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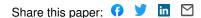**Institutions:** University of Alabama at Birmingham

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THE EFFECT OF COUNTY-LEVEL PRISON POPULATION GROWTH ON CRIME RATES*

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Research Summary:

Prior macro-level studies examining the impact of prison population growth on crime rates have produced widely varying results. Studies using national-level time series data find large impacts of prison growth on crime, whereas those using state panel data find more modest ones. Critics of the former studies maintain that the estimates are implausibly large, arguing that the effects are instead due to analysts' inability to control for potential confounding factors. Conversely, critics of the latter studies argue that they underestimate the total impacts of imprisonment by failing to account for potential free-riding effects. This study uses panel data for 58 Florida counties for 1980 to 2000 to reexamine the link between prison population growth and crime. Unlike previous studies, we find no evidence that increases in prison population growth covary with decreases in crime rates.

Policy Implications:

Our findings suggest that Florida policymakers carefully weigh the costs and benefits of their continued reliance on mass incarceration against the potential costs and benefits of alternatives. If the costs of mass incarceration do not return appreciable benefits, i.e., a reduction in crime, it is time to reconsider our approach to crime and punishment. Other research offers evidence of crime prevention programs operating inside the criminal justice system and in communities that hold promise for reducing crime; our findings indicate that policymakers carefully consider these options as a way to achieve their goals.

KEYWORDS: Prison Population, Mass Incarceration, Crime

Over the past 25 years, the United States has witnessed an unprecedented "imprisonment boom" as it has increasingly relied on a strategy of building and filling prisons to reduce crime (Austin and Irwin, 2001; Mauer, 1999; Walker, 2001). Between 1980 and 2000, the total number of

prisoners (serving sentences of more than one year) under federal or state jurisdiction soared from 315,956 to 1,329,367—a 321% increase (U.S. Bureau of Justice Statistics, 2005). The incarceration rate has more than tripled during the same time period. As of 2000, the United States had the highest incarceration rate of all industrialized nations (Stern, 2002). The rise in prison populations has been accompanied by equally large increases in correctional expenditures. In 2001, government outlays on corrections exceeded $60 billion (including intergovernmental transactions), a 531% increase from the nearly $10 billion spent in 1982 (U.S. Bureau of Justice Statistics, 2004a:2).

Support for the "incarceration reduces crime thesis" is largely based on the "incapacitation effect" principle, which simply states that criminals who commit crimes cannot do so from prison or jail, at least not in the general public (Greenwood, 1982; Kraska, 2004; Moore et al., 1984). The supposition that increases in imprisonment can deter crime is also supported by classical and neoclassical theories of crime. Both theories suggest that because criminals are rational, utility-maximizing individuals, they will be less likely to engage in criminal behavior if the perceived costs of crime outweigh the perceived benefits gained from committing crime (Becker, 1968). Thus, increases in imprisonment may lead to reductions in crime by raising the expected costs associated with crimes, due to criminals fearing an increased chance of being imprisoned and longer prison terms.

PREVIOUS STUDIES

Researchers have examined the prison-crime link using a menagerie of methodological approaches. Earlier studies attempted to estimate the impact of incarceration by computing lambda (the annual number of crimes an offender would have committed if on the street) using arrest records of individual offenders (e.g., Blumstein and Cohen, 1979) and prisoners' self-reports of criminal activity (e.g., Spelman, 1994; Zedlewski, 1987). The lambda approach to estimating incapacitation effects has produced disparate results ranging from 3 index crimes averted per year (Greenberg, 1975) to 187 nondrug crimes averted (Zedlewski, 1987). After making numerous adjustments to the lambdas derived in past research, Marvell and Moody (1994) report, on average, that 16 to 25 index crimes were averted each year for each additional prisoner.

The problems with computing lambda and its limitations for estimating imprisonment effects, even if a correct lambda could be obtained, have been widely documented (see Marvell and Moody, 1994 for a review of the lambda literature). As a result, researchers have largely abandoned the use of lambda in favor of regression analysis, which is considered a more

fruitful approach for estimating the crime-reduction effects of prison population growth (e.g., Levitt, 1996; Marvell and Moody, 1994, 1997a, 1998).

Estimates of the effects of prison population growth on crime rates using regression analysis have been based on either national-level time series data or state-level panel data. Most regression analyses of national-level time series data find large negative impacts of incarceration on homicide rates indicating support for the more prison, less crime hypothesis (e.g., Cohen and Land, 1987; Devine et al., 1988; Marvell and Moody, 1997a, 1998). For example, Devine et al. (1988) and Marvell and Moody (1997a) regressed homicide rates on prison population using national-level data from 1948 to 1985 and 1929 to 1994, respectively. Devine and his colleagues obtained homicide elasticities for the prison population variable ranging from –1.47 to –1.88, depending on the particular model specification, whereas Marvell and Moody (1997a) obtained an elasticity of –1.33. That is, for each 1% increase in prison population, homicide rates declined by roughly 1.47% to 1.88% and 1.33%, respectively. Both studies also found large negative associations between prison population and other crimes; however, as discussed below, their estimated crime reductions are implausibly large; indeed they are so high that some scholars have dismissed them as not being reasonably interpretable (see Levitt, 2001; Spelman, 2000).

State panel studies of imprisonment generally find more modest crime-reduction effects (DeFina and Arvanites, 2002; Marvell and Moody, 1994; Levitt, 1996). Marvell and Moody (1994) regressed state homicide rates on state prison population and controls for changes in age structure using data for 49 states from 1971 to 1989. The authors estimated that roughly 17 index crimes were averted each year for each additional prisoner. In a similar study, Levitt's (1996) results suggested a somewhat greater impact of prison population on crime rates than Marvell and Moody (1994), although not nearly as great as the impact found by Devine et al. (1988) and Marvell and Moody (1997a) with national-level data.[1] Levitt (1996) attributed the greater impact to the use of more appropriate statistical procedures, which produced prison population coefficients that were not affected by simultaneity bias (i.e., more crime may lead to more prisoners and mask any negative effects of prison on crime).[2]

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1.   The main difference between the two state panel studies is that Levitt extends the time period studied through 1993, controls for more potential confounding factors, and uses instrumental variables regression with prison litigation as an excluded instrumental variable to mitigate simultaneity problems between prison populations and crime.

2.   Levitt (1996) obtained elasticities roughly consistent with those obtained by Marvell and Moody (1994) when using OLS regression and not instrumenting for prison population. Marvell and Moody (1994) found no evidence that changes in the

Marvell and Moody (1997a, 1998) suggest the reason for such divergent results between national-level and state-level series studies is that the latter underestimate the full effects of incapacitation because of free-riding effects. Specifically, Marvell and Moody (1997a) argue that state-level studies necessarily assume there is little or no inter-state offending, and thus they ignore the fact that a criminal imprisoned in one state cannot commit crimes in other states. Thus, a state like Florida, for example, which has not increased its incarceration rate as much as other states, could nevertheless be benefiting from the rampant growth in out-of-state prison populations if a non-negligent share of criminals, had they not been imprisoned, moved to Florida. In such a scenario, the state of Florida would be considered a "free-rider," enjoying a crime-reduction impact from out-of-state imprisonment at no cost to itself.

To examine the "free-rider" effect hypothesis, Marvell and Moody (1998) regressed in-state homicide rates on out-of-state and in-state prison population and numerous control variables using state-level data from 1929 to 1992. A separate time-series regression was estimated for each of the 48 lower states. As the authors had predicted, out-of-state prison population had a larger negative impact on in-state homicide rates (elasticity = -0.76) than in-state prison population (elasticity = -0.22). Similar results were obtained when states were aggregated into regions; i.e., out-of-region prison population had a greater impact on in-region homicide than in-region prison, and when studying other UCR index crimes. They concluded that free-riding effects accounted for the widely varying estimates between national time-series and state panel studies and that criminals cross state lines more than most scholars probably believe. However, based on very consistent *a priori* empirical evidence at the individual level, out-of-state prison population could not reasonably have been expected to reduce crime by three times as much as the in-state prison population.

Over the past three decades, studies on criminal mobility of arrested offenders revealed that most offenders committed a large share of their offenses close to home (e.g., Baldwin and Bottoms, 1976; Brantingham and Brantingham, 1981; Bullock, 1955; Wright and Decker, 1997). Moreover, research by Orsagh (1992), for example, reported that out-of-state offenders accounted for, on average, only 31% of those arrested for homicide and approximately 25% of those arrested for other crimes. The most recent examination of criminal migration by the U.S. Bureau of Justice Statistics (2002) revealed that only 7.6% of prisoners rearrested for a new offense (within three years of their release) were rearrested in a state

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crime rate have short-term impacts on prison populations using the Granger causality test. Thus, for estimating purposes, the authors treated prison population as an exogenous variable in the crime equations.

other than the one that released them. The potential impact of out-of-state prison population is, however, weaker than even the 7.6% figure implies, because a subset of these offenders (3.9%) were also rearrested in the state that released them.

Moreover, a large share of this inter-state movement may reflect nothing more than criminals switching their state of residence. From 1985 to 1990, 9.4% of the U.S. population moved their residence across state lines (U.S. Bureau of the Census, 1994:19). As former in-state arrests remain a part of an offender's official criminal record when they move across state lines, it means that any tracing of prior arrests after a new arrest would indeed show up as an "out-of-state" arrest, but not because any of them were engaged in inter-state offending. In this case, criminals switching their state of residence would be responsible for their out-of-state arrest records.

If moving out of state is as common among criminals as noncriminals, then about 9.4% of the arrestee population of approximately 20 million persons also moves across state lines in any five-year period, or roughly 2 million arrestees.[3] Over a longer period of time, a still larger share of arrestees would have moved across state lines, even without any involvement in inter-state offending. For example, if a different 9.4% of the arrestee population moved their residences across state lines every five years, about half of the U.S. arrestee population would have moved across state lines in just 25 years. Consequently, even though inter-jurisdictional offenders are more likely to have out-of-state arrest records, the share of criminals that have out-of-state arrests should not be viewed as even a rough indicator of the frequency of involvement of criminals engaged in inter-jurisdictional offending. In sum, one should expect a 1% increase in out-of-state prison population to have, at most, one fourth the impact on in-state crime than a 1% increase in in-state prison population, not the other way around, as was reported by Marvell and Moody (1998).[4]

Another important finding of the U.S. Bureau of Justice Statistics study was that inter-state criminals did not seem to be more criminally active than those only rearrested in the state that released them. The average number of new charges for inter-state criminals was slightly over 3, about

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3. To estimate the size of the adult arrestee population, we relied on Tillman's (1987) estimated adult arrest prevalence of 10.5% for index crimes. This figure was then multiplied by the number of persons 18 years and older in 1990 (mid-point of study period). The arrestee population figure reported is probably a lower bound estimate because Tillman (1987) only studied the prevalence of arrest for a 12-year period (aged 18 to 29 years), and we did not include arrests for non-index crimes.

4. This figure is based on Orsagh's (1992) finding that out-of-state offenders accounted for approximately 25% of all arrests for all crimes.

one less than the average number of charges for intra-state criminals (personal communication from M. Durose, March 2, 2005). This finding is crucial because one proposition underlying the free-rider hypothesis is that mobile criminals are more active than more common local criminals; the U.S. Bureau of Justice Statistics rearrest data suggest that this is not the case.

Perhaps the most damaging evidence to the free-riding hypothesis crime can be found in Marvell and Moody's (1998) findings. According to the U.S. Bureau of Justice Statistics (2002) report on released prisoners, the two states that experienced the largest number of crimes from out-of-state criminals were New York and Arizona. If New York and Arizona are important free-rider states, as the U.S. Bureau of Justice Statistics rearrest data would suggest, then one would have expected the coefficients for the out-of-state prison population variable for these states to have been some of the largest. As seen in Table 1 of Marvell and Moody (1998), the coefficients on the out-of-state prison population variable in the New York and Arizona analyses were in the expected negative direction, but they were not even close to being statistically significant.[5] Thus, it seems that even in popular destination point states like New York and Arizona, there is little evidence of substantial free-riding effects taking place.

We agree with Spelman (2000) and Levitt (2001) that a more plausible explanation for the large differences obtained between national/state time-series and state-level panel studies is that the former suffer from omitted variable bias. Because national/state time-series studies have a limited number of degrees of freedom, and most crime-relevant variables are not measured (or are not easily measured) between census years, it is not possible to control for a large number of potential confounding factors. Spelman (2000) suggests that the national prison population variable in the 48 state time-series regressions by Marvell and Moody (1998) may have served as a proxy for one or more unobserved (or difficult to measure) factors that were inherently national in character. He offered two plausible explanations: (1) aggressive policing strategies, and (2) civilians' increased willingness to prevent crime on their own. We suggest that the national prison population may also have served as a surrogate for other as-yet unmeasured factors such as the stabilization of crack-cocaine markets (Blumstein, 1995), the legalization of abortion (Donohue and Levitt, 2001), increased support for traditional and nontraditional social institutions (LaFree, 1998), changes in labor markets (Grogger, 2000), increased community participation in anti-crime efforts (Friedman, 1998), decreases

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5. Conversely, states like Delaware, Nebraska, and South Dakota, which had very few out-of-state rearrests, actually had some of the largest out-of-state prison population coefficients.

in alcohol consumption (Parker and Cartmill, 1998), and increased expenditures on private security.[6] To the extent that any of these factors are at least partially positively correlated with changes in national prison populations and negatively correlated with crime rates, analysts regressing national crime rates on national prison populations (e.g., Devine et al., 1988) [or state crime on national prison populations (e.g., Marvell and Moody, 1998)] would be left with the false impression that increases in national prison populations are entirely responsible for decreases in national (or state) crime; i.e., the association between out-of-state prison population and crime would be at least partially spurious.

State panel studies, such as those discussed here, do not suffer from the inability of analysts to explicitly control for nationwide influences on crime because these forces are captured through the use of year dummy variables in a fixed-effects model. Indeed, one of the main advantages of state panel studies over more common national/state time-series studies is the analyst's ability to control for (1) unobserved heterogeneity across states (i.e., time invariant factors that cause crime rates to vary from state to state) and (2) unobserved time-varying factors that affect all states in a given year in the same fashion. The fixed-effects model is discussed more fully below.

Although state panel studies are undoubtedly a vast improvement over national time-series studies, we believe the use of states as the unit of analysis in prison-crime studies is also problematic. States are large aggregations and thus are more heterogeneous than smaller aggregations such as counties with regard to incarceration rates and crime rates. That is, state-level studies necessarily ignore the large amount of systematic variation (i.e., within counties) over time in incarceration rates and crime rates.[7] In Florida, for example, many counties have seen a lot of stability with regard to incarceration rates, whereas others have seen a lot of change. Between 1980 and 2000, the within-county standard deviation for the incarceration rate in Citrus, Clay, Collier, Flagler, Lee, Palm Beach, St. Johns, and Seminole counties ranges between 24.85 to 49.29, indicating little change in these counties with respect to their reliance on incarceration. On the other hand, Bay, Calhoun, Columbia, Dixie, Jefferson, Liberty, and Taylor counties all exhibited substantial variation in their incarceration rates during

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6. Marvell and Moody (1998) attempt to control for the crack epidemic of the mid-1980s and early 1990s using a step dummy variable, but as the authors readily admit, this is an extremely crude measure.

7. In a recent symposium on unemployment and crime (U-C), Levitt (2001) makes this very same argument, but with respect to national time-series studies of the U-C link. That is, Levitt claims that national time-series studies remove potentially important variation in unemployment and crime at the local level. We agree, but we believe the problem also extends to state-level studies of the prison–crime relationship.

the same study period with within-county standard deviations of 200 or higher. Simply summarizing the within-state standard deviation in the Florida incarceration rate over this time period as 121.64 averages out of existence all of this potentially useful within-county variation in prison levels.

Another benefit of using more a homogenous ecological unit is that it reduces the likelihood of within-unit variation, which can be a source of aggregation bias when larger, more heterogeneous units are used (Kleck and Chiricos, 2002). The greater such bias, the more problematic any inferences about individual-level behavior, such as inferring incapacitation/deterrence effects from prison population growth while using national- or state-level data.

The purpose of the research presented here is to revisit the relationship between prison populations and crime using regression procedures similar to those used in prior state panel studies, but with data aggregated to a more local level, the county level. As noted, the rationale for using counties as the unit of analysis is clear. Counties exhibit greater cross-temporal variability in both incarceration rates and crime rates, all of which would be squandered away in a national time series or state panel study.[8] By using county-level data, we are in a position to take advantage of the systematic variation in these variables over time. It is precisely this within-unit variance that prison population-crime research is trying to explain.

Specifically, we conduct a county-level panel-data analysis using annual crime and prison population data for 58 of Florida's 67 counties from 1980 to 2000.[9] Florida provides a perfect test site for reassessing the prison-crime link as it has witnessed similar changes in both prison populations and crime as the rest of the nation during the imprisonment boom of the 1980s and 1990s (Figure 1). As seen in Figure 1, during this time period Florida's prison population growth largely tracked the rate for the rest of the nation, although the rate of growth was somewhat greater for the latter. With respect to crime rates, both series have moved roughly in tandem over the past two decades: Crime rates declined in the early 1980s, then began rising in the mid-1980s, and have declined markedly throughout the 1990s. This pattern of results indicate that there may have been some broad forces operating that tended to push crime rates up and down in both Florida and the rest of the nation. Second, despite both Florida and the rest of the United States having experienced a sizeable drop in crime

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8. Between-unit variation in crime and prison populations is not relevant here because panel studies using a fixed-effects estimator rely solely on cross-temporal variation in crime and prison populations within each ecological unit (Wooldridge, 2000).

9. A decision was made to drop Franklin, Gilchrist, Glades, Gulf, Hamilton, Holmes, Jefferson, Lafayette, and Suwanee counties from the dataset because of incomplete crime reporting by law enforcement agencies within these counties.

rates throughout the 1990s, crime rates in Florida have declined slightly faster relative to the remaining states since the latter half (i.e., 1990s) of the imprisonment boom.

This similarity in trends is important because if a panel data analysis of Florida counties produces crime elasticities for incarceration rates significantly lower than those obtained using state panel data (e.g., Levitt, 1996; Marvell and Moody, 1994), it suggests the latter studies may also suffer from omitted-variable bias. That is, by aggregating the crime and prison data to the state level, these studies may have mistakenly attributed drops in crime to prison population growth that were really due to unmeasured state-level factors not explicitly controlled for in the fixed-effects regression model. Indeed, this is the same explanation used by Levitt (2001) and Spelman (2000) but with regard to the reason for the large differences between national time-series and state panel studies. In sum, if state-level data are considered more appropriate for crime studies than national-level data, then we see no sound theoretical or methodological reason for why county-level data would not be preferred over both national-level and state-level data.[10]

The next section explains at length the regression procedures and data used. The last two sections present and discuss the policy implications of our findings.

DATA AND METHODS

This study follows regression procedures for panel data similar to those used by Marvell and Moody (1994). As noted by Marvell and Moody (1994), panel data offer distinct advantages over more commonly used national time-series data. First, the most important advantage of panel data is the ability to enter dummy variables for each county and year, which mitigate omitted variable bias by controlling for overall county differences and for statewide yearly changes in crime rates. Second, the panel design provides for a larger sample size because for each of the 58 counties, there are 21 observations that allow us to control for a wide range of potential confounding factors but still retain a large number of degrees of freedom. Third, the high number of degrees of freedom provides for greater statistical power and thus makes it possible to detect more modest effects of prison population growth on crime (Wooldridge, 2000:409).

CRIME RATES

Crime is measured using the Uniform Crime Report (UCR) index

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10. To reiterate, we do not believe there is enough individual-level evidence of significant interstate movement of highly active criminals to warrant the use of larger, more heterogeneous units of analysis.

offenses for the period 1980–2000. The offenses include homicide (and non-negligent manslaughter), forcible rape, aggravated assault, robbery, burglary, larceny, and auto theft and are expressed in rates per 100,000 resident population. Crime data were obtained from the Florida Department of Law Enforcement on computer disk.

County-level crime data for 1988 are not available and are scored as missing data. Florida's incident-based reporting system was enhanced in 1988 to comply with changes in UCR program procedures. Many agencies did not convert reporting systems in time to comply with the new format, and thus, crime data for 1988 are incomplete (Florida Department of Law Enforcement, 1999). This procedure has the drawback of shortening the time series by one year, but the 20 years remaining is sufficient for a panel study.[11]

PRISON POPULATION

Prison population for each county/year is measured as the number of inmates who had been sentenced in the county to more than one year in prison, under state prison jurisdiction on June 30th and is expressed in rates per 100,000 resident population.[12] The county in which the offender was sentenced, regardless of where the offender was imprisoned, determines the county to which the inmate was assigned. The prison population data exclude inmates in local jail facilities and inmates imprisoned in other states or in federal facilities. The prison data start in 1980, the first year with data for county-level prison population as defined above. Prison data were obtained by the senior author from the Florida Department of Corrections, Bureau of Research and Data Analysis.

CONTROL VARIABLES

In addition to the year dummies and county dummies, we include numerous control variables that theory and prior research suggest are causally antecedent to both incarceration rates and crime rates. Failing to

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11.   As a check on the results for UCR homicide, and to see whether the inclusion of homicide data for 1988 makes a difference, we conducted a separate analysis using vital statistics counts of homicide victims derived from Centers of Disease Control and Prevention (CDC) Part III Mortality Detail Files, provided by Professor Gary Kleck. The homicide counts cover all 58 counties, but they are only available through 1998.

12.   One referee questioned the use of the overall incarceration rate, arguing instead that each crime model (e.g., burglary rate) be estimated with incarceration rates specific to each crime (e.g., incarceration rate for burglary). Such an estimation procedure would work against the prison–crime efficacy hypothesis because it mistakenly assumes that criminals specialize in particular crimes. Indeed, the results of offense specialization research has concluded that general offending trumps offense specialization (Blumstein et al., 1988; Osgood et al., 1988; Piquero, 2000).

control for factors that have same sign effects on incarceration rates and crime rates will lead to spurious findings for the prison population variable, whereas failing to control for factors that have opposite sign effects could suppress any negative impact of prison population growth on crime rates. The specific control variables included in the crime equations were the percent of the civilian labor force unemployed, real per-capita income, percent of the population divorced, percent of the population that is African American, percent of the population living below the poverty line, percent of households headed by women with children under the age of 18 years, and percent of the population aged 15 to 24 years, 25 to 34 years, and 35 to 44 years.[13]

The most important set of control variables we include in the crime equations are the variables controlling for economic deprivation, which numerous macrostructural theories and prior research suggest are related, in one way or another, to both punishment levels and crime (Chiricos and Delone, 1992; Greenberg and West, 2001; Land et al., 1990). Several criminological theories, including, strain/deprivation, social disorganization, and Marxist theory, maintain that economic downturns increase crime, and extant research provides support for the effects of economic deprivation on crime rates (see reviews in Chiricos, 1987; Land et al., 1990; and Vieraitis, 2000). With respect to punishment, 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 and Sykes, 1991; Hale, 1989; Parker and Horwitz, 1986; Rusche and Kirchheimer, 1939; Sabol, 1989; Speiglman, 1977). Others have suggested that judges may become frightened due to 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. These considerations lead to the prediction that economic downturns should be positively related to both prison population and crime rates. Thus, failing to control for changes in economic trends could suppress any negative impact of prison population on crime rates.

The age structure variables are also important because they are similarly related to both prison population and crime rates; thus, failing to control for changes in age-structure could suppress a negative relationship between prison population and homicide. The 15-to-24-years age group has one of the highest arrest rates for crime (Federal Bureau of Investigation, 2001:227), which suggests that crime should increase as the size of

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13.   Personal income data were converted from a current dollar estimate to a constant-dollar 1992 basis by dividing personal income by the consumer price index (CPI).

these age cohorts increase, although prior cross-sectional research gener-
ally finds no impact of age structure on crime (Land et al., 1990; Marvell
and Moody, 1991). Age structure is also an important determinant of
prison population. Prior research by Marvell and Moody (1997b), for
example, found that age structure, especially the 25-to-34-years age group,
was positively related to prison population. Such findings are in perfect
accord with data on the age of inmates in prison (U.S. Bureau of Justice
Statistics, 2001:10–11).

Racial composition is also likely to affect crime and incarceration rates.
African Americans, for example, were nearly six times more likely than
whites to be murdered in 2000, and seven times more likely than whites to
commit a homicide (U.S. Bureau of Justice Statistics, 2004b). With respect
to prison population, by year-end 2000, African Americans made up
nearly two thirds of all inmates, with incarceration rates for African Amer-
icans roughly eight times that of whites (U.S. Bureau of Justice Statistics,
2001:11).

Two indicators of family disruption are included to capture two dimen-
sions of social disorganization: percentage of female-headed households
with children under the age of 18 years and the percentage of the popula-
tion that is divorced. The first measure reflects the lack of supervision
available to youths, whereas the second measure accounts for weakened
family ties. Prior research finds that such family disruptions and break-
downs lead to violent crime (Messner, 1983; Messner and Sampson, 1991;
Sampson, 1987).

Unemployment data were provided by the Florida Department of
Labor and Employment Security on computer disk. Personal income data
were downloaded from the U.S. Bureau of Economic Analysis website
(http://www.bea.gov/beahome.html). Age-structure and racial composition
data were obtained directly from the Florida Legislature, Office of Eco-
nomic and Demographic Research. Data for female-headed households,
percent divorced, and percent persons living below the poverty-line for
1980 and 1990 were obtained from U.S. Census Bureau, *County and City
Data Books* (U.S. Bureau of the Census, 1983, 1994). Year 2000 data were
obtained from the U.S. Census Bureau website using American Fact
Finder. These variables were only available for decennial census years, and
we estimate data between decennial census years via linear interpolation.
Given the small changes in these variables between decennial census
years, a linear trend was assumed and considered justified (Levitt, 1996).
Means and standard deviations for all variables in our analysis are dis-
played in Table 1.

TABLE 1. SUMMARY STATISTICS

Variable	Mean	Standard Deviation (Overall)	Standard Deviation (Within county)
Dependent Variables			
Homicide Rate per 100K	10.81	7.94	6.31
Rape Rate per 100K	44.20	24.82	16.68
Robbery Rate per 100K	140.45	139.38	50.88
Assault Rate per 100K	484.97	251.36	147.14
Burglary Rate per 100K	1368.12	695.45	395.05
Larceny Rate per 100K	2831.24	1495.40	564.47
Auto Theft Rate per 100K	319.54	274.08	125.99
Independent Variables			
Prison Population	343.25	181.61	121.64
% Aged 15 to 24	13.72	3.69	1.77
% Aged 25 to 34	14.61	2.88	1.23
% Aged 35 to 44	13.50	2.34	1.62
% Black	13.39	8.33	1.20
Poverty Rate	15.89	5.25	1.16
Per-capita Income	12,198.56	3,622.17	1,537.74
% Unemployed	6.34	2.49	1.74
% Divorced	9.37	2.04	1.62
% Female Households With Children < 18 Yrs. Old	8.29	2.37	1.27

All values are expressed in their original units (i.e., before logging).

REGRESSION PROCEDURES FOR PANEL DATA

To assess the impact of prison population growth on crime, we follow conventional strategies for the statistical modeling of panel data and estimate a fixed-effects model. The fixed-effects model requires adding a dummy variable for each county and year (except for the first county and year to avoid perfect collinearity) (Wooldridge, 2000).[14] The county dummies are an integral part of the fixed-effects approach because they allow us to control for the collective effect of stable, unmeasurable, and unobserved county-specific factors that cause crime rates to vary from county to county. Examples of time-invariant factors may be deeply embedded cultural and social norms, policing strategies, political orientation of the county, and urbanity. The county dummies also have the added benefit of controlling for differences in county-level crime reporting practices that remained approximately stable over the study period. The year fixed-effects control for unobserved time-varying shocks that affect the entire state in a given year. Examples of events that may have affected crime

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14. Because the coefficients for the county and year dummies are uninterpretable, i.e., they merely denote the presence of some unobserved time-stable feature of counties or unobserved factors affecting all counties equally in a given year, we do not include them in Table 2.

rates statewide include economic recessions, court orders to alleviate prison overcrowding, revisions made to the state's sentencing guidelines in 1994 and 1995, passage of a concealed handgun law in 1987, and revisions made to the habitual offender statute in 1988. The year dummies also serve to capture the effects of unobserved time-varying shocks occurring at the national level that affected Florida counties in roughly the same manner. Examples of potential national-level factors that could be influencing county-level crime rates were discussed above. Because the analysis includes fixed-effects for both years and counties, the parameter estimates for the prison population variable and specific control variables are based solely on within-county deviations over time.

All continuous variables are expressed as rates per capita (usually per 100,000 population) or percentages to avoid giving high population counties greater weight in the crime rate models. We use a log–log model, also referred to as a constant elasticity model, in which both dependent and independent variables are expressed in their natural logs (Wooldridge, 2000). The log–log model allows us to interpret the coefficients for the independent variables as elasticities—the percent change in the crime rate expected from a 1% change in the independent variable, holding all other factors fixed (Wooldridge, 2000). It is also useful to note that because we estimate a log–log model the elasticity for any given independent variable will remain constant throughout, hence the alternative name, the constant elasticity model (Wooldridge, 2000:44–45). In other words, the constant prison population elasticity implies that the proportional change in crime rates due to proportional changes in incarceration rates is constant regardless of actual levels of imprisonment.[15]

Panel unit root tests (Levin and Lin, 1992; Wu, 1996) indicated that the crime and prison population series were both stationary; i.e., the unit root hypothesis was rejected in all instances, which suggests that the analysis be conducted in levels and not on differenced rates. Heteroskedasticity was detected using a modified Wald statistic, whereas the Breusch–Pagan statistic for panel independence in the residuals showed that the residuals for a given year were not independent across counties (Greene, 2003). Finally, Wooldridge's (2002) test for serial correlation indicated the presence of a first-order autoregressive process in our data. We follow the recommendation of Beck and Katz (1995) and use panel-corrected standard errors (PCSE), which correct for panel heteroskedastic errors and contemporaneous correlation of error terms. Autocorrelation is handled by allowing

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15. We also examined the possibility that the results might be sensitive to our choice of function form by estimating a log–level model and a level–level model. Both models produced results very similar to those obtained with the log–log model. Results from these alternative models are available on request from the authors.

for a first-order autoregressive process that is common to all counties.[16]

The coefficients on the prison population variable were not substantially affected by specification decisions concerning which control variables to include in the crime models, because in almost all cases, correlations between the prison variable and the control variables were weak. Of the 85 bivariate correlations between the prison variable and the control and proxy variables, none exceeded 0.6, and only two reached 0.5. The conclusion of no serious collinear relationships between the prison population variable and control variables was further supported by examination of condition indices and variance-decomposition proportions (discussed in Belsley et al., 1980). There were, however, some collinearity problems among the remaining control and proxy variables, a potential problem we addressed later. Estimation was carried out in Stata, version 8, using the XTPCSE procedure.

RESULTS

Table 2 presents parameter estimates of the effects of prison population growth on each index crime using the procedures outlined. The basic finding in Table 2 is that county-level prison population growth seems to have little or no significant relationship with county-level crime rates, at least not in Florida. Although the prison population variable is in the expected negative direction for each index crime type, the coefficients are far from significant, especially for violent crimes such as homicide, robbery, and assault.[17]

Perhaps the key finding in Table 2 is that the crime elasticities with respect to prison populations are substantially smaller than those reported by Marvell and Moody (1994) and Levitt (1996) using state-level panel data. The estimated elasticity of robbery with respect to incarceration rates, for example, is –0.07. This elasticity is only one fourth the size of the elasticity of –0.26 reported by Marvell and Moody (1994) and one tenth the size of the elasticity of –0.70 reported by Levitt (1996). Most other

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16. Reestimating the PCSEs with a specific autocorrelation coefficient for each county produced results largely similar to those obtained using an autoregressive process common to all counties. Beck and Katz (1995) suggest that analysts deal with serial correlation by including a one-year lag of the dependent variable on the right-hand side of the crime equation (see also Moody, 2001). As we will see, this alternative approach has little impact on the results reported in Table 2.

17. When testing a global null hypothesis, such as the case here (prison growth will be considered effective if it reduces any of seven crime types examined), it is recommended that a Bonferroni adjustment be made to significance levels to account for the use of multiple outcome measures. We do not make any such corrections; in which case, some coefficients reported in Tables 2 to 4 may actually appear to be statistically significant when they are not. We thank David Greenberg for calling to our attention this important point.

TABLE 2. THE ESTIMATED IMPACT OF PRISON POPULATIONS ON INDIVIDUAL INDEX CRIMES

	Dependent Variables: Natural Logs of the Crime Rate per 100,000 people													
	Homicide		Rape		Robbery		Assault		Burglary		Larceny		*Auto Theft*	
	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*
Prison Population	−0.06	−0.84	−0.13	−1.40	−0.07	−1.04	−0.00	−0.04	−0.08	−0.79	−0.08	−1.11	−0.07	−0.71
% Aged 15 to 24	0.33	0.84	**1.02**	2.54	**1.14**	3.09	0.78	1.86	1.11	1.79	0.62	1.47	0.84	1.87
% Aged 25 to 34	−0.16	−0.55	−0.35	−0.87	0.02	0.05	−0.14	−0.34	0.53	1.18	0.40	1.16	0.27	0.50
% Aged 35 to 44	**1.13**	2.67	−0.43	−1.21	**1.30**	3.19	**−0.16**	−0.28	**1.22**	2.11	**1.49**	2.89	**1.39**	2.81
% Black	0.13	0.96	0.02	0.12	−0.25	−1.81	−0.19	−0.84	−0.60	−1.68	−0.21	−0.82	−0.01	−0.02
Poverty Rate	−0.01	−0.05	0.29	1.11	0.11	0.47	0.47	1.92	0.04	0.14	**−0.52**	−2.52	0.24	0.91
Per-capita Income	−0.24	−0.89	0.42	1.23	0.26	0.99	**0.37**	1.13	0.11	0.31	**0.16**	0.56	0.20	0.68
% Unemployed	−0.06	−0.70	0.15	1.22	**0.23**	2.53	**0.32**	2.90	0.22	1.84	**0.22**	2.46	0.19	0.68
% Divorced	−0.04	−0.19	**0.59**	2.05	0.02	0.07	**0.88**	2.61	0.43	1.05	0.42	1.35	−0.15	−0.54
% Female Households With Children < 18 Yrs. Old	**−0.22**	−2.01	−0.11	−0.63	−0.32	−1.80	**−0.39**	−2.36	−0.24	−1.16	**−0.17**	−1.76	**−0.51**	−3.22
N	1,158		1,158		1,158		1,156		1,156		1,156		1,151	
Adjusted R^2	0.58		0.56		0.84		0.82		0.89		0.94		0.82	

Notes: The dependent variables are the natural log of each index crime rate per 100,000 residents. The dataset is comprised of annual county-level data for 58 counties from 1980 to 2000. Data for 1988 are missing. Prisoner populations correspond to June 30th of the year in question. In all cases, estimation allows for heteroskedasticity, contemporaneous correlation of error terms, and first-order autocorrelation. Although not shown, county and year dummies were included in all specifications. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the 0.01 level are both underlined and displayed in bold.

index crimes have crime elasticities with respect to incarceration rates that are one half to one third the size of those obtained using state-level panel data.

To estimate the crime-reduction impact of adding one prisoner at the margin for a county, we multiplied the crime elasticities by the ratio of index crimes to prison population (taken at the means).[18] The result is a reduction of slightly over one index crime per year per additional prisoner. Most crime reduction occurs for property crimes. Of course, this number would almost double if we adjusted for unreported crimes. The numbers of crimes attributable to the average criminal are substantially smaller than the 17 reported by Marvell and Moody (1994) and the 15 reported by Levitt (1996).

There are numerous reasons for why we cannot replicate the results of previous studies using state panel data, and it is impossible to know which is responsible for the differing results. One possible explanation for the smaller crime elasticities is that previous state panel studies also suffer from the omitted variable bias problem plaguing national time-series studies. As noted, to the extent that state prison population growth was correlated with other state-level factors not explicitly controlled for in the fixed-effects regression model, the apparent negative relationship between prison population growth and crime may simply be the byproduct of spurious regression. This issue does not arise here because we only examine the effects of prison population growth on crime for Florida, and thus the year dummy variables control for omitted state-level factors that raise or lower crime rates in a given year across the state.

Another possible explanation for the varying results is that the assumption in pooled state regression models of a single, uniform effect of incarceration on crime in all states is invalid. Indeed, the state-specific results reported by Marvell and Moody (1998, see Table 1) reveal large variations in crime elasticities with regard to in-state and out-of-state prison population growth. DeFina and Arvanites (2002) also find substantial variation across states in the estimated effects of imprisonment on crime when they relax the coefficient restriction imposed by the pooled regression model and estimate separate regressions for each state.[19]

SIMULTANEITY BIAS

We realize some readers might dismiss the nonsignificant results for the prison population variable in Table 2 as a result of our failure to properly

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18.   The ratio of crime to prison population for murder, rape, robbery, assault, burglary, larceny, and auto-theft are 0.03, 0.13, 0.41, 1.41, 3.99, 8.25, and 0.93, respectively.

19.   Interestingly, Florida was one of four states with significant negative imprisonment coefficients for three or more crime types—rape, burglary, and auto-theft.

model the possible simultaneous relationship between prison populations and crime rates (Levitt, 1996; Marvell and Moody, 1994). Simultaneity is considered a serious problem in prison-crime studies because higher crime rates inevitably lead to higher prison populations (Levitt, 1996; Marvell and Moody, 1994). Local justice systems may also respond to crime growth by relying more heavily on incarceration (Marvell and Moody, 1994). Recent research by Smith (2004), however, finds no support for these traditional explanations, i.e. that increases in crime lead to increases in prison population levels. Nevertheless, the coefficients for the prison population variables in Table 2 are biased if crime rates have a same-year impact on prison populations.

Similar to Marvell and Moody (1994), we explore the possibility of a nonrecursive relationship between prisons and crime using the Granger causality test. The Granger causality test is conducted by regressing the potential endogenous regressor, i.e., prison population, on one-year and two-year lags of itself and each index crime (Wooldridge, 2000). Granger causation is indicated when the lags of crime are significant in the positive direction. The Granger test has a drawback in that it cannot assess contemporaneous causation between prison and crime (Wooldridge, 2000); that is, it cannot determine whether prison populations are truly exogenous in the crime equations. We agree with Marvell and Moody (1994), however, that if crime does affect prison population levels, it must do so at least partially through a one-year lag because it takes time for authorities to become aware of crime and for the criminal justice system to arrest, convict, and sentence someone to prison for that crime.[20] Also, because we conduct the Granger test in levels (as opposed to first-differences), any contemporaneous causation would be reflected in the one-year lag due to serial correlation (correlation between current and prior year crime). For these reasons, therefore, the absence of a one-year lagged impact of crime on prison populations implies the absence of an immediate impact.

The results of the Granger test for each index crime are presented in Table 3. The key results are the *t*-ratios for the one-year lags of crime. With the exception of robbery, there is little evidence that changes in crime precede changes in prison populations. The coefficients on the one-year lags of crime are small, statistically insignificant, although they are always in the expected positive direction. Although the one-year lag of homicide is marginally significant, which suggests that increases in homicide lead to enlarging prison populations, it is unlikely to explain the null

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20. In 2000, for example, the median time between arrest and sentencing for violent crimes was 186 days (U.S. Bureau of Justice Statistics, 2003). Of course, it does not include the time period between when a crime is reported to authorities and when someone is arrested for that crime.

results for the prison population variable in the homicide equation in Table 2. First, homicide offenders account for a small proportion of annual admissions, only 3% in 2000 (U.S. Bureau of Justice Statistics, 2003a). This result suggests that homicide offenders account for very little of the year-to-year changes in prison populations. Second, the lag between arrest and conviction for homicide is slightly over a year (U.S. Bureau of Justice Statistics, 2003b), which makes an instantaneous impact of homicide rates on prison populations unlikely.[21]

TABLE 3. GRANGER ANALYSIS OF THE IMPACT OF CRIME RATES ON PRISON POPULATION GROWTH

Coefficients on Crime Rates Lagged One and Two Years (Independent Variables)						
	One-Year Lag		Two-Year Lag		F Value	
Independent Variable	Coef.	t	Coef.	t	Value	Prob.
Homicide	0.022	1.91	0.004	0.34	3.71	0.16
Rape	0.008	0.52	0.001	0.09	0.35	0.84
Robbery	**0.046**	2.63	0.006	0.35	**7.86**	0.46
Assault	0.023	1.12	−0.020	−0.98	1.55	0.46
Burglary	0.040	1.66	−0.011	−0.43	2.88	0.24
Larceny	0.008	0.30	−0.001	−0.04	0.12	0.94
Auto Theft	0.019	0.95	−0.013	−0.66	0.94	0.62

Notes: Prison populations rates are regressed on prison populations lagged one and two years, the crime rate lagged one and two years, and the control variables listed in Table 2. Only the results for lags of crime are presented, and the F value is for the two lags. In all cases, estimation allows for heteroskedasticity and contemporaneous correlation of error terms. Coefficients that are significant at the 0.05 level are displayed in bold. Coefficients that are significant at the 0.01 level are both underlined and displayed in bold.

With respect to robbery, we cannot rule out the possibility of a current-year impact on prison populations. The coefficient on the one-year lag of robbery is highly significant in the expected positive direction and seems to have a modest impact on prison populations. Because the one-year lag of robbery is likely to be correlated with current-year robbery due to serial correlation, the possibility of a current-year impact of robbery on prison

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21.   One referee suggested that the effects of homicide rates on prison population growth might be contemporaneous for reasons having little to do with convicted killers being sentenced to prison. If, for example, increases in county homicide lead judges to sentence other offenders more harshly, or parole boards to be less willing to grant parole, then the effects of homicide on prison population growth would be immediate. We are not persuaded by this claim as it presupposes a positive association between actual and perceived changes in homicide trends. The preponderance of the research evidence, however, suggests this association is not very strong (see Kleck et al., 2005).

levels remains. Consequently, the robbery equation in Table 2 may be unidentified.

In all, the results of the Granger causality test provide little evidence of a same-year impact of crime rates on county prison populations. More importantly, there is little reason to believe (with the possible exception of robbery) that the ordinary least-squares (OLS) estimates of the effects of prisons on crime reported in Table 2 severely underestimated the true magnitude of the effect of incarceration due to the presence of simultaneity bias.

ROBUSTNESS CHECKS

Table 4 investigates the sensitivity of the prison population coefficients to a range of alternative specifications.[22] We take the specifications with the full set of controls in Table 2 as a baseline. The prison coefficients from those regressions are reported in the top row of Table 4. Each row of the table represents a different specification.

The first set of specifications use alternative estimators for calculating standard errors that are robust in the presence of violations of regression assumptions discussed above. As seen in rows 2 to 5, the prison population variables remain statistically insignificant when using other preferred methods for calculating standard errors.

Perhaps the most important alternative specifications are those presented in row 6, which reexamine the potential two-way relationship between prison populations and crime rates using instrumental variables (IV) regression. The crime rates are identified by using the incarceration rate lagged three years as an instrument for the contemporaneous incarceration rates.[23] As one might expect, the incarceration rate lagged three-years is highly correlated with current-year rates.[24] The *F*-statistic for the lagged incarceration rate variable in the first-stage regression is 16, which exceeds the recommended cutoff of 10 for excluded instruments by Staiger

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22. One referee suggested we examine the possibility that the effects of imprisonment weaken as prison populations grow larger. Our model specification already takes into account such effects; that is, the log–log model assumes diminishing marginal returns on the crime-control effect of imprisonment.

23. Using lagged values of a potential endogenous regressor to instrument for current-year values is common practice in economics, especially with panel data (Wooldridge, 2002).

24. Because the crime equations are just-identified, it is not possible to test for overidentifying restrictions; that is, we cannot determine whether the three-year lagged incarceration rate variable is correlated with the error process in the crime equations. When we use multiple lags of the incarceration rate variable (three-year and four-year lags), the four-year lag is insignificant in the first-stage equation, which means that in effect there is no overidentification, and an overidentifying restrictions test would have no power.

TABLE 4. SENSITIVITY OF PRISON POPULATION COEFFICIENTS TO ALTERNATIVE SPECIFICATIONS

| | Coefficient on the Prison Population Variable When the Dependent Variable Is | | | | | | | | | | | | | |
| | Homicide | | Rape | | Robbery | | Assault | | Burglary | | Larceny | | Auto Theft | |
	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t	Coef.	t
1. Baseline	–0.06	–0.84	–0.13	–1.40	–0.07	–1.04	–0.00	–0.04	–0.08	–0.79	–0.08	–1.11	–0.07	–0.71
2. PCSE errors, county specific AR(1)	–0.07	–1.06	–0.12	–1.86	–0.06	–0.97	0.11	1.29	0.02	0.18	–0.09	–1.25	0.00	0.02
3. Huber-white std. errors with lagged DVs	–0.06	–0.90	–0.12	–1.81	0.01	0.14	0.04	0.56	–0.02	–0.22	–0.06	–1.10	0.04	0.69
4. Newey std. errors	–0.06	–0.85	–0.14	–1.90	0.03	0.55	0.08	0.84	0.01	0.12	–0.06	–0.72	0.10	1.44
5. Robust std. errors corrected for clustering	–0.06	–0.80	–0.14	–1.78	0.03	0.47	0.08	0.76	0.01	0.10	–0.06	–0.61	0.10	1.04
6. Prison population, lagged one-year	–0.00	–0.02	–0.09	–1.02	0.07	0.94	0.08	0.83	0.03	0.32	–0.01	–0.11	**0.22**	2.40
7. Homicide, Mortality-Detail Files, CDC	–0.02	–0.25	—	—	—	—	—	—	—	—	—	—	—	—
8. No control variables	**–0.27**	–4.25	–0.04	–0.72	–0.08	–1.67	**0.16**	2.52	**–0.21**	–2.91	–0.06	–1.03	0.06	0.96
9. No year dummies	–0.11	–1.62	–0.16	–1.92	**–0.16**	–2.22	–0.01	–0.14	–0.08	–0.75	–0.04	–0.49	–0.07	–0.72
10. Weighted by county population	–0.03	–0.30	**–0.16**	–2.33	–0.05	–0.81	–0.04	–0.96	**–0.10**	–2.78	**–0.09**	–2.82	–0.07	–1.14
11. 2SLS estimates, using prison lagged 3-years as an excluded instrument	–0.39	–1.18	–0.27	–0.93	0.49	1.33	–0.16	–0.54	0.25	0.57	–0.13	–0.33	0.28	1.04
12. Out-of-county prison population	.051	0.44	.147	1.08	.076	0.69	–.093	–0.67	.078	0.48	.156	1.30	–0.11	–0.08

Notes: Results in this table are variations on the crime specifications reported in Table 2. The top row of the current table is the baseline specification that is presented in Table 2. Coefficients that are significant at the 0.05 level are displayed in bold. Coefficients that are significant at the 0.01 level are both underlined and displayed in bold.

and Stock (1997:557). As seen in row 6, the coefficients on the prison pop-
ulation variable are still far from statistically significant in the negative
direction, although they are substantially larger than those obtained using
the OLS estimator, even after purging the prison population variable of
potential endogeneity bias.

Finally, we examine Marvell and Moody's (1998) thesis that by failing to
account for free-rider effects we have severely underestimated the total
effects of imprisonment on crime. Although free-rider effects are unlikely
to explain the smaller associations found for imprisonment in state panel
studies, because cross-state movement of criminals is not that significant,
such effects could be substantial in a county-level study. Because counties
are smaller geographical areas than states, and thus they are nearer to
each other, it is not unreasonable to expect substantial potential between-
county movement of criminals. It is not implausible, for example, that
locking up criminals in Gadsden or Wakulla counties could help reduce
crime in neighboring Leon County and vice versa, given the short dis-
tances criminals would have to travel to move from one of these counties
to the next.

To examine this possibility, we created an out-of-county prison popula-
tion variable including those prisoners locked up in nearby counties (i.e.,
counties that touch the subject county) and reestimated the benchmark
model specifications shown in Table 2 including both in-county and out-of-
county prison variables as independent variables.[25] As seen in row 12 of
Table 4, nearby prison population has no impact on in-county crime.[26] The
coefficients on the nearby prison variable are never significant and are
usually in the unexpected positive direction. This result implies that pris-
oners seldom travel to nearby counties to commit crime. More impor-
tantly, the results for the nearby prison variable indicate that we have not
severely underestimated the effects of imprisonment on crime by failing to
account for short-distance free-riding effects.[27]

DISCUSSION AND CONCLUSION

This study finds no support for the "more prisoners, less crime" thesis.

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25. The nearby prison variable is defined as the number of prisoners in bordering
counties divided by bordering county population. Similar to the in-county variable, the
nearby prison variable was expressed as a rate per 100,000 population and then logged.

26. Although not shown, the results for the in-county prison variable remained
qualitatively unaffected when including the nearby prison variable in the benchmark
model specification.

27. Of course, we cannot rule out the possibility of free-riding effects due to longer
distance migration. But the notion that such effects would be more important than
those due to short-distance migration or in-county prison population, and thus explain
the nonsignificant findings reported above for imprisonment, is simply inconceivable.

As mentioned, Marvell and Moody (1998) have criticized prison population-crime research for failing to consider possible free-riding effects. According to their logic, this phenomenon would be even more likely at the county level. However, we found no evidence of free-riding and thus no reason to suspect free-riding as a plausible explanation for our null findings. Three decades of studies on the criminal mobility of arrested offenders and ethnographic research on active offenders suggest that most offenders commit their crimes close to home (e.g., Baldwin and Bottoms, 1976; Brantingham and Brantingham, 1981; Bullock, 1955; Wright and Decker, 1997).

Although there is ample evidence at the national and state level that enlarging prison populations reduces crime (e.g., Devine et al., 1988; Levitt, 1996; Marvell and Moody, 1994, 1997a), our findings indicate that Florida counties relying most heavily on this crime-control strategy have not enjoyed similar benefits. These findings do not imply, however, that imprisonment did not have any crime-reduction benefits. Rather, they support the view that any deterrent or incapacitation effects, however large or small they may have been, did not covary with county-level prison population growth to any substantial degree. That is, there may have been some baseline deterrent and incapacitation effects that imprisonment generated, but apparently these effects did not consistently increase with increased county-level prison population growth.

Our study precludes us from drawing any definitive conclusions as to why county-level prison population growth does not seem to be associated with lower crime rates; thus, we limit the following discussion to possible explanations for this phenomenon. Perhaps our discussion can spur future research that can better explain why Florida and possibly other states have failed to benefit or benefited (as the case may be) from prison population growth.

As the prison population expands, its potential impact on crime may decrease as lower rate offenders are included in the expansion. Thus, incarcerating serious, high-rate offenders may reduce crime, but expanding incarceration to include less serious, lower rate offenders will produce small reductions in crime (Lynch and Sabol, 1997). Counties that incarcerated more offenders may not see the desired benefits in terms of crime reduction because a policy of mass incarceration may not target the right offenders, i.e., high-rate offenders. As evidenced by past research, a small percentage of offenders commit the greatest percentage of crime (Farrington, 1986; Greenwood, 1982; Wolfgang et al., 1972). A crime-control policy of incarceration should have the largest impact on crime rates when it is reserved for these types of offenders. However, targeting the "right" offenders has always been a challenging process (Walker, 2001). In addition, any incapacitative effects of imprisonment may be limited if the

offenders targeted for incarceration are at the end of their criminal careers; i.e., they would not have engaged in further crimes (Clear, 1996). It is possible that counties have not been as effective in targeting the "right" offenders and therefore have not seen a reduction in crime from increasing their prison populations.

In addition, removing active offenders may not control crime if they are being replaced (Clear, 1996). Previous research by Reiss (1988) suggests that a large percentage of crime, particularly drug crimes and robbery, occurs in groups. Thus, when one group member is incarcerated, the rest of the group continues to offend with or without a replacement. In addition, to the extent that other factors related to crime remain the same, e.g., poverty, unemployment, and social disorganization, there will always be a ready supply of potential offenders to replace those who have been incarcerated. Thus, counties that increased their prison population may have a large supply of potential offenders ready to replace those removed by imprisonment, in effect reducing or canceling out the incapacitative effects of imprisonment. According to our data, of the 19 counties that increased prison population from 1980 to 2000 above the state average, i.e., high imprisonment counties, 12 had poverty rates that also exceeded the state average. In other words, most high imprisonment counties also had higher rates of economic distress, which suggests that a pool of potential offenders was readily available.

In addition, incarceration should produce reduced crime rates through its deterrent effect on offenders and potential offenders. However, as Cook (1998) and others (see Clear, 1996; Mauer, 1999) have suggested, expanding incarceration may serve to lessen the deterrent effect of prison as the prison experience becomes less stigmatized. In theory, as offenders and potential offenders weigh the expected utility of offending, the "costs" associated with imprisonment should factor into their analysis. However, as Clear (1996) has argued, if offenders or potential offenders' images of prison are less severe, prison loses its deterrent factor. Through offenders' experiences or the experiences related to them by others, offenders and even potential offenders may lessen the costs of imprisonment when calculating the costs and benefits of crime. Thus, an overreliance on incarceration may lessen both specific deterrence, as offenders become knowledgeable about a previous "unknown," and general deterrence, as others become aware of the prison experience. Moreover, as Cook (1998) has suggested, prison has become less stigmatizing and may even be rationalized as a benefit (i.e., through status enhancement) rather than a cost. We also note that the potential for prison expansion to produce general deterrence is in part dependent on offenders' perceptions of the likelihood of their incarceration. As Kleck et al.'s (2005) research demonstrates, offenders and nonoffenders are woefully inaccurate in their assessment of

criminal justice penalties. Again, inaccurate perceptions as to the use of prison may reduce the costs of prison.

In sum, Florida counties that relied most heavily on imprisonment as a tool to control crime did not as a result experience greater reductions in crime. Although as a crime-control strategy, mass imprisonment does not seem to work in Florida, future research should continue to examine the relationship between prison population growth and crime in other states. In addition, researchers should continue to examine alternative explanations for the rise and fall of crime rates over time so that policymakers can be better informed as to what is working and what is not.

Considering the complexity of the policy-making process, it would be naïve to think that Florida policymakers could simply reverse their crime-control strategy of incapacitation, but our findings do suggest that Florida policymakers should continue to explore alternative crime-control strategies. This research is particularly important in light of the tremendous financial and social costs of imprisonment as compared with its benefits (Austin and Irwin, 2001; Mauer and Chesney-Lind, 2002; Petersilia, 2003). Lastly, considering the expected prison releases to occur as the result of over two decades of crime-control policy that has relied heavily on incarceration, policymakers need to carefully weigh the costs and benefits of our continued reliance on mass incarceration against the potential costs and benefits of alternatives. If the costs of mass incarceration do not return appreciable benefits, i.e., a reduction in crime, it is time to reconsider our approach to crime and punishment. Researchers have offered evidence of crime prevention programs operating inside the criminal justice system and in communities that hold promise for reducing crime (see Petersilia, 2003; Sherman et al., 2002).

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Figure 1. U.S. and Florida Prison Population and Crime Rates, 1980-2000