When Prisoners Get Out: The Impact of Prison Releases on Homicide Rates, 1975-1999

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Although studies examining the relationship between prison populations and homicide rates find significant negative impacts, no published studies have examined the impact of prison releases on homicide. The issue here is whether release rates directly affect crime, independently of any impact they might have through their impact on prison populations. We examine this question by regressing homicide rates on prison release rates, prison population, and numerous control variables using panel data for 46 states from 1975 to 1999. The results provide no evidence of a significant positive relationship between prison releases and homicide. Similar to prior studies, however, we found that prison population growth has greatly reduced homicide rates. The main policy implication of these findings is that those leaving prison have no greater propensity to commit homicide than those entering or remaining in prison.

Keywords: *prison release; prison population; homicide*

As is well known, U.S. criminal justice policy has increasingly relied on a strategy of building and filling prisons to control crime (Austin & Irwin, 2001; Mauer, 1999). The U.S. imprisonment rate grew from 139 to 478 per 100,000 population, or by 244%, from 1980 to 2000 (Bureau of Justice Statistics [BJS], 2003). As of 2000, the United States had the highest incarceration rate of all industrialized nations (Stern, 2002). In 1999, the United

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States spent a record $49 billion on corrections, or about 3 cents of every dollar spent by state and local governments (BJS, 2002, p. 4).

Support for crime control through imprisonment is based on the rationales that the threat of prison terms deters criminals and that incarcerated criminals are unable to commit crimes on the street. A large body of literature has examined the relationship between prison populations and crime rates, especially homicide. Most time-series analyses of national U.S. imprisonment rates find large negative impacts on homicide rates (e.g., Cohen & Land, 1987; Devine, Sheley, & Smith, 1988; Kaminski & Marvell, 2002; Marvell & Moody, 1997b, 1998, 1999), whereas state panel studies of imprisonment generally find more moderate impacts (e.g., Levitt, 1996; Marvell & Moody, 1994).

Although previous research on the prison population-crime relationship tells us something about the deterrent/incapacitative effects of imprisonment—what happens when large numbers of prisoners are locked up—they do not explicitly address the question of what happens to the crime rate when large numbers of ex-prisoners, many of whom have served lengthy sentences, are released back into affected communities. At least 95% of all state prisoners are eventually released, and the numbers leaving continue to grow each year (BJS, 2003). A projected 595,000 state inmates were released from state prisons in 2001, a 41% increase over the 405,400 inmates released in 1990 (BJS, 2003).

The question we address is: Does the release of prisoners affect crime independently of the impact that the releases have on prison population? We wish to distinguish the direct impact of releases from the impact through their affect on prison population size. Because prison populations seldom decline, we are essentially asking whether released offenders commit more crime than the criminals remaining in or entering prison would have committed if they were not imprisoned.

There are numerous theoretical arguments for why release rates might affect crime directly. The arguments cut both ways. Those released have graduated from prison "crime school" and, thus, might be more motivated and able criminals than those entering prison (e.g., Tonry & Petersilia, 1999). This possibility is made more likely by the declining interest in rehabilitation over the past 30 years (Feeley & Simon, 1992; Zimring & Hawkins, 1991). The decrease in prison programs produces a larger number of prisoners returning to society not having participated in educational, vocational, and prerelease programs (Lynch & Sabol, 2001). Released prisoners, because of their status as former prisoners, are likely to find legitimate employment difficult, encouraging them to return to crime (Tonry &

Petersilia, 1999). Because of such concerns, some have argued that the impact of the half-million returning felons may be so great as to erode the previous decade's gains with respect to declining crime rates (Lynch & Sabol, 2001; Petersilia, 2000). Homicide rates have started to increase, up 2.5% nationwide from 2000 to 2001 (Federal Bureau of Investigation [FBI], 2002), and many have speculated that the volume of prison releases may be partly responsible (Abramsky, 1999; Lynch & Sabol, 2001; Rashbaum, 2002). For example, Winship (2002) suggested that the recent increase in homicides in Boston, after years of decline, might be due to the large increase in the number of felons, most of whom are unprepared for life outside, returning to the community from prison.

On the other side are arguments why the growing volume of released prisoners should not affect crime, or might even reduce it. The issue here, again, is whether the growing volume of prison releases has an impact on crime rates independent of the impact on prison populations, and this requires us to compare prisoners released to those remaining incarcerated. More than three fourths of releasees are on parole or otherwise supervised after release (Petersilia, 2000). They are watched loosely by parole officers, and they typically have restrictions on their activities, such as traveling and associating with other criminals. This, however, is not a strong argument that prison releases lead to less crime because incoming prisoners, if they were not incarcerated, would probably be under probation, where the restrictions are similar.

Another argument is that imprisonment is likely to enhance individual deterrence. Having experienced prison, those released might be especially weary of being imprisoned again. Their past criminal history would likely lead to more severe sentences than criminals who had not previously received prison sentences. A possible counterargument is that the power of prison to deter is predicated, in part, on the "fear of the unknown" (Clear 1996; Mauer, 1999), such that as more people acquire knowledge of prison life, the power to deter crime through this fear might be diminished (Clear, 1996). Furthermore, researchers have suggested that the stigmatization of prison is lessened when a high percentage of a particular community is incarcerated (Clear, 1996; Cook, 1998; Mauer, 1999).

Finally, it is likely that prison officials release less active and less dangerous criminals than the remaining prisoners. Those released tend to have shorter sentences, and they are often released early because of good behavior. According to BJS (2001), 65% of state prisoners to be released by year-end 1999 had one or no prior incarcerations. Only 25% were violent offenders.

Therefore, theory does not give us an a priori hypothesis concerning the existence and direction of an impact of prison releases on crime rates. We explored the issue by regressing homicide rates on prison release rates, using state-level panel data from 1975 to 1999. To our knowledge, no studies have estimated the impact of prison releases on crime. Although several individual-level studies have examined the rearrest and reimprisonment rates of released offenders, these studies cannot accurately measure the number of crimes committed by released offenders, and they cannot estimate whether the released offenders commit more crime than would offenders who remain in prison or who take their place as newly admitted prisoners.[1] That requires analysis of prison populations generally. As a result, research examining the link between prison releases and homicide rates must be done at the aggregate level.

DATA

The current study examined the impact of prison releases on homicide rates using state panel data for the period 1975 to 1999.

Homicide Rates

The dependent variable is homicides per 100,000 population. Homicide is the most serious and reliably measured of all crimes. Data from 1975 to 1999 for each state were taken from the FBI's "Crime in the United States" report (1976-2000). The FBI (2002) estimated homicide counts for agencies providing incomplete data by extrapolating homicide experiences of similar areas within the state and assigning the same proportional homicide volumes to nonreporting agencies.

Prison Releases

The primary independent variable is the prison release rate per 100,000 population. The prison release variable is operationalized as the total number of inmates released back into society during the calendar year that had been sentenced to terms of more than a year. Calendar year data are available for 1975 through 1999. Prison release data for 1975 to 1998 were taken from BJS (1976-2002). Unpublished prison release data for 1999 were made available to us by BJS.

The prison release data, according to the definition set by BJS, are supposed to be releases of prisoners who are under prison jurisdiction and who

were originally sentenced to more than a year. A problem is that several states changed their definitions of releases to comply with the BJS standard. These changes can affect prison release data. For example, a state adopting a jurisdiction count of prisoners would start including prisoners held in local jails because of prison overcrowding, which would artificially increase the number released. We strove to delete data when definitional changes are major. To do this, we subtracted the difference between prison admissions and releases from the difference between prison populations in the current and prior years. If reporting is consistent, the result is zero, however that is sometimes not the case. Most discrepancies are small, probably because of recounting. However when the difference reached 10% of the prison population, we considered it a problem that needed to be addressed. Usually, these occurred during the early years, and we included the state but used data only after the problems occurred (data used for Alabama starts with 1981, Alaska 1978, Delaware 1978, Kentucky 1988, Louisiana 1977, Mississippi 1981, Missouri 1975, Montana 1980, Nebraska 1982, New Jersey 1988, New Mexico 1981, Oklahoma 1978, Pennsylvania 1978, Tennessee 1989, Vermont 1978, and Virginia 1980). Four states (Connecticut, Oregon, Texas, and West Virginia) were deleted from the analysis because problems continued until recent years. In addition, the Alaska 1994 data are missing, and we estimated it to be the average of 1993 and 1995 numbers.

Prison releases include inmates released unconditionally or conditionally, inmates out on appeal or bond, escapees and AWOL prisoners, and those released back into society for unspecified reasons. Unconditional releases are mainly due to expiration of sentence but also include inmates who were released from prison due to court order, good time, commutation, or pardon. Conditional releases are primarily inmates released to parole, intensive supervision, work releases, or some form of community corrections program. Because our concern is with the impact of prisoners released back into society, the prison release counts do not include prisoners who died or those transferred to other jurisdictions.

Control Variables

In addition to the year dummies, state dummies, and state-trend variables (discussed below), we included numerous control variables that theory and prior research suggest are causally antecedent to homicide rates and punishment levels. Failing to control for factors that have opposite or same sign effects on prison releases and homicide rates could suppress (i.e., mask any

positive impact of prison releases on homicide rates) or lead to spurious or partially spurious results for the prison release variable, respectively.

The first group of control variables we entered into the homicide model is age-structure variables. Age structure is important because it affects prison releases and homicide rates in a positive direction, thus, failing to control for changes in age structure could lead to spurious results for the prison release variable. The age-structure variables include the percentage of the population ages 15 to 19 years, 20 to 24 years, and 25 to 29 years. These age groups are consistently those with the highest homicide arrest rates (FBI, 2002). This suggests that homicide rates should increase as these age cohorts grow, although many studies do not support that hypothesis (see, Land, McCall, & Cohen, 1990; Marvell & Moody, 1991). Age structure is also an important determinant of prison population. Marvell and Moody (1997a) found that age structure, especially the 25 to 34 age group, is positively related to prison population. Such findings are in accord with data on the age of inmates in prison (BJS, 2001, pp. 10-11). Because a state's age structure is likely to affect the size of its prison population, it is also likely to affect its prison release rates because states with greater numbers of younger persons have a larger pool of prisoners eligible to be released from prison. Consequently, one might expect prison release rates to increase as the number of individuals in these age groups increases. Age group data were taken from the U.S. Census Bureau (2003a).

Similar arguments can be made for other demographic variables, including race and urbanity. However, it is difficult to include these variables because they change little from year to year in a state and, thus, are collinear with state effects (which are discussed later).

The second set of variables control for changes in economic trends, which numerous macrostructural theories and prior research suggest are related, in one way or another, to punishment levels and homicide rates (Chiricos & Delone, 1992; Greenberg & West, 2001; Land et al., 1990). Variables controlling for economic trends are the poverty rate, unemployment rate, real per-capita income (divided by the consumer price index), and real welfare payments.[2] A number of criminological theories including strain/deprivation, social disorganization, Marxist theory, and macrostructural theory contend that economic distress has a positive impact on homicide, and extant research provides support for the effects of economic deprivation on homicide rates (see reviews in Chiricos, 1987; Land et al., 1990; Vieraitis, 2000). The underlying theme in strain/deprivation theory is that individuals lacking legitimate or limited opportunities for economic gain may become frustrated by their inability to attain, through lawful

means, the material goods that others around them possess. This frustration or strain, which is often accompanied by feelings of injustice and resentment, could manifest itself in the form of expressive violence, as those in the lower class respond to the unfulfilled expectations of justice and equity or instrumental violence, as individuals attempt to acquire the material goods they have been unable to attain through legitimate means (Bernard, 1990; Messner & Rosenfeld, 2000). For social disorganization theory, adverse economic conditions cause crime indirectly by weakening networks of informal social control, and diminishing a community's ability to regulate its members and to solve crime problems (Kornhauser, 1978; Sampson, 1986).

With respect to the connection between prisoners and economic conditions, the basic argument is that prisons are an effective way to manage populations (e.g., unemployed and marginal workers) perceived as threatening during economic downturns, and the business community will devalue potential laborers when unemployment rates are high (Cappell & Sykes, 1991; Hale, 1989; Parker & Horwitz, 1986; Rusche & Kirchheimer, 1939; Sabol, 1989; Speiglman, 1977). Others have suggested that judges may become frightened because of threats made by unemployed workers (e.g., Greenberg, 1977) or consider them a greater risk for returning to crime (Box, 1987) and, therefore, sentence them more harshly. Thus, extant theory suggests that prison administrators and parole boards release fewer prison inmates during economic downturns. (On the other hand, it is possible that budget restraints lead to more releases during economic downturns.) In sum, these considerations lead to the prediction that economic trends are positively related to prison releases and negatively related to homicide rates. Thus, failing to control for changes in economic trends might suppress any positive impact of prison releases on homicide rates.

Finally, we enter prison population in the homicide models.[3] There might be a positive association between homicides and prison releases simply because releases decrease the number of prisoners and, thus, reduce the incapacitation impact. In addition, prison population is likely to affect prison releases and homicide rates in the opposite direction, thus, failing to control for it might suppress any positive prison release-homicide relationship. As noted above, studies generally find a negative relationship between prison population and homicide rates. As for prison releases, states with higher incarceration rates have higher prison release rates because of the greater number of inmates eligible to be released from prison. Indeed, in a regression similar to that in Table 2, with prison releases as the dependent variable and prison population as the primary independent variable, we

found prison population to have a strong positive association with prison releases $b = .230$, $t = 4.17$).

METHOD

To assess the influence of prison releases on homicide rates, we use a time-series cross-section (TSCS) design, which has become increasingly popular in the past decade. We use data for 46 states over 25 years. The major benefit of this design is the ability to add numerous control variables, including state and year dummies as discussed below. In addition, the high number of degrees of freedom provides for greater statistical power and thus makes it possible to detect even modest effects of prison releases on homicide rates.

We follow conventional strategies for TSCS data and estimate a fixed-effects model, which requires adding dummy variables for each state and year (Hsiao, 1986, pp. 41-58; Pindyck & Rubinfeld, 1991, pp. 224-226). The fixed effects are partial controls for missing variables. The state dummies control for the collective effect of unobserved state-specific factors that do not greatly trend upward or downward over the 25-year period. Likely examples are climate, race, urbanity, relative economic deprivation, gun ownership, and deeply embedded cultural and social norms. State dummies also help control for differences in crime reporting procedures. Coefficients are based solely on within-state changes over time, and cross-section variation is not used; among other things, this lessens the risk of simultaneity bias.

Year dummies control for national events that raise or lower homicide rates in a given year across the country. A likely example is the crack epidemic starting in the mid 1980s (Blumstein, 1995). Finally, we add separate state-specific time-trend variables for each state. These are coded zero for all observations except in the particular state, where it is a simple counter. These control for consistent trends in a state that depart from the national trends captured by the year dummies. An example might be the number of young men who grew up without fathers.

As in most TSCS studies, the substantive variables are divided by population and converted to natural logs. Logging reduces the impact of outliers and converts coefficients into elasticities—the percentage change in the homicide rate expected from a 1% change in prison releases (Greene, 1993). Heteroscedasticity is a problem because variation in homicide rates is greater over time in smaller states. Hence, we weighted the regressions by state population, the proper weight as determined by the Breusch-Pagan

Table 1: Descriptive Statistics

Variable	*Description*	M	SD
CRMUR	Homicides per 100,000 population	6.78	3.78
PRDTX	Prison releases per 10,000 population	10.56	6.60
P1519	Percentage aged 15 to 19 years	8.08	1.19
P2024	Percentage aged 20 to 24 years	8.15	1.23
P2534	Percentage aged 25 to 34 years	16.02	1.78
PRCYEZ	Prison population per 10,000 population	21.94	12.71
RTPI	Real personal income per 100 persons	45.28	7.78
UNRATE	Unemployment rate	6.12	2.06
PRATE	Poverty rate	12.88	3.94
RWEL	Real welfare per 1,000 persons	57.25	21.36

Test (Breusch & Pagan, 1979). Panel unit root tests (Levin & Lin, 1992; Wu, 1996) indicated that homicide and prison releases are stationary, suggesting that the analysis be conducted in levels, rather than in differences. Autocorrelation is mitigated by including the homicide rate lagged 1 and 2 years (Hendry, 1995); these variables have the added benefit of controlling for omitted lagged factors that affect homicide rates in the current year. Examination of collinearity diagnostics (Belsley, Kuh, & Welsh, 1980) revealed no collinearity problems for the prison release variable. Perfect collinearity among proxy variables was avoided by dropping one year dummy, one state dummy, and one state-trend variable. Estimation was carried out in SAS, version 8.2.

Table 1 lists homicide rates and the substantive independent variables, along with their means and standard variations (the standard deviation reflects variation between states, whereas only within-state variation is used in the analysis). Again, the additional control variables are homicide rates lagged 1 and 2 years, year dummies, state dummies, and individual state trends. All variables except the year and state dummies vary between states and between years. The data and the programs used here are available on the Internet at http://mmarvell.com/justec.html.

FINDINGS

Table 2 presents the homicide results, using regression procedures described above. After controlling for prison population and other factors, the basic finding in Table 2 is that prison releases appear to have no

Table 2: The Impact of Prison Releases and Other Structural Covariates on Homicide Rates: Evidence From State Panel Data

	Dependent Variable: Natural Lag of the Homicide Rate per 100,000 Population	
Independent Variables	*Coefficient*	t *Ratio*
Prison releases	.018	0.70
Population aged 15 to 19 years	.214	1.05
Population aged 20 to 24 years	.562	3.79*
Population aged 25 to 29 years	.207	.72
Prison Population	–.159	–3.31*
Real personal income	.261	1.07
Unemployment rate	–.080	–2.34*
Poverty rate	–.004	–.11
Real welfare payments	.051	.92
Homicide rate, one-year lag	.313	9.99*
Homicide rate, two-year lag	.177	5.61*
Adjusted R^2 = .95		
df = 960		

Note: The homicide regression encompasses 46 states from 1975 to 1999. Not shown are year dummies, state dummies, and state trends. The prison release variable is the total number of sentenced prisoners released to society in each state during a calendar year. The regressions are weighted by population to the .9 power. All continuous variables are divided by population and logged.
*p < .05. (two-tailed).

significant relationship with homicide. Although the coefficient for the prison release variable is in the expected positive direction, the coefficient is small in magnitude, and it is far from being statistically significant. Given the large number of degrees of freedom, any meaningful impact of prison releases on homicide rates should have produced a significant coefficient. We also found no evidence that prison releases affected homicide when we reran the regressions in Table 2 using differenced variables. Again, the coefficient for the prison release variable was weak and statistically nonsignificant, and in the unexpected negative direction (b = –.015, t = –.50). In sum, the hypothesized positive relationship between prison releases and homicide is not realized in these data.

The control variables present a number of interesting results. The prison population variable has a large negative coefficient, again a result often found in other research. Coupled with the nonsignificant coefficient for the prison release variable, this suggests that those who remain in prison, or

those recently admitted to prison, are at least as likely to commit murder as those recently released. As discussed above, because the continuous variables are expressed in logarithms, the coefficients are elasticities. However, the coefficient underestimates the full impact on homicide rates because of the inclusion of lagged dependent variables. To estimate the full, long-term elasticity, we follow Hamilton's (1994) recommendation of multiplying the coefficient for the prison population variable by the reciprocal of one minus the coefficients on the lagged dependent variables. Adjusting the coefficient for the prison population variable produced a long-term elasticity of .312 [(–.159)/(1 – .313 – .177)]; that is, for each 10% increase in a state's prison population, homicides decrease, on the average, by about 3.1% in the state. The number of homicides reduced for each additional prisoner can be estimated by multiplying the elasticity by homicides per prison population. This can be done two ways. Taken at the means, the homicide rates and imprisonment rates (per 100,000) population are 6.8 and 219.4, respectively (see Table 1). Thus, historically, for every 100 additional prisoners, there has been one less homicide. Second, using current prison population figures, 470 prisoners per 100,000 persons, the benefit drops to one less homicide for roughly every 200 additional prisoners. It is important to note, however, that these estimates are averages across all states, and the impact probably varies between states. In addition, a state-level study such as the current study cannot capture the full effect of incapacitation on homicide because it implicitly assumes that criminals do not move across state lines; that is, imprisoning a criminal in one state prohibits him or her from committing murder in other states as well (see Marvell & Moody, 1998).

Age groups have the predicted relationship to homicides, however the economic variables, with the exception of unemployment, show little or no impact. Unemployment is negatively associated with homicide, perhaps because of the opportunity effects of unemployment as postulated by Cantor and Land (1985). The authors argued that maximum opportunity effects are likely to be contemporaneous and most responsive to levels of unemployment, as is the finding here.

One possible explanation for the weak and nonsignificant coefficient obtained for the prison release variable is simultaneity, which can happen if state justice systems respond to homicide growth by curtailing the number of inmates released from prison. In such a situation, the coefficient on the prison release variable is biased in the negative direction, thereby negating any positive impact of prison releases on homicide rates. We explored this possibility by using the Granger causality test, in which the prison release variable becomes the dependent variable, and it is regressed on 1- and 2-

year lags of homicide (as well as 1- and 2-year lags of itself; Granger, 1969; Pindyck & Rubinfeld, 1991). Reverse causation is indicated when the lags of homicide are significant. The Granger test has a drawback in that it misses purely contemporaneous causation. In the current situation, however, this is not a problem. It can be assumed that if homicide rates have a contemporaneous impact on the number of inmates released from prison, homicides must also have a 1-year lagged impact on releases. The reason is that it takes time for policy makers and prison officials to learn of changes in homicide trends, provide funds to incarcerate offenders for longer periods of time, and change standards inmates must meet to be eligible for release. In addition, because we conduct the Granger test in levels (as opposed to differences), any contemporaneous causation would be reflected in the 1-year lag because of serial correlation (correlation between current and prior year homicides). For these reasons, therefore, the absence of a 1-year lagged impact of homicides on the prison release variable implies the absence of an immediate impact. The results of the Granger test show no evidence of simultaneity. The lagged homicide variables in the prison release regression are far from significant, small in size, and in the unexpected positive direction (homicide, 1-year lag, $b = .019$, $t = .74$; 2-year lag, $b = .054$, $t = 2.17$). Thus, there is no evidence that state policy makers and prison officials respond to increases in homicide rates by reducing prison releases.

CONCLUSION

The purpose of the current study was to examine the impact of increased prison releases on homicide rates independent of their impact on prison populations. As noted above, because state prison populations have generally remained steady or increased over the past 2 decades, the basic question addressed by this research was whether individuals admitted to prison in any given year or those who remained in prison were more or less criminogenic than those released by prison officials. Using panel data for 46 states from 1975 to 1999, we found virtually no evidence of an association between prison releases and homicide rates. It is important to note, however, that we only estimated the impact of prison releases on homicide rates, and it is possible that releases have beneficial or detrimental effects on other crimes. This possibility is left for future research. It is also possible that prison releases do have some effects on homicide, however they are too small to be statistically detectable. Given our large sample size, however, the effects would have to be small indeed to avoid detection.

Higher prison release rates may have a homicide-elevating effect, however one that is counterbalanced by homicide-reducing effects. As noted earlier, prison releases might increase homicide rates as offenders become more motivated or skilled in crime during their prison terms or motivated on release as their status as former prisoners and lack of skills makes securing legitimate employment difficult, while also reducing homicides as prison officials release less violence-prone criminals to make room for more active and dangerous criminals, with the two opposite sign effects canceling each other out.

The current results also indicate a strong negative relationship between prison population and homicide. This suggests that criminals remaining in prison are at least as active and dangerous as those released. The main policy implication of this research is that the release of prisoners, necessitated by the large increase in prison populations, is not in itself causing more homicides. This finding does not imply that we should abandon other strategies to reduce homicides such as programs to reduce poverty, however it does reinforce earlier findings that prison expansion has been more effective at reducing homicide than many competing alternatives, and it is probably a major reason why homicide rates declined over the past decade.

NOTES

1. Researchers have also begun to explore the "collateral consequences" of the "imprisonment binge" and the subsequent release of prisoners after lengthy incarcerations (Mauer & Chesney-Lind, 2002; Petersilia, 2000). These consequences include the disproportionate use of incarceration on minority males (Chaiken, 2000; Mauer, 1999; Western, Pettit, & Guetzkow, 2002), its impact on family and social structures in heavily affected neighborhoods (e.g., Rose & Clear, 1998), and the political disenfranchisement of current and former felons (Chaiken, 2000; Mauer, 2002).

2. Poverty data were obtained from the U.S. Census Bureau (2003b). Data on state-level unemployment were taken from the Bureau of Labor Statistics (2003). Data on personal income, real welfare payments, military employment, and construction employment were obtained from the Bureau of Economic Analysis (2003).

3. For the source of these data see Marvell and Moody (1998). The data are for the end of the year, and we estimate the prison population over the year by averaging the current and prior year numbers. There is a chance that the prison population variable induces simultaneity bias; that is, homicide rates might affect incarceration rates. However, this is unlikely to be the case because murderers make up only a small proportion of the overall prison population. Supporting this conclusion, Marvell and Moody (1994, 1997b) found no evidence of simultaneity between homicide rates and state prison populations using a Granger causality test. Deleting prison population from the homicide models does not substantively impact the results presented in Table 2.

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