphael 2010), and the potential effects on the long-term employment prospects of former inmates, the benefit-cost ratio on the margin is likely to be substantially below one.

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Appendix A: Incorporating Behavioral Responses into the Model of Incarceration and Crime

The model presented in section 3 assumed that the underlying transition parameters in our aggregate model of crime and incarceration do not respond to one another, either instantaneously or with a dynamic lag. In this appendix, we sequentially relax some of the behavioral assumptions that we have made and assess how this impacts the interpretation of our results.

We first maintain the assumption that underlying criminality is unresponsive to either the threat of incarceration (captured by *p*) or the severity of punishment (captured by the release parameter θ). We begin by considering the case where the likelihood of being apprehended and incarcerated varies with the degree of underlying criminality –i.e. where *p=p(c)*. One might hypothesize a priori that *p* may either be increasing or decreasing in the degree of criminality. If policy makers respond to an increase in criminal behavior by greatly increasing resources devoted to policing and corrections, one would expect that increases in criminality would lead to increases in the incarceration risk. On the other hand, to the extent that increases in criminality dilute enforcement resources, dp/dc may be negative.

With regards to our identification strategy, such a policy reaction in isolation should not compromise the exogeneity of our proposed instrument, although the timing of this response may impact the strength of our first stage relationship. To see this, first consider the case where *p* responds instantaneously to changes in *c*. For dp/dc>0, an increase in criminality will cause an instantaneous increase in the incarceration risk. The higher p will translate into a lower initial crime spike, a larger initial increase in incarceration, and a speedier adjustment of incarceration and crime to their new equilibrium levels. The opposite would apply when *p* decreases in *c*. In either case, the change in incarceration subsequent to the initial change still provides exogenous variation in incarceration and thus, the IV strategy outlined above still identifies a causal incapacitation effect.

However, the incarceration risk parameter is unlikely to respond instantaneously, since policy makers are only likely to learn of an increase in *c* with time and since the budgetary process impedes instantaneous reaction to new problems. Such a delayed response will impact the first stage relationship between the predicted change in incarceration in equation (10) and actual changes. To see this, suppose that at t=0, the incarceration risk is p_0 but increases to p_1 at the beginning of period 1 (fully one period after the increase in criminality modeled above). Given that the incarceration risk remained constant for the first period following the shock, the initial change in the incarceration rate remains equal to that described in the first line of equation (10). However, the new increase in incarceration between periods 1 and 2 becomes

(A.1) $$\Delta S_{2,1} = \Delta S_{2,1|constant\ p} + (S^*_{2,2} - S^*_{2,1})(c_1 p_0 + \theta) + c_1(p_1 - p_0)(S^*_{2,2} - S_{2,1}),$$

where the first term is the predicted change in incarceration holding the incarceration risk constant (the second line in equation (10) and our proposed instrument), $S^*_{2,2} = \dfrac{c_1 p_1}{c_1 p_1 + \theta}$ is the new long run equilibrium incarceration rate with the elevated incarceration risk, $S^*_{2,1} = \dfrac{c_1 p_0}{c_1 p_0 + \theta}$ is the equilibrium incarceration rate given last periods parameters, and $S_{2,1}$ is the actual incarceration rate in period 1. The second term in equation (A.1) adjusts our previous estimate of the increase in incarceration for this period for the change in the long-term equilibrium rate, while the third terms reflects the instantaneous effect of the increase in the incarceration risk parameter *p*. When *p* is increasing in *c*, both terms are positive. Thus, our proposed instrument will systematically underestimate the actual change in incarceration. If *p* is a decreasing function of *c* than the opposite would hold. Regardless, our proposed instrument is still a component of the change in incarceration and thus a first stage relationship should exist. Moreover, as the criminality parameter is assumed constant across periods, this particular behavioral response will not induce correlation between the instrument and the second stage error term.

The impact of a change in sentence length on our instrument will require a dynamics analysis since a change in sentence length today will not impact changes in incarceration rates until today's cohort of admitted inmates reaches their counterfactual release dates under the prior sentencing regime. In practice, inmates sentenced to state or federal prison receive sentences of at least one year, although those being sent back for parole violation often serve terms that may fall short of a full year (Raphael and Weiman 2006). Here, we work through the effect of an increase in sentence length in response to an increase in criminality, operating under the assumption that an increase in *c* at time t=0 does not impact release rates until t=1.

If policy makers increase sentence severity in response to an increase in criminality, this implies that period release rates decrease in the once lagged value of c, or $\theta_t'(c_{t-1})<0$. Again, since θ does not change instantaneously in response to a change in the criminality parameter, the period one change in incarceration rates will not differ from that in the fist line of equation (10). The change in incarceration between periods 1 and 2, however, will differ by an identifiable quantity. Specifically, assuming that the release probability increases from θ_0 at t=0 to θ_1 at t=1, the implied change in the incarceration rate between periods 1 and 2 will be

(A.2)
$$\Delta S_{2,1} = \Delta S_{2,1|cons\tan t\ \theta} + (\theta_0 - \theta_1)S_{2,1}.$$

The first term on the right hand side of (A.2) is our previous estimate of the period change in incarceration rates assuming no responsiveness of θ to changes in *c*, while the second term reflects an additional component associated with the lower release rate at t=1 (which will be positive for $\theta_1<\theta_0$). Thus, allowing sentence severity to respond punitively to increases in criminality suggests that our proposed instrument will under-estimate the increase in incarceration. Nonetheless, the instrument again predicts a component of the change and thus should be able to identify significant variation in the future change in incarceration rates. Moreover, we are holding the criminality parameter constant by assumption, and thus instrument will not be correlated with the error term in the second stage equation.

Allowing criminality and corrections policy to respond reciprocally complicates the analysis somewhat and does indeed bias the second stage estimate of our structural estimate of the effect of incarceration on crime, if we interpret this estimate narrowly as a pure incapacitation effect. However, if we broaden the interpretation of the effect we are estimating to allow for a general deterrence effect in addition to an incapacitation effect, our instrument is still valid. To see this, we begin with a simple model whereby criminality and enforcement adjust instantaneously to one another. We then think through a likely dynamic structure of this response and how it will impact our estimation strategy.

Suppose that criminality is determined by two factors: a variable *x* measuring criminogenic influences that are unresponsive to policy, and the overall incarceration risk *p*. Assume further that criminality is determined according to the additively separable function *c* =

$h(x) + f(p)$, where $h'(x) > 0$ and $f'(p) < 0$. We also assume that the incarceration risk is a monotonically increasing function of the criminality parameter – i.e., $p = g(c)$ where $g'(c) > 0$. The assumption that p is increasing in c best describes the recent history of corrections policy in the United States.[15] The equilibrium incarceration risk is defined by the condition

(A.3)
$$h(x) + f(p) = g^{-1}(p)$$

where $g^{-1}(p)$ is the inverse of the function $g(c)$. Totally differentiating (A.3) with respect to x as well as the equation for c gives the response of p and c in response to a change in x:

(A.4)
$$\frac{dp}{dx} = \frac{h'(x)}{g^{-1}{}'(x) - f'(p)}$$
$$\frac{dc}{dx} = h'(x)\left[1 + \frac{f'(p)}{g^{-1}{}'(p) - f'(p)}\right].$$

For $g'(c) > 0$, both derivatives in (A.4) are positive. Thus a positive shock to underlying crime fundamentals leads to an equilibrium increase in both criminality as well as the incarceration risk.

The implications of this bi-directional responsiveness for our identification strategy will again depend on whether these adjustments occur instantaneously or over time. If a shock to c causes an instantaneous adjustment of the incarceration risk and criminality to new equilibrium levels, then our identification strategy is not impacted. Future increases in incarceration will be based on stable behavioral and policy parameters and our proposed instrument will identify exogenous variation in future changes in incarceration rates.

However, the reaction of policy to a change in criminality is likely to be slower than the reaction of criminal behavior to changes in enforcement, since changes in policy will be slowed down to some degree by the timing of the budgetary process. Moreover, if policy makers choose an optimal enforcement level based on their best estimate of current criminality, equilibrium values of p and c may not be reached only after a multi-period adjustment process. Here, we consider the implications for our identification strategy of the specialized case where c responds instantaneously to changes in p but where p responds to changes in c with a one period lag. Since our chosen instrument will be used to predict changes in incarceration between periods t=1 and t=2, we'll only consider the effect of this reciprocal responsiveness for the first two changes in crime and incarceration following a shock to c.

Assume that at t=0, criminality increases from c_0 to c_1. Given the lagged responsiveness of the incarceration risk, p will not increase until period 1. At that time, the parameter increases from p_0 to p_1. Since criminality responds instantaneously, the criminality parameter will decrease from c_1 to c_2, as suggested by the simple model in equations (A.3) and (A.4).[16] For this example, the first two changes in observed crime rates are given by the equations

[15] The time period with perhaps the strongest evidence in favor of an overall increase in criminality occurred during the later 1980s and early 1990s (the time period corresponding to the crack cocaine epidemic). During this time period, prison admission rates increased steeply relative to previous levels and sentences were enhanced at the federal level and in many states for non-violent drug crimes and for other felony offenses. For detailed discussions of sentencing policy in the U.S. see Tonry (1997) and Jacobson (2006).

[16] To be sure, c_2 will not be an equilibrium value for criminality but the optimal behavioral response to the incarceration risk associated with p_1. It will take several additional policy iterations and reciprocal behavioral

(A.5)
$$\Delta C_0 = -c_1 \Delta S_{2,0} + (c_1 - c_0)(1 - S^*{}_{2,0})$$
$$\Delta C_1 = -c_2 \Delta S_{2,1} + (c_2 - c_1)(1 - S_{2,1}).$$

Note, the crime change between periods 0 and 1 is equivalent to the crime change reported in equation (11) (section 3 of the paper) where we assumed away behavioral responses of the parameters. The change in crime between periods 1 and 2 however has changed, with an alternative coefficient on the change in incarceration ($-c_2$ instead of $-c_1$) and an additional term capturing the effect on crime of the decrease in criminality from c_1 to c_2. This second term is basically the general deterrence effect of an increase in the incarceration risk.

Since we cannot observe the actual values for the criminality parameter, this general deterrence component will be swept into the error term in the second stage crime equation (given by the second line in equation (A,5)). Since the decreases in criminality in period 1 is driven by the response of policy to the increases in criminality in period 0, the change in criminality between periods 0 and 1 will be negatively correlated with the change in criminality between periods 1 and 2. Our proposed instrument is increasing in the initial increase in criminality. Thus, our instrument will be negatively correlated with the deterrence effect contained in the error term of the second stage regression, causing a negative bias to our estimate of the incapacitation effect of prison (i.e., our elasticity estimates will be too large).

Despite this change, however, we are still able to interpret the structural estimate from the second stage as a causal effect of corrections policy on crime, although we must change our interpretation somewhat. With a lagged reciprocal response, the second stage estimate of the coefficient on the change in incarceration will reflect both the incapacitation effect as well as the general deterrence effect of prison on crime (through the correlation between the predicted incarceration change and the general deterrence component in the error term). Thus, while we are unable to disentangle the separate avenues by which prison is likely to impact criminality activity, our proposed IV estimate does permit estimation of a cumulative impact.

With regards to our first stage prediction, the predicted increase in incarceration with stable parameters may either over or under-estimate the actual increase. With the stated sequence of criminality and incarceration risk values listed above, the actual increase in incarceration between periods 1 and 2 will be given by the equation

(A.6)
$$\Delta S_{2,1} = \Delta S_{2,1|constant\ p,c} + (S_2^* - S_1^*)(c_1 p_0 + \theta) + (S_2^* - S_1)(c_2 p_1 - c_1 p_0),$$

where the first term is the predicted change in incarceration between periods 1 and 2 assuming stable parameters following the initial crime shock, $S_2^* = \dfrac{c_2 p_1}{c_2 p_1 + \theta}$ is the new equilibrium incarceration rate after the period 1 response of criminality to the change in the incarceration risk, $S_1^* = \dfrac{c_1 p_0}{c_1 p_0 + \theta}$ is the equilibrium rate after the initial criminality shock but preceding the incarceration risk response, and S_1 is the actual incarceration rate at the beginning of period 1. The second an third terms in this model with both be either positive or negative depending on the

responses to reach steady state values for the parameters. However, since we are primarily interested in instrumenting the change in incarceration between periods 1 and 2, we do not need to consider subsequent changes.

relative size of c_2p_1 and c_1p_0. If $c_2p_1 > c_1p_0$, the increase in the incarceration risk is so large relative to the subsequent decrease in criminality that the equilibrium incarceration rate actually increases. In this instance, the instrument will under-estimate the actual increase in incarceration. On the other hand, if $c_2p_1 < c_1p_0$, then the decline in criminality is sufficiently large relative to the increase in the incarceration risk that the equilibrium rate declines and both of the second terms in (A.6) are negative. In this instance, our non-behavioral prediction will over-estimate the actual change in incarceration.

There is one potential behavioral response on the part of the potentially criminal that may bias our results, even when interpreted to represent a total effect of incarceration on crime. Suppose that current increases in criminal behavior cause subsequent decreases in criminality due to a revulsion on the part of the general public to the consequences of the crime spike. Such responses suggest that changes in the criminality parameter may exhibit negative serial correlation. An example of such a revulsion effect may be a drug epidemic that runs it course where a younger generation is reluctant to use a drug which devastated the lives of the generation before them. This type of serial correlation would create a negative correlation between our instrument and the corresponding change in crime for reasons similar to those laid out in the discussion of equation (A.6). Here however, the decline in crime cannot be attributed to a general deterrence effect of prison, since the decline would occur regardless of a change in the chance of punishment. In this case, there would be a spurious reduced-form relationship between the instrument and the change in the crime rate.

While such changes in behavior are certainly possible, that spikes in criminal behavior driven by the introduction of a new drug or other variants of crime shocks would correct themselves within a year or two is unlikely and contrary to recent history. Nonetheless, we acknowledge this threat to the validity of our estimation strategy.

Figure 1: Dynamics Adjustment Path of Incarceration and Crime in Response to a Permanent Increase in the Criminality Parameter

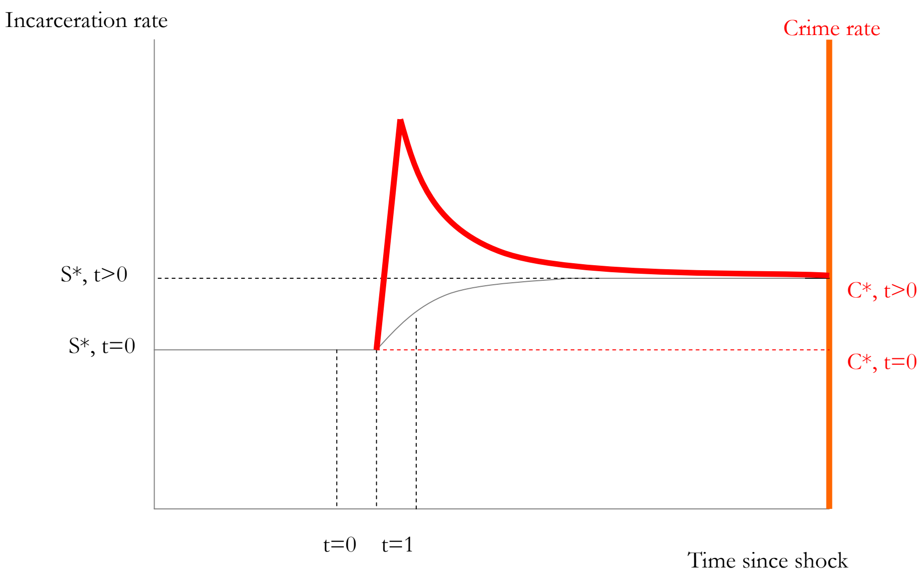

Figure 2: Scatter Plot of Actual Annual Changes in State-Level Incarceration Rates Against the Predicted Changes

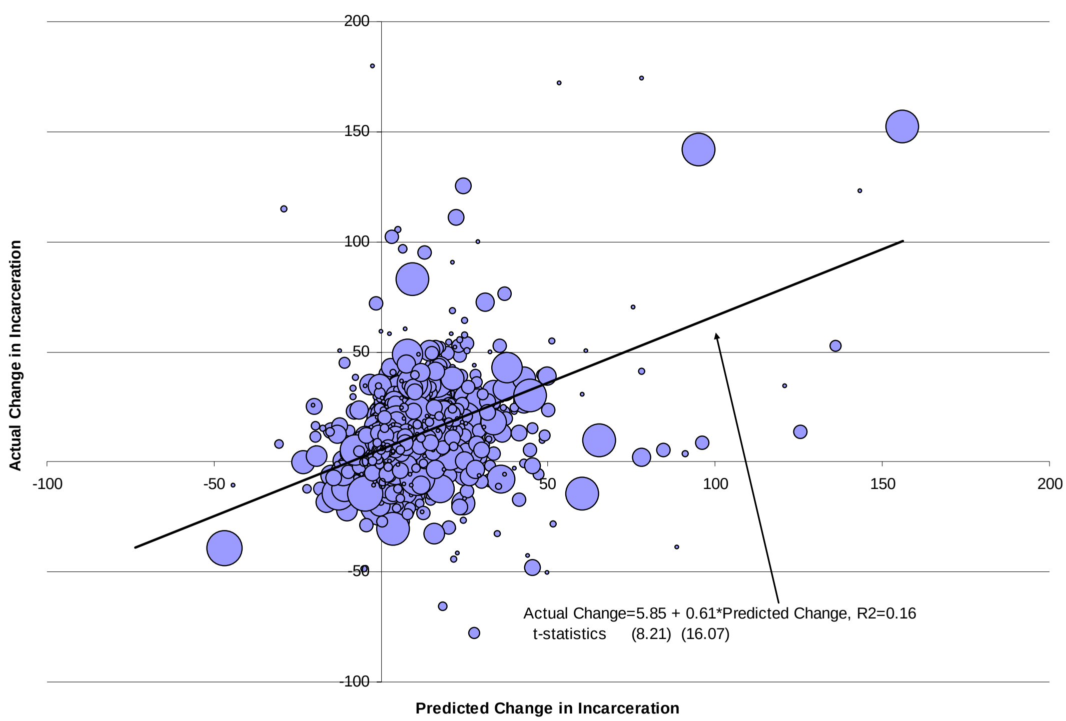

Table 1
Illustration of the Calculation of the Predicted Change in Incarceration Rates for New York Between 1980 and 1982

	1979	1980	1981	1982
Current incarceration rate ($S_{2,t}$)	118.39	125.33	147.30	161.39
Admission rate (cp)	-	0.00059	0.00071	0.00072
Release rate (θ)	-	0.432	0.329	0.360
Equilibrium Incarceration rate based on current transition probabilities ($S^*_{2,t>0} = \frac{cp}{cp+\theta}$ *100,000)	-	135.87	215.61	199.97
Incarceration rate at t = 0 ($S_{2,0}$)	-	118.39	125.33	147.30
Predicted change in incarceration rate, t=1 to t=2 $(S^*_{2,t>0} - S_{2,0}) * (1-cp-\theta)(cp+\theta)$	-	4.29	19.94	12.15
Actual change in incarceration rate, t=1 to t=2	-	21.97	14.09	13.64

Table 2
First Stage Effect of the Predicted Change in Incarceration Rates Based on Last Period Shock on the Current Change in Incarceration Rates

	Dependent Variable=ΔIncarceration Rate			
	(1)	(2)	(3)	(4)
Predicted Δ	0.605	0.603	0.549	0.513
Incarceration	(0.037)	(0.037)	(0.037)	(0.040)
Δ% in	-	-2.415	-0.684	-2.236
popul. 0 to 17		(3.851)	(3.857)	(4.078)
Δ% in	-	-3.871	0.393	-1.109
popul. 18 to 24		(4.695)	(5.047)	(5.328)
Δ% in	-	-2.602	-1.074	-1.968
popul. 25 to 44		(4.718)	(5.362)	(5.621)
Δ% in	-	-3.619	1.752	2.386
popul. 45 to 64		(4.872)	(5.142)	(5.336)
Δunemployment	-	-1.595	-1.430	-1.378
rate		(0.655)	(0.933)	(0.946)
Δpoverty rate	-	-0.943	0.078	0.091
		(0.407)	(0.411)	(0.417)
Δ% black	-	0.819	0.736	0.793
		(0.471)	(0.446)	(0.451)
Δ per capita	-	-0.008	-0.005	-0.004
income		(0.001)	(0.002)	(0.002)
Year Effects	No	No	Yes	Yes
State Effects	No	No	No	Yes
R^2	0.164	0.190	0.312	0.328
N	1,321	1,321	1,321	1,321
F-statistic*	258.35	256.44	213.20	161.21
(P-value)	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)

Standard errors are in parentheses. All models include a constant terms and are weighted by the state-year populations.
*F-test from a test of the significance of the instrumental variable.

Table 3
Descriptive Statistics for Crime and Incarceration Rates for the Overall Sample Period and Sub-Periods

	Average	Standard Deviation	Within-State Standard Deviation
Panel A: 1978 to 2004			
Violent Crime	596.35	256.50	124.70
Murder	7.85	4.05	2.34
Rape	36.03	11.69	6.63
Robbery	205.52	127.34	66.32
Assault	346.95	151.49	74.10
Property Crime	4,503.20	1,189.87	817.27
Burglary	1,120.32	438.50	356.33
Larceny	2,875.62	701.85	431.54
Motor Veh. Theft	507.27	223.40	138.55
Incarceration Rate	302.62	163.70	132.10
Panel B: 1978 to 1990			
Violent Crime	594.19	264.07	76.26
Murder	8.85	3.99	1.54
Rape	36.33	12.24	5.01
Robbery	226.38	145.17	36.07
Assault	322.63	138.59	58.28
Property Crime	4,929.62	1,191.04	454.14
Burglary	1,392.10	423.60	217.76
Larceny	3,024.91	707.41	260.95
Motor Veh. Theft	512.62	233.75	103.13
Incarceration Rate	186.38	87.07	56.61
Panel C: 1991 to 2004			
Violent Crime	598.09	250.23	137.96
Murder	7.06	3.91	2.10
Rape	35.77	11.24	5.36
Robbery	188.71	108.04	67.44
Assault	366.53	158.43	72.95
Property Crime	4,160.04	1,072.13	679.24
Burglary	901.59	308.77	205.33
Larceny	2,755.48	67373	377.09
Motor Veh. Theft	502.96	214.61	131.14
Incarceration Rate	396.20	150.44	78.74

All figures are rates per 100,000 state residents and are weighted by the state-year population.

Table 4
OLS and IV Estimates of the Effect of Changes in Incarceration Rates on Changes in Overall Violent and Property Crime Rates Using the Entire State-Level Panel

	Dependent Variable=ΔViolent Crime Rate				Dependent Variable=ΔProperty Crime Rate			
	Specification (1)		Specification (2)		Specification (1)		Specification (2)	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
ΔIncarceration rate	0.048	-0.116	0.038	-0.230	-0.994	-2.315	-1.109	-3.272
	(0.044)	(0.118)	(0.045)	(0.136)	(0.233)	(0.625)	(0.237)	(0.721)
Δ% in popul. 0 to 17	-5.704	-5.753	0.195	-0.036	41.477	41.084	89.019	87.156
	(6.616)	(6.652)	(6.956)	(7.053)	(34.817)	(35.249)	(36.097)	(37.300)
Δ% in popul. 18 to 24	-5.281	-4.649	2.450	3.272	7.121	12.184	65.427	72.079
	(8.650)	(8.706)	(9.076)	(9.209)	(45.518)	(46.137)	(47.096)	(48.704)
Δ% in popul. 25 to 44	-6.889	-6.593	-3.207	-2.702	99.559	101.929	128.378	132.459
	(9.191)	(9.242)	(9.578)	(9.714)	(48.366)	(48.978)	(49.707)	(51.373)
Δ% in popul. 45 to 64	-2.115	-1.028	-5.662	-3.820	105.093	113.797	82.560	97.458
	(8.806)	(8.883)	(9.091)	(9.258)	(46.339)	(47.069)	(47.176)	(48.965)
Δunemployment rate	-1.776	-2.028	-1.813	-2.183	27.294	25.269	27.850	24.855
	(1.601)	(1.619)	(1.615)	(1.647)	(8.428)	(8.579)	(8.382)	(8.711)
Δpoverty rate	-0.955	-0.938	-0.774	-0.735	4.522	4.659	6.019	6.329
	(0.706)	(0.710)	(0.711)	(0.721)	(3.716)	(3.762)	(3.691)	(3.814)
Δ% black	0.339	0.494	0.382	0.643	3.810	4.050	3.874	5.988
	(0.766)	(0.777)	(0.770)	(0.791)	(4.033)	(4.119)	(3.999)	(4.184)
Δ per capita income	-0.001	-0.002	0.003	0.002	-0.065	-0.073	-0.024	-0.032
	(0.003)	(0.003)	(0.003)	(0.004)	(0.017)	(0.018)	(0.019)	(0.019)
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.471	0.468	0.487	0.481	0.503	0.496	0.532	0.516
N	1,321	1,321	1,321	1,321	1,321	1,321	1,321	1,321
Implied elasticity at the mean	0.023	-0.057	0.019	-0.113	-0.064	-0.151	-0.072	-0.213

Standard errors are in parentheses. All models include a constant term and are weighted by state level populations.

Table 5
OLS and IV Estimates of the Effect of Changes in Incarceration Rates on Changes on Individual Crimes Using the Entire State-Level Panel

	Specification (1)		Specification (2)	
Dependent Variable	OLS	IV	OLS	IV
ΔMurder	-0.002	-0.005	-0.001	-0.005
	(0.001)	(0.003)	(0.001)	(0.004)
ΔRape	-0.005	-0.029	-0.005	-0.033
	(0.003)	(0.009)	(0.004)	(0.011)
ΔRobbery	-0.025	-0.165	-0.028	-0.227
	(0.025)	(0.066)	(0.025)	(0.077)
ΔAssault	0.079	0.082	0.072	0.035
	(0.030)	(0.080)	(0.031)	(0.092)
ΔBurlgary	-0.398	-0.857	-0.414	-1.064
	(0.080)	(0.216)	(0.082)	(0.248)
ΔLarceny	-0.498	-1.182	-0.573	-1.720
	(0.146)	(0.392)	(0.149)	(0.449)
ΔMotor Vehicle	-0.097	-0.275	-0.122	-0.487
Theft	(0.055)	(0.146)	(0.056)	(0.167)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes

Standard errors are in parentheses. The figures in the table all represent the coefficient on the change in incarceration rates from either the OLS or IV model for the corresponding dependent variable. All models include a constant terms and the control variables listed in the specifications in Table 4. All models are also weighted by state-level populations.

Table 6
First Stage Effect of the Predicted Change in Incarceration Rates Based on Last Period Shock on the Current Change in Incarceration Rates by Sub-Period

	Dependent Variable=ΔIncarceration Rate			
	(1)	(2)	(3)	(4)
Time Period: 1978 – 1990				
Predicted Δ	0.545	0.513	0.424	0.182
Incarceration	(0.049)	(0.049)	(0.051)	(0.057)
F-statistic*	120.47	106.74	69.32	10.26
(P-value)	(<0.0001)	(<0.0001)	(<0.0001)	(0.0014)
Time Period: 1991-2004				
Predicted Δ	0.625	0.594	0.564	0.476
Incarceration	(0.054)	(0.054)	(0.054)	(0.062)
F-statistic*	132.67	120.91	109.16	59.80
(P-value)	(<0.0001)	(<0.0001)	(<0.0001)	(<0.0001)
Controls Variables	No	Yes	Yes	Yes
Year Effects	No	No	Yes	Yes
State Effects	No	No	No	Yes

Standard errors are in parentheses. All models are weighted by the state level population.
*F-test of the significance of the instrument in the first stage regression.

Table 7
OLS and IV Estimates of the Effect of Changes in Incarceration Rates on Changes in Violent and Property Crime Rates by Sub Period

	Dependent Variable = Δ Violent Crime Rate			
	Specification (1)		Specification (2)	
	OLS	IV	OLS	IV
Marginal effect				
78 to 90	0.135	-0.529	0.028	-2.534
	(0.112)	(0.368)	(0.127)	(1.264)
91 to 04	0.022	-0.076	-0.024	-0.321
	(0.049)	(0.127)	(0.047)	(0.160)
Implied Elasticity				
78 to 90	0.042	-0.166	0.009	-0.794
91 to 04	0.014	-0.048	-0.015	-0.206
	Dependent Variable = Δ Property Crime Rate			
	Specification (1)		Specification (2)	
	OLS	IV	OLS	IV
Marginal effect				
78 to 90	-2.192	-4.163	-2.422	-11.414
	(0.585)	(1.892)	(0.666)	(5.834)
91 to 04	-0.666	-1.832	-0.799	-2.693
	(0.259)	(0.688)	(0.265)	(0.909)
Implied Elasticity				
78 to 90	-0.083	-0.157	-0.092	-0.432
91 to 04	-0.062	-0.170	-0.074	-0.250
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes

Standard errors are in parentheses. The figures in the table all represent the coefficient on the change in incarceration rates from either the OLS or IV model for the corresponding dependent variable. All models include a constant terms and the control variables listed in the specifications in Table 4. All models are also weighted by state-level populations.

Table 8
OLS and IV Estimates of the Effect of Changes in Incarceration Rates on Changes in Individual Part 1 Felony Offenses by Sub Period

	Specification (1)		Specification (2)	
Dependent Variable	OLS	IV	OLS	IV
Murder				
78 to 90	0.006 (0.003)	-0.002 (0.010)	0.002 (0.004)	-0.038 (0.030)
91 to 04	-0.003 (0.001)	-0.006 (0.003)	-0.003 (0.001)	-0.006 (0.004)
Rape				
78 to 90	-0.011 (0.008)	-0.075 (0.027)	-0.005 (0.009)	-0.202 (0.094)
91 to 04	-0.002 (0.004)	-0.019 (0.010)	-0.002 (0.004)	-0.021 (0.014)
Robbery				
78 to 90	-0.141 (0.070)	-0.793 (0.239)	-0.216 (0.081)	-2.555 (0.961)
91 to 04	-0.010 (0.024)	-0.086 (0.062)	-0.043 (0.021)	-0.257 (0.075)
Assault				
78 to 90	0.281 (0.072)	0.341 (0.232)	0.248 (0.081)	0.262 (0.625)
91 to 04	0.037 (0.034)	0.036 (0.091)	0.023 (0.036)	-0.037 (0.119)
Burglary				
78 to 90	-1.031 (0.239)	-2.731 (0.796)	-1.078 (0.271)	-6.769 (2.743)
91 to 04	-0.224 (0.069)	-0.474 (0.183)	-0.232 (0.070)	-0.514 (0.234)
Larceny				
78 to 90	-1.076 (0.362)	-1.249 (1.159)	-1.081 (0.417)	-2.627 (3.230)
91 to 04	-0.328 (0.165)	-1.027 (0.437)	-0.445 (0.171)	-1.674 (0.586)
Motor Vehicle Theft				
78 to 90	-0.085 (0.122)	-0.183 (0.393)	-0.263 (0.136)	-2.018 (1.178)
91 to 04	-0.114 (0.066)	-0.331 (0.176)	-0.123 (0.066)	-0.505 (0.223)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes

Standard errors are in parentheses. The figures in the table all represent the coefficient on the change in incarceration rates from either the OLS or IV model for the corresponding dependent variable. All models include a constant terms and the control variables listed in the specifications in Table 4. All models are also weighted by state-level populations.