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General Equilibrium Effects of Prison on Crime: Evidence from International Comparisons

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ABSTRACT

We compare crime and incarceration rates over time for the United States, Canada, and England and Wales, as well as for a small selection of comparison countries. Shifts in U.S. punishment policy led to a five-fold increase in the incarceration rate, while nearly every other country experienced only minor increases in incarceration. The large shifts in U.S. punishment policy do not seem to have caused commensurately large improvements in public safety.

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1. INTRODUCTION

From 1920 through 1970, the rate of incarceration in the United States was roughly constant, hovering around 100 per 100,000. Today, the incarceration rate is five times that level. The incarceration rate in the United States is thus markedly higher today than it was historically.

The incarceration rate in the United States is also markedly higher today than it is in other countries. According to the International Centre for Prison Studies of the University of Essex, in 2008 the United States accounted for 5 percent of world population but 23 percent of worldwide prisoners (Walmsley 2009).

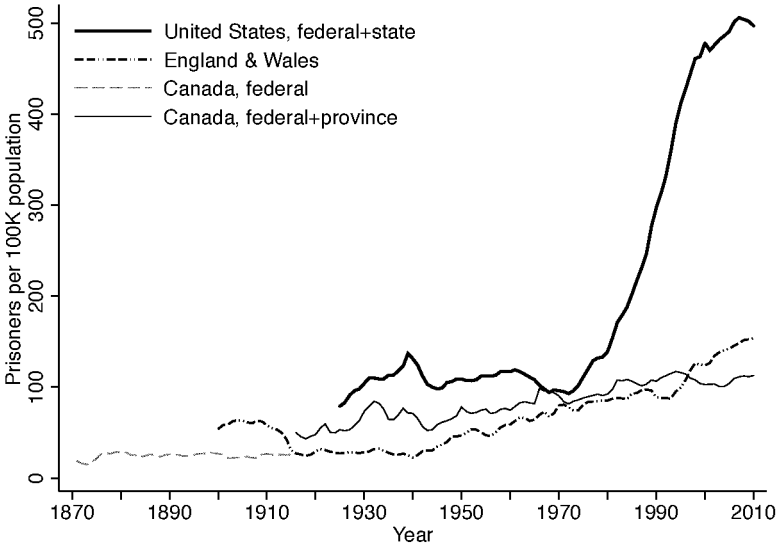
Figure 1 displays the time series of the incarceration rate for the United States as compared with that of other countries. Panel A compares the United States to Canada and England and Wales (combined) over the last century. These countries have perhaps the longest tradition of collecting data on incarceration rates and are additionally relatively comparable to one another in terms of language, economy, law, and culture. The figure indicates that already during the early part of the 20th century, the United States had higher incarceration rates than Canada and England and Wales. From 1925 through 1970, however, those countries essentially caught up to the United States. But starting in 1970, the United States made substantial investments in prison capacity, and by 2010 the U.S. incarceration rate was 3.3 times that of England and Wales and 4.4 times that of Canada. These conclusions are particularly stark; compared to other countries that are members of the Organization for Economic Cooperation and Development, England and Wales have a relatively high incarceration rate.

Panel B compares the United States to selected OECD countries over the last four decades.[1] The figure indicates that the U.S. increase

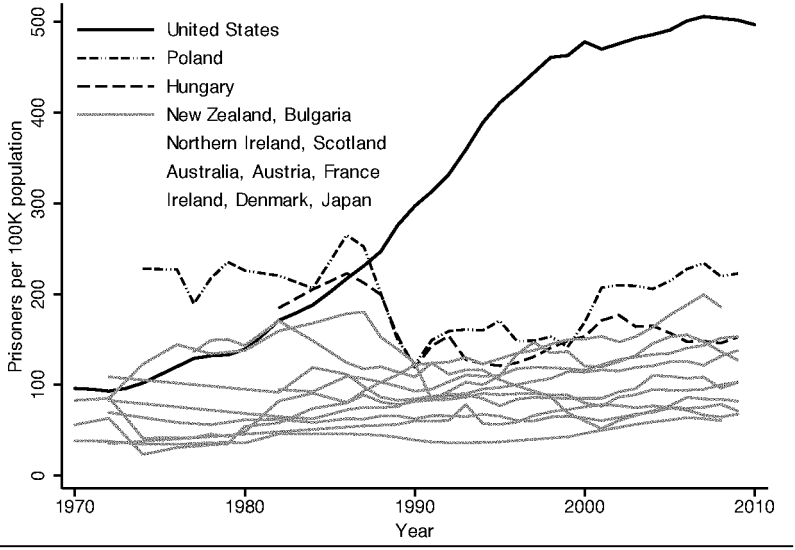
[1] Throughout this paper, countries were selected on grounds of data availability and quality.

Figure 1
Incarceration Rates in Perspective

A. U.S., England & Wales, and Canada: 1870 to present



B. Selected Rich Countries: 1970 to present



Source: See text, pp. 170–71, 173.

in incarceration is surprising compared to Canada and England and Wales, as well as to a broader set of countries.

In sum, from a historical and comparative perspective, the expanded use of prisons in the United States in recent decades is breathtaking. However, while the punitiveness of the current U.S. system is unusual, some people may be willing to set aside the obvious liberty concerns if they are persuaded that prison is sufficiently effective at providing for the safety of those not imprisoned. Scholars and policymakers alike note that a large prison system could reduce crime through two important channels: deterrence and incapacitation. Assessing the magnitude of these channels is an important task for research and one that is taken up in an extensive academic literature.

However, a general equilibrium policy evaluation of the increased use of imprisonment must take account of additional possible mechanisms. One such mechanism is the so-called prison reentry problem, which has been much discussed in the popular press recently and in the academic literature. Nationally, roughly 700,000 people will be released from prison (long-term incarceration) this year, and roughly 7 million people will be released from jail (short-term incarceration). It is conceivable that those released will be changed by virtue of the experience of incarceration. Such changes could be protective against crime if, for example, former prisoners decided to "go straight" to avoid any subsequent confinement. More concerning is the possibility that the changes could encourage crime if, for example, former prisoners found themselves unable to obtain legitimate work and were thereby encouraged to engage in crime, or if they were scarred by the experience and unable to cope with life on the outside.

A second such mechanism is the replacement hypothesis (Freeman 1999). In Freeman's view, criminal opportunities are limited and rivalrous—if one person is taking advantage of the opportunity, another cannot take advantage of it simultaneously—and the group of potential offenders is large relative to the number of criminal opportunities. Accordingly, if this mechanism is important, incapacitation could be entirely offset by replacement. In simple terms, one corner drug dealer is sent to prison, and another steps forward to take his place.

A third mechanism is the effect of the scope of imprisonment on deterrence via externality. Typically, deterrence is framed as an individual's decreased inclination toward crime because of a higher threatened sanction. However, the stigma associated with a criminal

record may be an important deterrent as well, for example in the labor market or in social interactions. Stigma means that in the extreme, higher threatened sanctions can be counterproductive (Rasmusen 1996). In simple terms, when punishment is rare, a punished person is more likely to be a bad seed than when punishment is prevalent.

The research designs used in the literature focus on measurement of deterrence and incapacitation and are unable to capture these broader general equilibrium phenomena. In the literature, general equilibrium policy evaluation has primarily been done in the context of formal structural modeling of the potential offenders' economic and legal environment (see, for example, Burdett, Lagos, and Wright 2004). This approach has many merits, including the clear explication of mechanisms and a natural methodology for evaluating counterfactual policy experiments.

In this paper, we complement the theoretical literature with an empirical assessment of the general equilibrium effects of mass incarceration. Our approach is rooted in the observation that the magnitude of the expansion in the prison population in the United States over the last 40 years has been nearly unique internationally. Our conclusions are informed by a new data set on the use of imprisonment and the extent of crime for a large group of countries over many years. We pay particularly close attention to Canada and to England and Wales, as these are natural comparisons for the United States, and the governments of those countries have a tradition of collecting the relevant data.

The plan for the paper is as follows: Section 2 describes the data we use. Section 3 focuses on a comparative analysis of trends in the United States, Canada, and England and Wales. Section 4 introduces some simple panel data regressions to summarize the results. Section 5 concludes.

2. DATA

Our first analysis compares the United States to Canada. Data on crime in Canada are taken from the *Statistics Canada* website, www.statcan.gc.ca. Data on prisoners in Canada are taken from the *Statistics Canada* website for 1978 to the present. Historical data on prisoners were obtained from Tables Z173–174 (federal prisoners) and Tables Z198–208 (provincial prisoners) of *Historical Statistics of Canada* (2nd edition). Data on U.S. crime are taken from the Federal

Bureau of Investigation's *Uniform Crime Reports*. Data on U.S. prisoners are taken from the University at Albany's *Sourcebook of Criminal Justice Statistics*.

Our second analysis compares the United States to England and Wales. Data on crime for the latter are taken from two electronic files produced by the Home Office, "Recorded Crime Statistics 1898–2001/2" and "Recorded Crime Statistics 2002/3–2009/10." Data on prisoners are taken from Table 7.5 of "Offender Management Caseload Statistics 2009."

Our final analysis uses data from the *Surveys of Crime Trends and Operations of Criminal Justice Systems*. These data were collected by the Crime Prevention and Criminal Justice Division of the United Nations ("UN data") in 10 separate waves. The data collection for the first wave was conducted in 1978 and pertained to aspects of crime and the criminal justice system for the years 1970–75. Subsequent waves were collected roughly every five years; the most recent information from the survey pertains to 2006. All of the statistics reported in the survey are collected from statistical reports from the respondent countries. We have hand-checked these data using Eurostat data, which are available after 1987. We have observed some minor discrepancies between the values in the survey and those in the Eurostat data, but these seem to emerge from definitional differences.

Perhaps oddly, a counterexample is the UN data set for the United States. Fortunately, high-quality data for the United States are available from several other sources, and we have replaced the U.S. values in the UN data with information from the *Sourcebook*. For other countries, our sense is that the main measurement problem in the survey emerges from nonresponse rather than incorrect values.

3. COMPARISON WITH CANADA AND ENGLAND AND WALES

Previous research has noted that, despite substantial similarity between the two countries on many dimensions, Canada does not imprison its citizens at nearly the rate the United States does (Doob and Webster 2006). Figure 2A displays total incarceration rates per 100,000 using publicly available data for Canada and the United States. The figure indicates that Canada did not increase its use of prisons over the

Figure 2
Imprisonment and Crime: United States and Canada

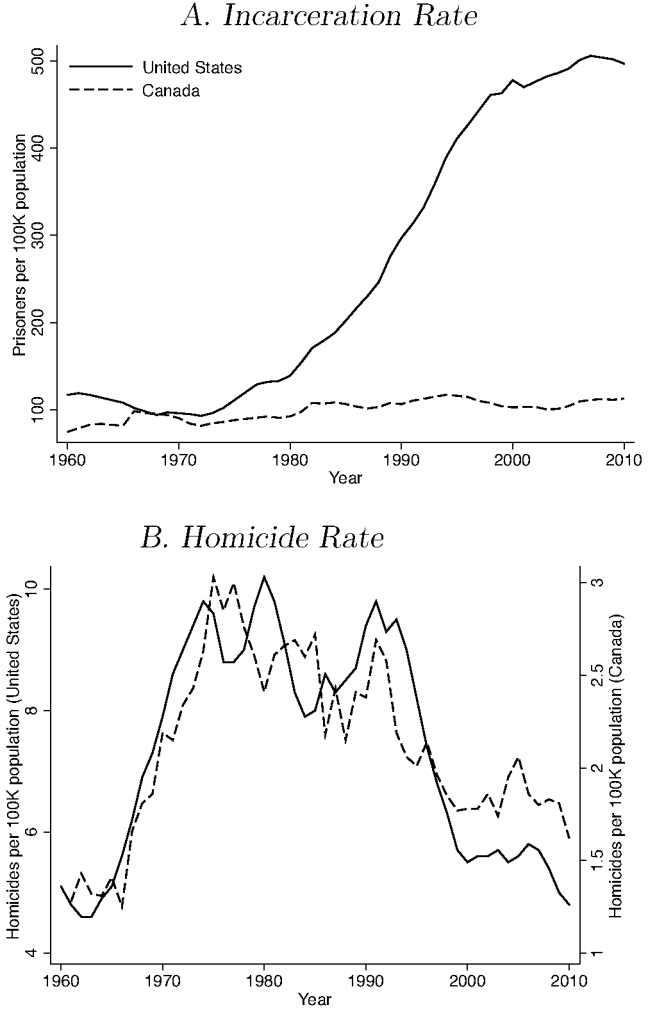
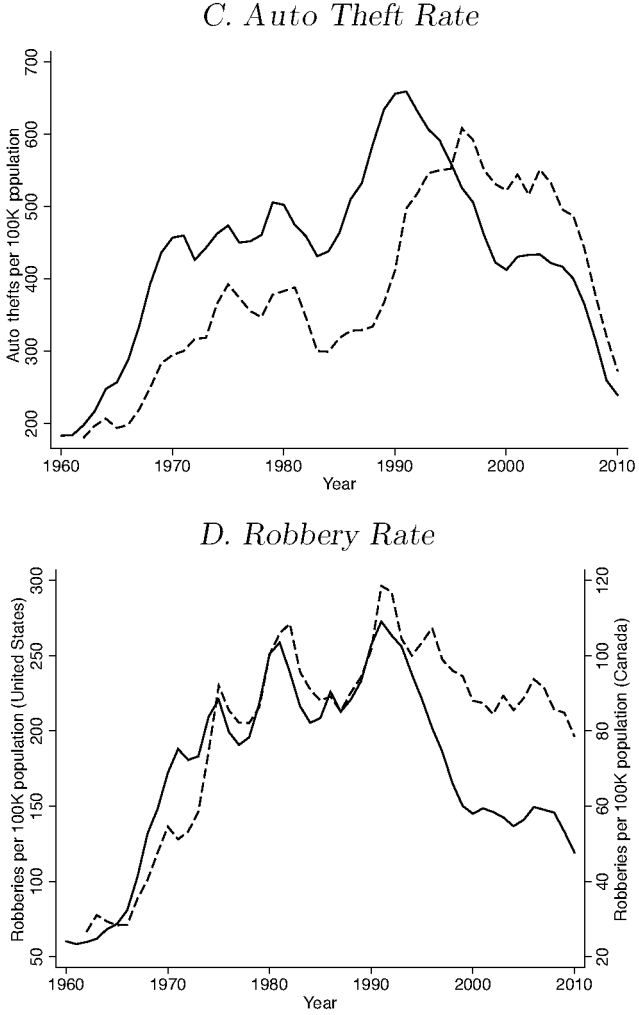


Figure 2
(continued)



Source: See text, pp. 170–71, 174.

last 30 years in the same way that the United States did. While *Statistics Canada* presently provides a series going back only to 1978, data are available going back to 1916 in *Historical Statistics of Canada*. The figure indicates that Canada has displayed little change in incarceration rates over 40 years, whereas U.S. incarceration rates have grown rapidly.

One explanation for the low Canadian incarceration rates observed in Figure 2A is a low rate of crime: a country with a low rate of crime has little need for imprisonment. However, this is not a good explanation for the stark differences in trend observed in Figure 2A because Canadian and U.S. crime rates exhibit rather similar trends. Panels B, C, and D provide time series for the rates of homicide, auto theft, and robbery, respectively, in the two countries. These are the three crime series believed to be measured most accurately in aggregate police statistics, upon which both series are based.

Despite their differences in scale, with the U.S. homicide rate generally three to four times higher than in Canada, homicide rates in the two countries exhibit remarkably similar trends (correlation coefficient of 0.86). Auto theft is more similar in its level, but somewhat less similar in its trend. In Canada, the peak auto theft rate comes about five years after the peak rate in the United States. Panel D displays the robbery rate for the two countries. The similarity in the series is remarkable; the most prominent difference in the series is that the post-1990 decline in crime is more marked in the U.S. data. An important question is whether the faster decline in crime in the United States can be attributed to the prison expansion.

These comparisons are suggestive but largely anecdotal. Nonetheless, drawing a contrast between the United States and Canada clarifies two simple points. First, despite a variety of similarities between the two countries, the increased use of imprisonment in the United States saw little parallel in Canada. Second, the effect on crime of the large investment in prisons is hard to discern with the naked eye. The United States and Canada seem to have generally similar crime trends that may or may not be related to changes in punishment policy.

Before attempting to draw any more conclusions from these data, we pause to note a conceptual difficulty with inferring the effect of punishment policy on crime using natural variation in incarceration rates. Imprisonment is an equilibrium phenomenon that reflects both changes in punitiveness as well as changes in crime, and im-

prisonment both causes and is affected by crime. McCrary (2009) emphasizes the cohort decomposition of those in prison as a means of clarifying these points. Let Q_t denote the fraction of the population in prison, G_t the fraction of those not in prison who engage in crime, p_t the fraction of offenders arrested, and $H_t(s) \equiv P_t\,(S_t \geq s)$ the fraction of arrestees obtaining a sentence of at least s periods, where s is an integer. Since those in prison were either free last period and committed an offense for which they were sentenced to at least one period in prison, or were free two periods ago and committed an offense for which they were sentenced to at least two periods, and so on, we have

$$Q_t = \sum_{s=1}^{\infty}(1 - Q_{t-s})\, G_{t-s} p_{t-s} P(S_{t-s} \geq s) \tag{1}$$

In the steady state, where G_t, p_t, and $H_t(\cdot)$ have been constant for sufficiently long that Q_t is constant, we have

$$\begin{aligned} Q &= (1 - Q)\, Gp \sum_{s=1}^{\infty} H(s) \\ &\leftrightarrow Q = \frac{Gp\mathbb{E}[S]}{1 + Gp\mathbb{E}[S]} \leftrightarrow 1 - Q = \frac{1}{1 + Gp\mathbb{E}[S]} \end{aligned} \tag{2}$$

where we make use of the fact that the sum of the survivor function is equal to the mean, or $\sum_{s=1}^{\infty} H(s) = \mathbb{E}[S]$. Some calculus shows that

$$\frac{\partial \ln Q}{\partial \ln \mathbb{E}[S]} = (1 - Q)(1 + \varepsilon) < 1 \tag{3}$$

where $\varepsilon = \partial \ln G / \partial \ln \mathbb{E}[S]$ is the elasticity of crime on the part of the free with respect to expected sentence lengths. This equation says that a 1 percent increase in the punishment schedule confronting offenders exerts less than a 1 percent increase in the incarceration rate. A standard empirical policy evaluation exercise would relate the growth rate in crime to the growth rate in imprisonment. That is, it would measure empirically the quantity $\Delta \ln C / \Delta \ln Q$, perhaps using a regression. Equation (3) shows that this approach will tend to exaggerate the effect of imprisonment on crime because the denominator is functionally related to the numerator. We will try to quantify this effect momentarily.

Outside of the steady state, we can use equation (1) to understand the dynamic effects on incarceration of a change in punishment policy. Figure 3 demonstrates the effect of an immediate shift and a slow shift in the distribution of sentence lengths on the incarceration rate with no, modest, and large deterrence effects of expected

Figure 3
Hypothetical Changes to Crime and Incarceration Rates Associated with Increases in Sentence Lengths

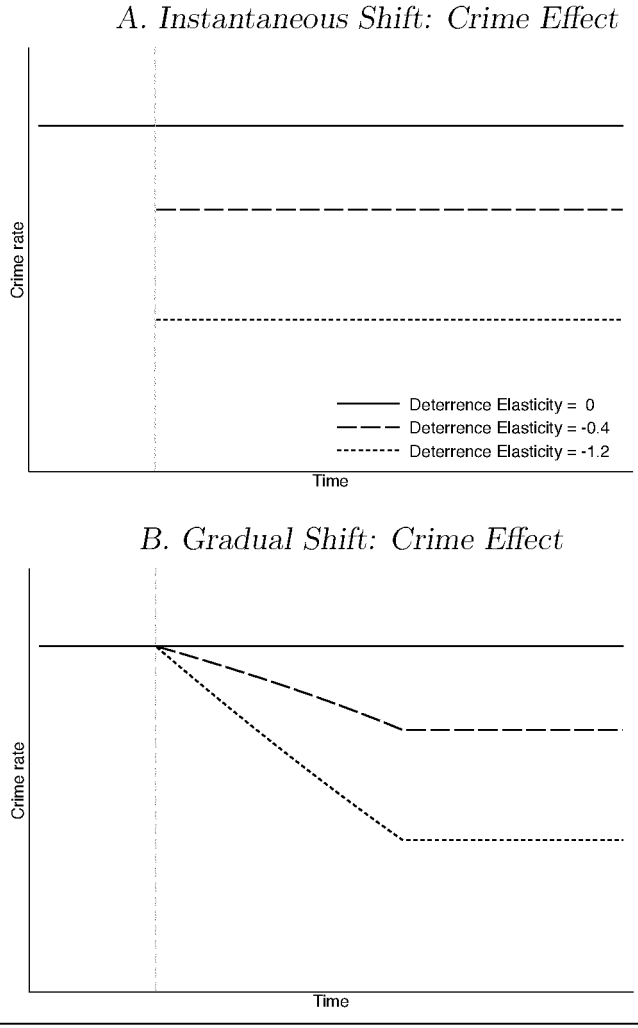
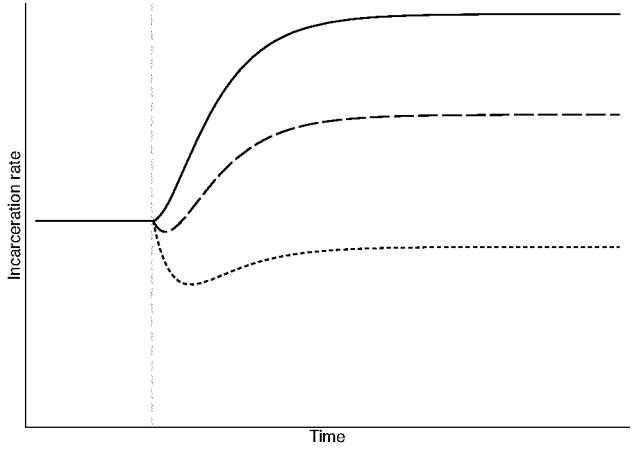
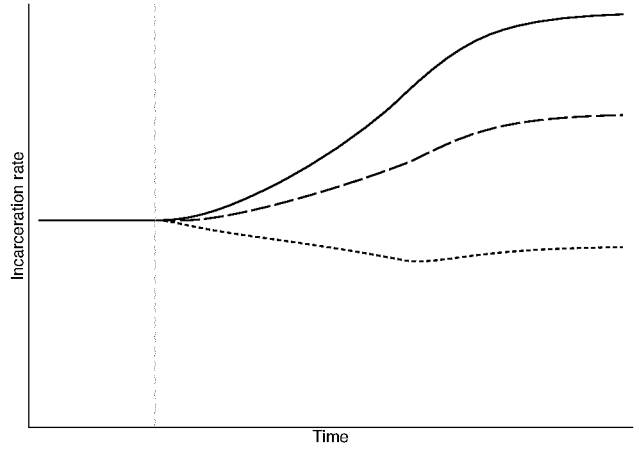


Figure 3
(continued)

C. Instantaneous Shift: Incarceration Effect



D. Gradual Shift: Incarceration Effect



sentence lengths on crime.[2] Panel A shows the effect on the overall crime rate of an instantaneous and large shift to the right in the distribution of sentence lengths. The solid line shows the crime rate assuming no deterrence; the long dashed line shows the crime rate assuming a deterrence elasticity of -0.4; and the short dashed line shows the crime rate assuming a deterrence elasticity of -1.2. The solid line imperceptibly declines after the policy reform (indicated by a vertical dashed line) because of the incapacitation effect of prison. Both dashed lines show dramatic and immediate declines because of the deterrence effect.

Panel C shows the effect of this policy reform on incarceration. The solid line increases rapidly, but at a decreasing rate, converging to the new steady-state value after 300 months and to 90 percent of the steady-state value after 120 months. Prison populations evolve very slowly, like the temperature in the ocean. Empirical evidence consistent with this fact is that while crime began dropping precipitously in 1990, the U.S. prison population continued to increase for another 19 years, until 2009. The dashed line initially declines because of deterrence effects, but after 24 months the incarceration rate rises above its initial level and continues to climb to its new steady-state value. While fewer individuals cross the threshold of the prison because of deterrence, those who do must stay longer. Interestingly, computing $\Delta \ln C / \Delta \ln Q$ yields -0.67, or about 1.68 times the deterrence elasticity of -0.4. In this example, the incapacitation effect is small enough that -0.4 is also the overall effect of a sentence enhancement on crime.

Panel B shows the effect on the overall crime rate of a more plausible policy shift, which is a linear increase in the expected sentence length facing a potential offender. The solid line is essentially unchanged (the incapacitation effect is now even less perceptible), but the dashed line declines nearly linearly in time as sentence lengths increase. Panel D

[2] The example uses a geometric distribution for sentence lengths on 0, 1, 2,... so that $P(S_t \geq s) = \gamma_t^s$, where $1 - \gamma_t$ is the per-period release probability for a prisoner. We peg the steady-state values for the key variables C_t, Q_t, G_t and p_t to roughly match empirical values for the United States in recent years. The hypothetical values for G_t are then constructed using a log linear approximation to the relationship between the crime rate of the free and the mean sentence length, i.e., we adjust the crime rate as $G' = \exp(\ln G + \varepsilon \Delta \ln \mathbb{E}[S])$, where ε is the elasticity of crime with respect to the mean sentence length and $\Delta \ln \mathbb{E}[S]$ is the percent change in the mean sentence length associated with the example. Hypothetical values for Q_t are generated directly from equation (1) and the hypothetical values for C_t are generated according to the identity $C_t = (1 - Q_t)G_t$.

shows the effects of this shift on incarceration. As before, incarceration declines at first because all the prisoners are incumbents and hence the prison exit rate is unaffected, yet the prison entry rate is lower because of deterrence. The effect is hard to detect visually but lasts for about 24 months. Eventually, the exit rate from prison is reduced because enough prisoners entered after the reform in punishment policy, and incarceration climbs rapidly thereafter.

This discussion highlights the hazards of using natural variation in incarceration rates to draw inferences about the effect of prison on crime. As panel C emphasizes visually, in the short run, one sees a positive association between incarceration and crime. This follows for two reasons. First, a spike in punitiveness reduces crime faster than it increases incarceration. Second, the immediate reduction in crime that occurs reduces the flow rate into prison enough to shrink the incarceration rate, even though the long-run consequences are for higher incarceration rates. After a decade, however, we are in a long-run scenario where there is a negative association between incarceration and crime. Nonetheless, the magnitude of the association is exaggerated because of the functional relationship between incarceration and crime. Roughly speaking, the association at long-run frequencies should be discounted by roughly $1 \div 1.67$, or about 0.6. However, if the magnitude of the elasticity of crime with respect to expected sentence lengths is sufficiently large, one will observe a positive association with incarceration and crime even in the long run.

Perhaps the most important takeaway from panel C is this: holding fixed the probability of apprehension, long-run secular increases in the incarceration rate will be observed under only two conditions. First, sentence lengths have to increase. Second, the deterrence elasticity of sentence lengths cannot be too great. Were it to be substantial, the flow rate into prison would be reduced by too much for the prison population to be able to grow. Finally, note that if deterrence effects were appreciable yet inelastic, then we should observe oscillation in the prison population, with short-run prisoner-reducing effects of policy reforms on the prison population being offset by medium- and long-run prisoner-increasing effects.

Returning to the data from the United States and Canada, we now present an analysis of the long-run differences in the data. Table 1 presents growth rates in crime and incarceration rates for Canada and the United States for 1960, 1970, 1980, 1990, 2000, and 2010. Table 2

Table 1
Log Differences in Crime and Incarceration Rates

	Canada				United States			
	Murder	Auto Theft	Robbery	Prison	Murder	Auto Theft	Robbery	Prison
One decade								
1970–1960	0.9	115	28	16	2.8	274	112	−21
1980–1970	0.2	88	46	2	2.3	45	79	43
1990–1980	0.0	29	1	14	−0.8	154	6	158
2000–1990	−0.6	110	−13	−4	−3.9	−244	−112	181
2010–2000	−0.2	−250	−9	10	−0.7	−173	−26	19
Two decades								
1980–1960	1.1	203	74	18	5.1	319	191	22
1990–1970	0.2	117	47	16	1.5	199	85	201
2000–1980	−0.6	139	−12	10	−4.7	−90	−106	339
2010–1990	−0.8	−140	−23	6	−4.6	−417	−138	200
Three decades								
1990–1960	1.1	232	75	32	4.3	473	197	180
2000–1970	−0.4	227	33	12	−2.4	−45	−27	382
2010–1980	−0.8	−111	−22	20	−5.4	−263	−132	358
Four decades								
2000–1960	0.5	342	61	28	0.4	229	85	361
2010–1970	−0.6	−23	24	22	−3.1	−218	−53	401
Five decades								
2010–1960	0.3	92	52	38	−0.3	56	59	380

Table 2
Estimated Effect of Prison on Crime:
U.S.-Canadian Comparisons

	Naive			Adjusted		
	Murder	Auto Theft	Robbery	Murder	Auto Theft	Robbery
One decade						
1970–1960	−0.05	−4.32	−2.28	−0.03	−2.59	−1.37
1980–1970	0.05	−1.05	0.81	0.03	−0.63	0.49
1990–1980	−0.01	0.87	0.03	0.00	0.52	0.02
2000–1990	−0.02	−1.92	−0.53	−0.01	−1.15	−0.32
2010–2000	−0.06	8.43	−1.81	−0.04	5.06	−1.08
Two decades						
1980–1960	0.96	27.98	28.28	0.57	16.79	16.79
1990–1970	0.01	0.44	0.21	0.00	0.26	0.12
2000–1980	−0.01	−0.70	−0.29	−0.01	−0.42	−0.17
2010–1990	−0.02	−1.43	−0.59	−0.01	−0.86	−0.36
Three decades						
1990–1960	0.02	1.62	0.82	0.01	0.97	0.49
2000–1970	−0.01	−0.74	−0.16	0.00	−0.44	−0.10
2010–1980	−0.01	−0.45	−0.33	−0.01	−0.27	−0.20
Four decades						
2000–1960	0.00	−0.34	0.07	0.00	−0.20	0.04
2010–1970	−0.01	−0.52	−0.20	0.00	−0.31	−0.12
Five decades						
2010–1960	0.00	−0.11	0.02	0.00	−0.06	0.01

presents naive and adjusted estimates of the effect of punishment on crime. The naive estimates are the difference-in-difference for the given crime rate (i.e., the U.S.-Canadian difference in the temporal growth rate) relative to the difference-in-difference for the incarceration rate. The adjusted estimates are discounted by 0.6, reflecting the conceptual discussion above.

These estimates indicate that there are often quite violent swings in crime rates that have little to do with changes in penal policy. This is consistent with a potential identification problem, which is that in the medium run, changes in incarceration rates may be a response to changes in crime. Our preferred difference is the longest difference in the data. We are persuaded that the U.S.-Canadian difference in response to crime between 1960 and 2010 has less to do with crime than it has to do with politics and culture. Even if the dramatic run-up in incarceration rates in the United States were reflective of a response to crime, it was plausibly a response to the crime wave of the 1960s and 1970s, and not to current conditions.

Our preferred 2010–1960 difference indicates very small effects of prison on crime. These are consistent with zero and are generally small in magnitude. However, the 2010–1970 difference is essentially as credible on a priori grounds to us and is more consistent with the idea that prison is protective against crime. Plainly, more data are needed to triangulate.

We turn now to the data from England and Wales. Figure 4 is structured analogously to Figure 2, and Tables 3 and 4 are structured analogously to Tables 1 and 2. The results for England and Wales depend less on the base year. The estimates for both 2010–1960 and 2010–1970 indicate that prison may indeed be protective against crime.

4. PANEL DATA REGRESSIONS

We estimate

$$C_{ct} = \alpha_c + \delta_t + \gamma Q_{ct} + \epsilon_{ct} \tag{4}$$

where C is either robbery, homicide, or auto theft. These results are in Table 5. Table 6 lists the number of observations each country contributes to these regressions. The results are quite sensitive to specification, with the seemingly innocuous change from levels to logs changing the sign of the robbery estimate.

Figure 4
Imprisonment and Crime: United States and England and Wales

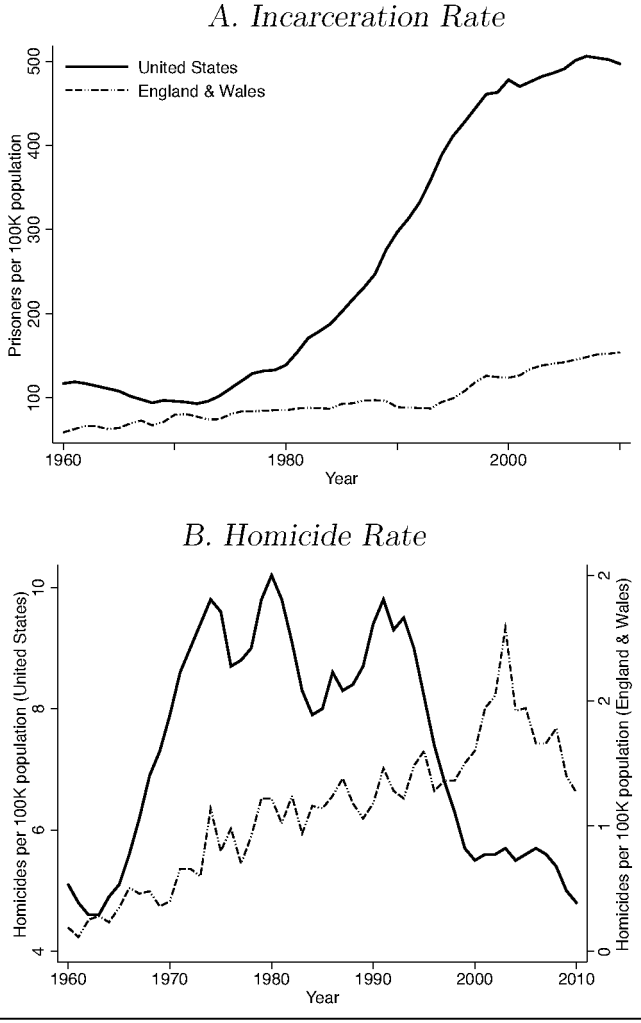


Figure 4
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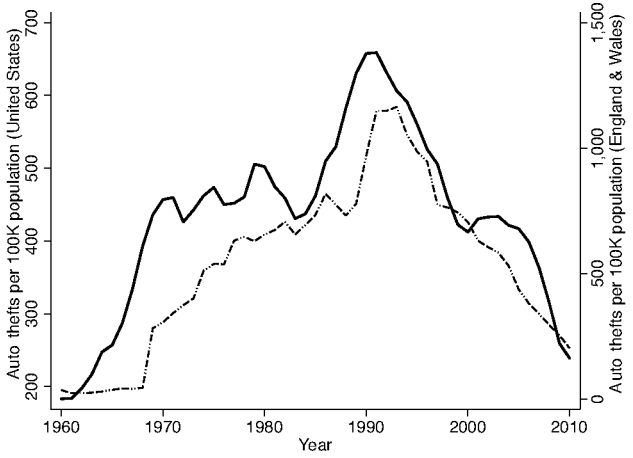
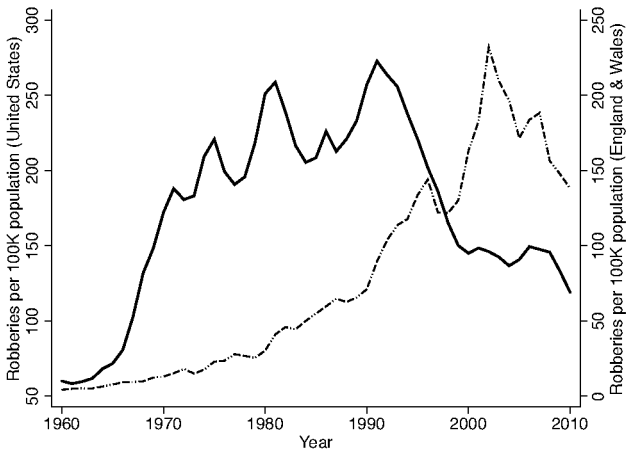
C. Auto Theft Rate



D. Robbery Rate



Source: See text, pp. 170–71.

Table 3
Log Differences in Crime and Incarceration Rates

	England and Wales				United States			
	Murder	Auto Theft	Robbery	Prison	Murder	Auto Theft	Robbery	Prison
One decade								
1970–1960	0.11	269.33	8.71	20.92	2.8	273.8	112.0	−21
1980–1970	0.41	350.95	17.39	5.32	2.3	45.4	79.0	43
1990–1980	−0.02	314.81	40.75	3.75	−0.8	155.6	5.9	158
2000–1990	0.21	−265.92	91.95	34.95	−3.9	−245.6	−112.0	181
2010–2000	−0.17	−502.55	−24.80	29.97	−0.7	−173.4	−25.9	19
Two decades								
1980–1960	0.52	620.28	26.09	26.24	5.1	319.2	191.0	22
1990–1970	0.39	665.76	58.13	9.07	1.5	201.0	84.9	201
2000–1980	0.19	48.89	132.70	38.70	−4.7	−90.0	−106.1	339
2010–1990	0.04	−768.47	67.15	64.92	−4.6	−419.0	−137.9	200
Three decades								
1990–1960	0.50	935.10	66.84	29.99	4.3	474.8	196.9	180
2000–1970	0.60	399.84	150.08	44.02	−2.4	−44.6	−27.1	382
2010–1980	0.02	−453.66	107.90	68.67	−5.4	−263.4	−132.0	358
Four decades								
2000–1960	0.71	669.18	158.79	64.94	0.4	229.2	84.9	361
2010–1970	0.43	−102.71	125.28	73.99	−3.1	−218.0	−53.0	401
Five decades								
2010–1960	0.54	166.63	133.99	94.91	−0.3	55.8	59.0	380

Table 4
Estimated Effect of Prison on Crime: U.S.–England and Wales Comparisons

	Naive			Adjusted		
	Murder	**Auto Theft**	**Robbery**	**Murder**	**Auto Theft**	**Robbery**
One decade						
1970–1960	−0.06	−0.11	−2.46	−0.04	−0.06	−1.48
1980–1970	0.05	−8.11	1.64	0.03	−4.87	0.98
1990–1980	−0.01	−1.03	−0.23	0.00	−0.62	−0.14
2000–1990	−0.03	0.14	−1.40	−0.02	0.08	−0.84
2010–2000	0.05	−30.00	0.10	0.03	−18.00	0.06
Two decades						
1980–1960	−1.08	71.02	−38.90	−0.65	42.61	−23.34
1990–1970	0.01	−2.42	0.14	0.00	−1.45	0.08
2000–1980	−0.02	−0.46	−0.80	−0.01	−0.28	−0.48
2010–1990	−0.03	2.59	−1.52	−0.02	1.55	−0.91
Three decades						
1990–1960	0.03	−3.07	0.87	0.02	−1.84	0.52
2000–1970	−0.01	−1.31	−0.52	−0.01	−0.79	−0.31
2010–1980	−0.02	0.66	−0.83	−0.01	39.00	−0.50
Four decades						
2000–1960	0.00	−1.49	−0.25	0.00	−0.89	−0.15
2010–1970	−0.01	−0.35	−0.55	−0.01	−0.21	−0.33
Five decades						
2010–1960	0.00	−0.39	−0.26	0.00	−0.23	−0.16

Table 5
Estimated Effect of Prison on Crime, World Panel

	Dependent variable is crime per 100,000 population					
	Robbery	**Homicide**	**Auto Theft**	***ln* (Robbery)**	***ln* (Homicide)**	***ln* (auto theft)**
Incarceration rate	0.028	−0.010	−0.336			
	(0.028)	(0.002)	(0.102)			
ln (Incarceration rate)				0.312	−0.333	−0.232
				(0.078)	(0.043)	(0.077)
Adjusted R^2	−0.054	−0.015	−0.044	−0.028	0.043	−0.048
Observations	649	591	529	649	591	529

Note: Standard errors in parentheses.

We also estimate the log difference regression

$$C_{ct} - C_{ct-s} = \beta(Q_{ct} - Q_{ct-s}) + \mu_{ct} \tag{5}$$

as a function of the lag length, s. These results are in Figure 5, with the solid lines representing point estimates and the dashed lines the 95 percent confidence intervals. Table 7 lists the number of observations each country contributes to the regressions.

On a priori grounds, we prefer these results to those of Table 5 because they focus on long-run differences, which are less affected by the mechanical relationship between incarceration and crime. However, the results of this empirical exercise are difficult to interpret because of the differing composition of countries. Nonetheless, bracketing the issue on composition, some conclusions may be drawn. First, for homicide and auto theft, the short-run estimates tend to be more positive than those 5 to 10 years out. This is somewhat consistent with a deterrence hypothesis, with the short-run estimates contaminated by the short-run reduction in the flow rate into prison. As discussed above, this effect exerts a positive bias on the estimated coefficients. However, the same tendency is not present for robbery, warning against strong interpretation. Second, after 20 years, according to the data, incarceration tends to have much smaller negative effects—and possibly large and positive effects—on

Table 6
Distribution of Country Observations for Regressions of Table 5

Country	Dependent Variable			First Year of Data
	Robbery	Homicide	Auto Theft	
United States	41	41	41	1970
Canada	39	39	15	1970
England & Wales	41	41	41	1970
Australia	24	17	15	1982
Austria	13	16	15	1994
Belgium	10	10	10	2000
Bulgaria	27	32	15	1970
Croatia	11	11	10	1994
Czech Republic	17	1	17	1993
Denmark	17	17	17	1993
Estonia	14	16	17	1993
Finland	23	17	17	1987
France	13	16	16	1994
Greece	15	15	12	1993
Hungary	26	17	17	1982
Ireland	10	16	17	1993
Italy	17	17	17	1993
Japan	26	15	13	1980
Latvia	15	15	15	1995
Lithuania	17	17	17	1993
Macedonia	13	9	9	1990
Netherlands	21	16	17	1987
New Zealand	15	15	15	1994
Northern Ireland	17	16	17	1993
Norway	17	17	17	1993

(continued)

Table 6
(continued)

Country	Dependent Variable			First Year of Data
	Robbery	Homicide	Auto Theft	
Poland	17	17	17	1993
Russia	12	12	9	1994
Scotland	26	17	17	1982
Serbia	8	8	8	2002
Slovenia	17	15	16	1993
South Africa	14	14	14	1994
Sweden	23	17	17	1987
Switzerland	17	16	1	1993
Turkey	16	16	16	1993
Total observations	649	591	529	

Source: See text, p. 171.

Figure 5
World Panel Log Difference Regressions

A. Homicide Rate

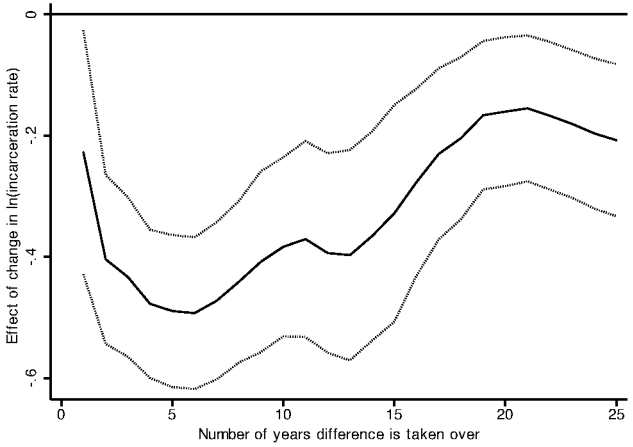
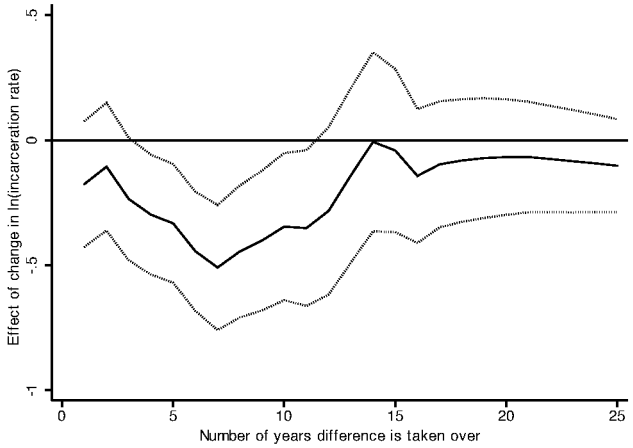


Figure 5
(continued)

B. Auto Theft Rate



C. Robbery Rate

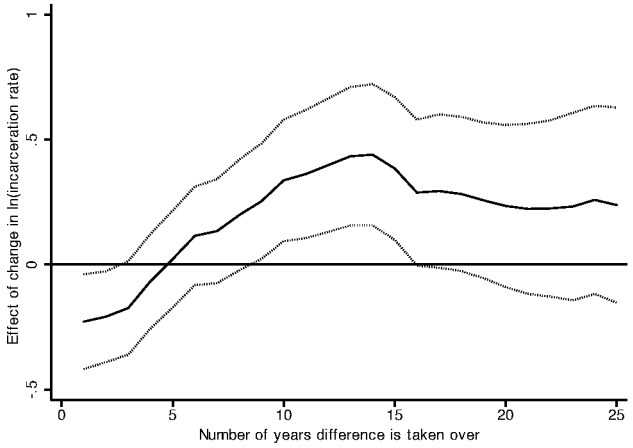


Table 7
Distribution of Country Observations for Regressions of Figure 5

	Dependent variable and number of years over which difference is taken					
	Robbery		**Homicide**		**Auto Theft**	
Country	**1 year**	**10 years**	**1 year**	**10 years**	**1 year**	**10 years**
United States	40	31	40	31	40	31
Canada	37	29	37	29	0	0
England & Wales	40	31	40	31	40	31
Australia	20	13	16	7	14	5
Austria	12	3	15	6	6	5
Belgium	9	0	9	0	9	0
Bulgaria	23	17	23	22	14	5
Croatia	9	1	9	1	9	0
Czech Republic	16	7	0	0	0	7
Denmark	16	7	16	7	16	7
Estonia	12	5	15	6	16	7
Finland	22	13	16	7	16	7
France	12	3	15	6	15	5
Greece	14	5	14	5	5	2
Hungary	23	16	16	7	16	7
Ireland	9	0	15	6	6	7
Italy	16	7	16	7	16	7
Japan	22	16	14	5	12	3
Latvia	14	5	14	5	5	5
Lithuania	16	7	16	7	16	7
Macedonia	10	3	8	0	8	0
Netherlands	19	11	15	6	16	7
New Zealand	14	5	14	5	14	6
Northern Ireland	16	7	15	6	16	7
Norway	16	7	16	7	16	7
Poland	16	7	16	7	16	7
Russia	11	2	11	2	2	0
Scotland	23	16	16	7	16	7
Serbia	7	0	7	0	7	0
Slovenia	16	7	14	5	15	6
South Africa	13	4	13	4	4	4
Sweden	22	13	16	7	16	7
Switzerland	16	7	15	6	6	0
Turkey	15	6	15	6	15	6
Total observations	596	311	547	263	496	212

Source: See text, p. 171.

crime. For homicide, the long-run estimate is approximately −0.20. For auto theft, it is close to −0.10, and for robbery it is roughly 0.25. This is potentially consistent with short-run deterrence effects that are negative and general equilibrium effects that are positive. Overall, however, we caution against strong interpretation based on the regression estimates.

5. CONCLUSION

Since the data are not definitive, a natural question is whether there is evidence against a stark prior. An example of such a stark prior is one that posits no general equilibrium effects and large deterrence effects of punishment. We see three key problems with such an interpretation of the data. First, while in the 1990–2010 period incarceration was generally on the rise in the United States and crime was on the decline, incarceration was rising faster in the 1970–1990 period and no decline in crime was evident. Indeed, crime was rising. Of course, the increase in crime may well have been the impetus for the increased sentences that led to higher incarceration rates.

Second, however, U.S. fluctuations in crime rates are not without peer. Figure 2 indicates that Canadian crime, particularly homicide and robbery, has turning points similar to the U.S. series. This is despite the fact that Canadian incarceration rates are essentially flat over the last 40 years. While Canadian auto theft's turning point is roughly five to seven years after that of the United States, the turning point for England and Wales is essentially the same. However, homicide and robbery in England and Wales turn 10–12 years after they do in the United States. In all three countries, crime is on the decline for all three of these crime types in recent years. This indicates that it is not necessary to have an explosive expansion in prison capacity to see major crime declines, since neither Canada nor England and Wales expanded their prison capacity, yet they eventually saw crime declines.

Third, the timing of the story works poorly. As noted above, an increase in sentence lengths takes some time to work its way through to increases in prison population. Using an example in which we calibrate to U.S. data in 1970, we show that the "python" is not done "swallowing the pig" even after a decade: sentence lengths affect prison populations with a long lag. This implies that the increase in prison population between 1990 and 2000, say, was likely the result

of changes to sentencing policy put in place in 1980–85. However, the data contain little evidence of this timing.

Overall, we can hardly doubt that, ceteris paribus, an increase today in the sentence length confronting a potential offender does not have a positive influence on the probability that a nonincarcerated person will commit a crime. This channel would weakly reduce crime. We certainly do not doubt that the same increase in the sentence length would lead to increases in prison stays for those who do elect to commit crime. However, we are not persuaded that these are the only two relevant effects of a shift in punishment policy on the aggregate crime rate. Future work should focus on research designs capable of teasing out these important, but elusive, mechanisms.

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Comment

Steven N. Durlauf

Justin McCrary and Sarath Sanga's paper represents an ambitious effort to identify the general equilibrium effects of imprisonment without the use of a structural model but rather through comparisons of similar polities with very dissimilar imprisonment rates. Methodologically, the paper makes two contributions: First, it provides a careful delineation of the distinction between the transition and steady-state empirical relationship between imprisonment rates and crime rates. Second, the paper uses cross-country comparisons to allow for a difference-in-difference strategy to evaluate the effects of imprisonment on crime and so avoids reliance on a structural model. This second idea constitutes the conceptual basis of the paper. As such, this second methodological contribution is tied to current debates in empirical economics and econometrics about the role of economic theory in empirical work.

My discussion will focus on the second contribution because the paper only succeeds to the extent that the cross-country comparison strategy does in fact produce estimates of general equilibrium effects. I will argue that the strategy fails in this respect. In doing so, let me be clear at the start that this is an ambitious and carefully executed analysis of a difficult and policy-relevant problem. The authors' analysis represents a first step in a longer research program. For this reason, while my comments will be critical, they should not be construed as suggesting that the research program be abandoned. Rather, I believe that there may well be a useful role for analyses of the type the authors pursue.

In the discussion, I will refer only to comparisons between the

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United States and Canada and England and Wales. The criticisms I make apply a fortiori to comparisons between the United States and other countries that appear in the paper.

IS THIS FORM OF ANALYSIS APPROPRIATE?

The first criticism I have with the authors' empirical strategy concerns the assumption that the polities under study are so similar that bivariate imprisonment crime relationships can be revealed by a difference-in-difference strategy. The thought experiment behind the paper is well encapsulated in McCrary and Sanga's statement, "We pay particularly close attention to Canada and England and Wales, as these are natural comparisons for the United States." How is this (asserted) comparability exploited in the empirical exercises? Determinants of crime rates and imprisonment rates that are not due to differences in sanction regimes are "canceled out" in the cross-polity differences. This cancellation plays a role for both series. First, it eliminates factors other than the sanction regime that determine crime. Second, it purges aspects of imprisonment that may be unrelated to the sanction regime difference. However, in my view it is hard to conclude that the difference-in-difference approach is credible in producing interpretable, policy-relevant information on punishment and crime rates.

First, the authors do not make an adequate case that the United States, Canada, and England and Wales are sufficiently similar that the only difference between them is that the United States imposes harsher penalties than the other two polities. The question is not whether the three polities are similar, but whether one can argue that they are comparable in a way that justifies the authors' interpretation that a comparison of the difference in crime rates and imprisonment rates yields evidence of "the" effect of imprisonment on crime. In some sense, the authors are in great trouble in making such an assumption because of the data series they have chosen to study: U.S. crime and imprisonment rates are quite different from Canada and England and Wales, so it is obvious that one has to make an argument that comparability holds along the dimensions needed for this particular study. Two pieces of evidence are presented in favor of comparability. Figures 2B and 2D and 4B and 4D show that if one rescales the homicide and robbery rates for the United States versus Canada and the United States versus England and Wales, both pairs

of series appear to move together. Second, there is a high correlation, 0.86, between the U.S. and Canadian homicide rates. Leaving aside the failure to provide a comparable statistic for robberies and the confused discussion of trends (the term is not defined and it is not clear what the authors mean when they distinguish trends and levels; in time series analysis one distinguishes trends and *cycles*), neither of these pieces of evidence really tells us anything about comparability. One reason is statistical: it has long been known that spurious correlations can be found between time series with temporal dependence (Granger and Newbold 1974). So neither piece of evidence is, on its face, informative about actual similarities in the countries. Second, the fact that the crime series for two polities move together is consistent with the coexistence of common and idiosyncratic determinants of the series. It does not mean that the polities react similarly to a given variable such as imprisonment policy.

This comparability problem has plagued the cross-country growth literature. Evidence of coefficient heterogeneity, nonlinearity, and residual region-specific heterogeneity have all proven to be of first-order importance in understanding cross-country growth patterns; see Brock and Durlauf (2001) and Durlauf, Johnson, and Temple (2006) for elaboration. Given the checkered history of empirical claims based on cross-country growth regressions (many of which evaporate when comparability assumptions are relaxed), concern about comparability issues for cross-country analyses of crime and imprisonment is only natural. The authors need to provide a credible justification as to why their differencing strategy is adequate for the questions they wish to answer rather than simply relying on conventional wisdom that the United States, Canada, and England and Wales are similar in a manner that renders the authors' empirical strategy valid or using a simple correlation to justify the implicit exchangeability assumption they are making. (See Brock and Durlauf (2001) for a discussion of exchangeability, which is the statistical formalization of the comparability notion used in this paper.)

The cross-country growth literature can in fact be used to illustrate why the authors' empirical strategy is problematic. The use of log differences in this paper can be interpreted as comparing $\overline{\Delta \log CRIME_{U.S.}} - \overline{\Delta \log CRIME_{other}}$ to $\overline{\Delta \log IMPRISONMENT_{U.S.}} - \overline{\Delta \log IMPRISONMENT_{other}}$. Suppose one asked the following cross-country growth question: What is the effect of a country's savings rate

on its growth rate? If one were to use the methodology of this paper, one would answer the question by comparing $\overline{\Delta \log INCOME_{U.S.} - \Delta \log INCOME_{other}}$ to $\overline{\Delta \log SAVINGS_{U.S.} - \Delta \log SAVINGS_{other}}$. However, this would make little sense even if data were restricted to similar countries; for example, the United States, Canada, and England and Wales. Why? The Solow growth model (a useful baseline) implies that growth is determined by initial income and population growth; augmentations of the model would include factors such as monetary and fiscal policy, all of which differ across countries. Note that if the question were the effect of a physical capital savings rate, one would be assuming that the effects of the human capital savings rates are canceled out. The problem is that comparability in the loose form that is documented in the paper does not mean that the difference in growth rates can be attributed to differences in a single variable.

A possible authors' rejoinder is that the imprisonment difference swamps other differences between the United States and Canada or England and Wales as pertains to imprisonment and crime. But this is not obvious unless one knows how the other variables affect crime. Further, there are good reasons to question comparability as it is used here. One obvious source is differences in the time series for unemployment, output, etc., which presumably bear upon crime. In Durlauf (1989), I found that the difference between per capita output for the United States and Canada under a second order autoregression specification produced coefficients that summed to 0.95, and the difference between the United States and the United Kingdom under a second order autoregression specification produced coefficients that summed to 0.83. Standard errors were large enough that one could conclude there were unit roots. Similar results have subsequently appeared in the years since.

I believe this lack of comparability likely also reflects cultural factors involved in "American exceptionalism." Personally, I think that economic models of crime ignore the moral dimension to criminal behavior, which in my judgment is something other than a claim about the disutility of immoral actions. I further think that most social scientists would agree that there are complex cultural reasons why immoral behaviors (e.g., crime) can differ across countries. Whether or not one agrees with my view that preference heterogeneity cannot accommodate the moral aspects of individual choice, many attitudinal differences can easily be identified among the three polities the authors study in the World Value Survey (Inglehart et al.

2010). I do not delineate these because their relationship to crime is not clear. For example, Americans are far more religious than Canadians or the British (Inglehart et al. do not break out England and Wales).[1] That would seem inconsistent with the high American crime rates. But if one considers oppositional culture ideas (e.g., Ogbu 2003), which have been applied to understand black/white educational differences to socioeconomic groups, one can imagine that, given the value society places on success, those who are not well off may react by rejecting mainstream ethical norms. Obviously, this is speculative. My argument is that the authors do not have a theory of why cultural differences on crime-relevant dimensions do not constitute confounders that invalidate the conceptual basis of their thought experiment.

PRIMACY OF THE SANCTION REGIME

My second criticism is that McCrary and Sanga's paper does not establish that the estimated imprisonment rate effects are policy-relevant. The authors recognize that natural variation in incarceration rates occurs for multiple reasons, writing, "These estimates indicate there are often quite violent swings in crime rates that have little to do with penal policy." I would go further. The imprisonment rate is an endogenous variable, so all the calculations in this paper involve comparing differences in endogenous variables. It is unclear how the calculations should affect our thinking about criminal justice policy. The policy-relevant object for assessing criminal justice policy is the *sanction regime*, which determines both the crime and imprisonment rates. Sanction regimes are usually specified in terms of the probability of apprehension and severity of punishment. Blumstein and Nagin (1978) showed that in a steady state, the relationship between crime rates and imprisonment rates is not constrained by deterrence theory in the sense that a change in the sanction regime can either raise imprisonment and lower crime or lower both imprisonment and crime; the effect depends on the elasticity of the crime rate with respect to certainty and severity. The McCrary

[1] On a 1–10 scale, with 10 meaning very important, 26 percent of the British, 49 percent of Canadians, and 58 percent of Americans answered 9 or 10 to the question, "How important is God in your Life?" in 2006. These differences are qualitatively similar for other years (Inglehart et al. 2010, 246).

and Sanga simulation results suggest the same is true for transitions. The authors are in essence regressing endogenous variables against endogenous variables and relying on the interpretation of coefficient values at different horizons in the differences to tell a story about policy. This is not uninteresting, but it does not indicate what substantive information is revealed in the calculations. The Blumstein and Nagin result illustrates why one cannot make facile claims as to what is learned from comparing imprisonment and crime rates. For this reason, McCrary and Sanga's title is misleading. The concept of a general equilibrium effect of imprisonment on crime is as conceptually ill-posed as the concept of a general equilibrium effect of prices on output levels. A well-posed concept is the general equilibrium effect on both imprisonment and crime of an exogenous change in the sanction regime.

Once one considers the complexities involved in describing a sanction regime, the interpretability of the authors' results is further muddied. Actual sanction regimes will involve distributions of penalties as functions of offender characteristics, including past offenses. A "three-strikes" policy is very different from a policy of automatic imprisonment for a first felony. I believe that U.S. imprisonment policy is inefficient in terms of maximizing deterrence for a steady-state imprisonment rate. (Durlauf and Nagin 2011a and 2011b conclude that three-strikes policies should not be continued for cost-benefit reasons, for example.) But nothing in the exercises done in this paper would let me draw policy-relevant conclusions of this type.

The limitations of the substantive conclusions one can draw are inherent in the atheoretical empirical strategy the authors have chosen. Some explicit description of the process by which individuals make the choice to commit a crime is needed to produce interpretable results. The statistical exercises produce evidence on long-run relationships between endogenous variables; but without a behavioral framework, the relevance of the evidence to the question of how different sanction regimes affect imprisonment and crime is unclear. And this question is precisely the one that is relevant to policy.

FUTURE WORK

These criticisms do not imply that McCrary and Sanga's exercises fail to provide insights that may be useful in policy evaluation. Vector autoregression and cointegration analyses are purely statistical

methods that have produced important insights in macroeconomics. Understanding the strengths and limitations of these methods took years of research and debate.

McCrary and Sanga have launched a research program. It is easy to raise objections at this early stage of their work. My remarks do not imply that they are on the wrong track. Hence, while I do reject the claim that this paper has produced credible general equilibrium estimates of the different effects of alternative sanction regimes on crime, on the research program I conclude with the Scottish verdict "Not Proven." When evaluated against theoretical models of crime so that the informational content of the exercises is better understood, the research program may prove to be a useful contribution to the abductive analysis of sanction policy.

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Comment

Bruce Sacerdote

Justin McCrary and Sarath Sanga present an innovative paper on a critical public policy topic. The authors are pursuing a very big question, namely whether the large U.S. expansion in incarceration rates over the past few decades paid off in terms of reduced crime. The United States had a large increase in incarceration beginning in the 1980s, which accompanied a massive crime wave. Crime began to subside in the mid-1990s and continues to fall. It is tempting to attribute some or all of the drop in the crime rate to increased incarceration. But as the authors show, this is not a simple causal relationship; crime, sentence length, and imprisonment are all endogenous variables within a complex system. The authors note that other countries experienced similarly timed rises and falls in crime without the expansion of imprisonment.

The authors' hope is to construct a difference-in-differences estimate, which takes the United States/Canada (or United States/England and Wales) log differences in the crime rate over long time periods and divides by the log differences in the incarceration rate. This is intended to capture the effects of changes in incarceration on crime. Subtracting out the changes for the comparison country (Canada or England and Wales) is supposed to remove general time trends that are common to both countries but that are unrelated to incarceration policy.

McCrary and Sanga's paper assumes that the big run-up in incarceration rates is a policy shift that is largely exogenous to crime. They present a plausible argument for this and, if the shift is exogenous to the crime and punishment system of equations, they may be able to identify the effect of imprisonment on crime.

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The authors' object of interest is how crime responds to the sentence length. Because a long time series of average sentence lengths is hard to construct, the authors use the actual incarceration rate as a proxy for the severity of sentence lengths. The paper includes a very nice section on how the dynamics of crime (C) and incarceration (Q) would evolve in response to an exogenous shift in sentence length (S). The bottom line from this analysis is that in the presence of deterrence, calculating $\Delta \ln C / \Delta \ln Q$, will tend to overstate the true elasticity of crime with respect to sentence length. Both discussants agreed that working out the dynamics of this problem is a nice contribution to the literature.

The authors conclude that there is little evidence that crime fell in the United States in response to increased incarceration rates. However, it's important to recognize that the results depend greatly on which log difference is used and which crime rate (murder, auto theft, robbery, etc.) is used. For example, the 1970–2010 differences for motor vehicle theft show big drops in crime associated with big increases in incarceration. Mechanically, this is because, relative to its base value, auto theft fell by 10 times as much in the United States as it did in Canada. The audience and the other discussant encouraged the authors to add standard errors to their point estimates, which seems like a sensible idea.

My first suggestion for the authors is to attempt to distinguish among imprisonment regimes for various crimes in the analysis. If 40 percent of the run-up in imprisonment in the United States is for drug-related crimes, then separating those from other crimes may affect the results. Admittedly, drug crimes are often comingled with robbery, assault, etc., so this is not a clean or clear-cut task. But given the importance of big shifts in narcotics policy in explaining the run-up in imprisonment, this issue ought to be addressed. The data might show, for example, that auto thefts responded to auto theft sentences.

My second suggestion is to consider measuring expected sentence length directly rather than using incarceration rates as a proxy. Again, the availability of data is a big issue, but using sentencing data directly would make measuring the elasticity of crime with respect to sentence length easier and would remove one of the endogeneity problems.

This leads to my third point, which brings out the great strengths and weaknesses of the paper. This is essentially a macro paper

written on a question for which we have traditionally used micro evidence. The authors already have a series of excellent papers using local identification to get at the deterrence effects of enhanced sentences. Specifically, McCrary and Lee (2009) and McCrary and Sanga (2011) use the discontinuity in expected sentence that occurs when offenders turn "the age of majority." That is, expected sentence length increases when a potential offender turns 16, 17, or 18 years old. And since states have actually altered those laws, there is not only significant discontinuity in expected sentence, but exogenous shifts in the point in the age distribution where that discontinuity is located.

More broadly, it may be possible to identify other exogenous shifts in sentence length at the state, local, or crime-specific level. For example, Kuziemko (forthcoming) uses a large prison release in Georgia to identify the effects of actual sentence length on recidivism. Kessler and Levitt (1999) suggest that the implementation of sentence enhancement laws could be useful for identifying deterrence effects. These sorts of identification strategies allow one to hone in on the effect of interest with less worry about differential trends in unobservables or the endogeneity of incarceration and crime driving the results.

McCrary and Sanga take a much bigger-picture approach, which is potentially good—we might miss the forest for the trees if we look at too narrow a source of local identification. And I agree with the authors that it is useful at least to ask whether the broad time trend in incarceration and crime can help explain whether incarceration reduces crime. The downside of the big-picture/macro approach is its reliance on strong assumptions (e.g., the increase in incarceration is an exogenous policy change) and the potential for confounding factors to invalidate the analysis.

One such confounding factor might be the crack epidemic of the 1980s and 1990s (see Fryer, Heaton, Levitt, and Murphy, forthcoming). As mentioned above, a large portion of the run-up in imprisonment was for drug-related crimes, which are counted in the imprisonment statistics but not necessarily the crime statistics in the authors' macro analysis. During discussion of the paper, many people mentioned prosecutorial discretion and the fact that many crimes are interrelated. Police and prosecutors may simply pursue the most convenient charge or the one with the stiffest penalty. If all

crack crimes are connected with "index" crimes, then the authors' approach is a very useful one and gets around this inability to separate crimes cleanly.

Overall, I found this paper to be a useful exercise, even though it did not shift my priors as much as the authors' related work on deterrence using micro data. I appreciate that in the current paper the authors attempt to capture both deterrence and incapacitation effects, especially given that we seem to have many clever micro identification strategies to measure deterrence and few to measure incapacitation.

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