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CRIMINOLOGY

THE EMPIRICS OF PRISON GROWTH: A CRITICAL REVIEW AND PATH FORWARD

JOHN F. PFAFF*

A growing empirical literature has sought to explain the forces behind the significant expansion of the U.S. prison population over the past thirty years. Unfortunately, the studies to date have suffered from important methodological, conceptual, and definitional problems that have significantly curtailed their ability to identify causal effects. In this Article, I examine several of the central limitations and discuss remedies. I start by reviewing the theories that investigators have sought to test. I then discuss the studies' empirical defects, such as failing to account for endogenous relationships, overlooking the risk of model dependency, ignoring complex dynamic relationships, using variables that either do not correctly measure the relevant theory or do not isolate it from competing claims, and relying consistently on the same source of data. I also propose several remedies to these and other shortcomings. The current literature has begun to shed light on the forces driving prison growth, but more carefully designed models are needed before we can be confident in the findings.

I. INTRODUCTION

The single most striking statistic in the American criminal justice system is its thirty-year expansion in prison population. From 300,000 prisoners in 1977, the prison population has risen steadily to over 1.5 million as of June 30, 2005, a 400% increase. By 2005, states were collectively spending over \$43 billion per year on corrections. That this followed nearly fifty years of relative stability makes the growth all the more remarkable.

* Associate Professor of Law, Fordham Law School. I would like to thank David Abrams, Kenworthy Bilz, David Greenberg, Bernard Harcourt, Tracey Meares, Tom Miles, and Daniel Richman, as well as participants at the University of Chicago's Crime and Punishment Workshop, for many helpful comments. All errors are my own.

This growth touches on myriad major social issues: how governments have responded to declining faith in the welfare state; how the American legal system—and American society more generally—treats its minority and poorer citizens; how states and counties allocate their scarce financial resources, and how the federal government influences these decisions; how governments respond to shifting patterns in crime and social disorder. Not surprisingly, given its far-reaching effects and implications, the prison system's growth has spawned a large literature examining the causes, implications, and justifications (or lack thereof) for this profound shift in the use of incarceration.

I have two goals in this Article: to provide a systematic analysis of what we know about the forces that have driven this growth, and to set forth a framework for producing more reliable and accurate results in the future. In particular, I evaluate an empirical literature that spans criminology, economics, law, political science, and sociology. While hundreds of articles and dozens of books have been written about recent trends in U.S. prison populations, most have been predominantly normative or historical in their approaches. A small core of articles, however, has attempted to evaluate the factors influencing these trends using sophisticated empirical models, and it is on these that I focus. That is not to say at all that the other approaches are unimportant, but they require a different sort of evaluation.

An accurate empirical understanding of prison growth is vital for policy planners of all stripes. Reformers concerned with the magnitude of the prison population may not know whether to focus on the adoption of sentencing guidelines, more expansive representation for the indigent, or the use of commission-drafted fiscal impact statements unless they know whether the growth of the prison population has been driven by racial inequalities, the poverty of defendants, or legislative budgetary concerns. Conversely, those who support incarceration for utilitarian or other normative (e.g., retributive) reasons need to understand the factors driving its expansion to assess its appeal. A clearer appreciation of how we got here and where we are heading benefits everyone—from fiscal conservatives to social liberals.

I find, however, that in general the current empirical literature answers these questions poorly. Those who study prison populations have settled on a well-defined set of widely accepted explanatory theories. But the efforts to test these theories suffer from numerous important limitations that inhibit their ability to identify causal effects accurately. That is not to say that we know nothing at this point, but our current knowledge is quite rough.

In this Article, I set out to accomplish two specific tasks. First, I flesh out what it is that we know, and (of equal importance) what we do not know. Though limited, the current findings do provide some useful

insights. In particular, the studies point to seven broad claims, four with respect to prison admissions and three with respect to total population. First, the studies suggest that the admissions rate increases with the crime rate, the percent of the population between the ages of twenty-four and thirty-five, and the percent of the population that is black; and falls with personal income per capita. The studies provide fewer insights for the models looking at total prison population, due at least in part to complex (and unaddressed) dynamic features of those models. Nonetheless, the results imply that total prison population rises with the crime rate, the percent of the population between eighteen and thirty-five, and the percent of the population that is black.

Second, as part of a larger empirical project looking at prison growth,¹ I discuss numerous methodological limitations with the current studies and improvements that will lead to more accurate and stable results. I focus on four concerns in particular: that models (1) suffer from uncorrected endogenous relationships; (2) exhibit little stability within and across articles, either due to problems of omitted variable bias or deeper structural limitations; (3) rely on poorly chosen variables (both dependent and independent); and (4) consistently use the same data rather than looking for alternative sources of information. Of course, in a world of limited and sometimes flawed data, it may prove impossible to satisfy a theoretical gold standard, and it is important not to cross the thin line between idealism (“we can do it better”) and nihilism (“we will never be able to do it well enough”)—or at least not without caution (and evidence). Some of the problems with the current studies can be overcome to produce increasingly reliable results, and I explore several such possibilities in this Article. But it is also important to identify those issues that are, at least for now, intractable, and to consider the implications of such blind spots.

This Article proceeds as follows. Part II lays out the broad trends in prison population and discusses the various theories developed over the years to explain them. Part III then critically reviews the current empirical literature, identifying the insights we can draw from it and highlighting the

¹ See John F. Pfaff, *Prison Population Growth: A Comprehensive Empirical Model* (2008) (unpublished manuscript, on file with author) (implementing many of the suggestions in this Article to develop a more precise model of the causal factors that have influenced prison growth) [hereinafter Pfaff, *Prison Population Growth*]; John F. Pfaff, *Sentencing Guidelines, Prison Populations, and Crime Rates: A Cost-Benefit Analysis* (2008) (unpublished manuscript, on file with author) (exploring how guideline adoption influences both crime rates and correctional expenditures) [hereinafter Pfaff, *Sentencing Guidelines*]; John F. Pfaff, *Trends in Prison Release: Evidence from the National Corrections Reporting Program* (2008) (unpublished manuscript, on file with author) [hereinafter Pfaff, *Trends in Prison Release*].

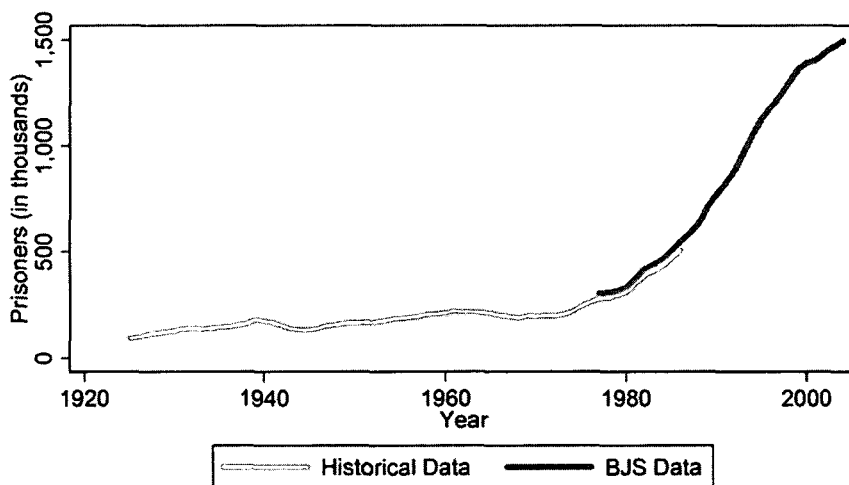
open questions. Part IV focuses on several significant improvements for future efforts that lead to more reliable results.

II. A BRIEF HISTORY OF PRISON GROWTH

A. TRENDS IN THE USE OF PRISON

Figure 1 is by now well known. The U.S. prison population fluctuated gently from the 1920s to the 1970s, but by the late 1970s it had started a slow and steady expansion that has continued unabated ever since. State expenditures on corrections grew rapidly during this period as well: data from the Census suggest that the share of state expenditures dedicated to corrections grew by approximately 4% per year during the 1980s and 1990s, reaching nearly \$43 billion in 2005. And these numbers ignore the collateral implications of prison growth, such as its effects on inmates (both while in prison and after), their families, and their communities.

Figure 1
United States Prison Population, 1925-2004

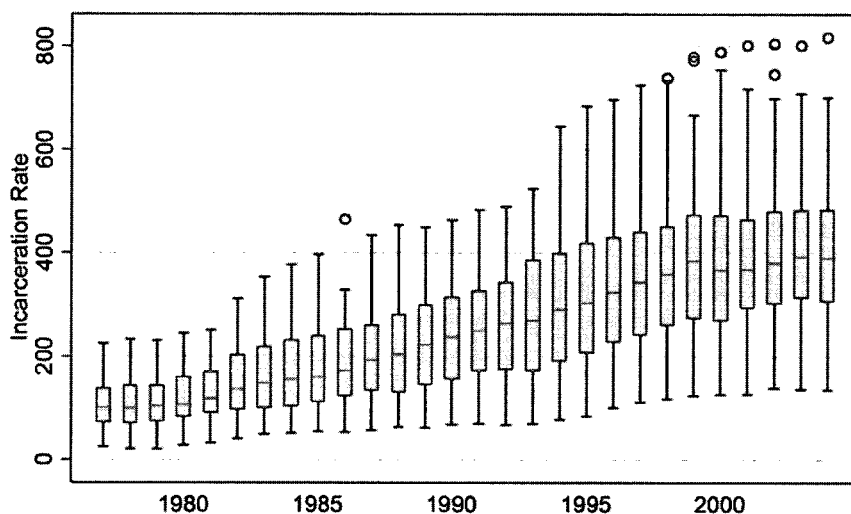


Sources:
 1925 to 1986: Bureau of Just. Stats.: Historical Statistics on Prisoners in State and Federal Institutions, Year End 1925-1986.
 1977 to 2004: BJS website: <http://www.ojp.usdoj.gov/bjs>.

Showing *that* prison populations have soared is easy; explaining *why* they have is more difficult. Figure 2 suggests one reason why simple solutions may be hard to find. This Figure plots the variation in annual state incarceration rates between 1977 and 2004. It is clear that growth in

the U.S. prison population has been accompanied by a divergence in state behavior. The rate of growth has varied widely across the states: between 1977 and 2004, prison population grew by only 59% in North Carolina, but by 678% in North Dakota and 538% in Mississippi.²

Figure 2
State Incarceration Rates, 1977-2004



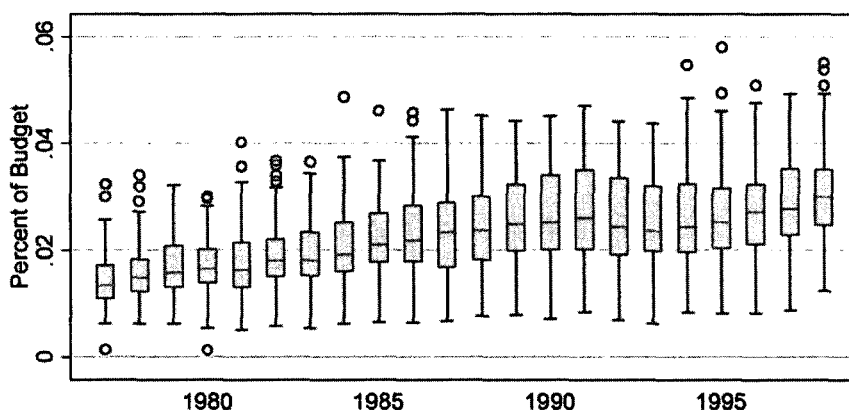
Source: BJS website: <http://www.ojp.usdoj.gov/bjs>.

Because of extreme values that noticeably flatten the figure, Washington, D.C. is omitted.

The trends in expenditures appear to be less divergent. Figure 3 plots the variation in the percent of state budgets dedicated to corrections. As in Figure 2, the average rises slowly and steadily, but in Figure 3 the range between the upper and lower bounds appears relatively stable. No state appears to dedicate more than 5.8% of its budget to corrections in a given year. Nonetheless, in 1998 over \$30 billion was spent on corrections; in 2005, nearly \$43 billion. Regardless of whether this is too much or too little—a challenging normative and empirical question beyond the scope of this Article—the total amount, along with the number of people incarcerated, indicates the critical importance of understanding the dynamics of prison growth.

² North Carolina's seeming laxity is deceptive. Its rate of growth is low because in the late 1970s it often had the highest incarceration rate in the nation. By 2004, its incarceration was below the national average, but not by much: 359 per 100,000, compared to 488 per 100,000 nationally.

Figure 3
Percent of State Budgets Spent on Corrections, 1977-1998



Source: Bureau of the Census, Annual Survey of Gov'ts, Finance Statistics, various years.
 Data available on-line at <http://www.icpsr.umich.edu>.
 Two observations have been omitted: Arizona in 1983 and South Carolina in 1990.
 The first reported 11%, more than twice the second-highest value (5.4%).
 The second reported 13%, more than four times the second-highest value (3.6%).
 These appear to be errors.

B. THE GENERAL THEORIES OF PRISON GROWTH

Though the issue of relatively short-term prison population *trends* has only recently attracted significant scholarly attention, the prison as an institution has long been studied. Among the most prominent to explore its role in society are Émile Durkheim, Max Weber, Michel Foucault, and Georg Rusche and Otto Kirchheimer.³ Durkheim considers the historical rise of the prison, arguing that it becomes the primary form of punishment as a society matures.⁴ His concern, however, is not with the trends in prison populations over several years within a society, but instead with the evolution of prison's use as the society itself develops. Weber focuses more on the rationalization of punishment, examining its centralization and use of hierarchical systems, uniformity, specialization, penal infrastructure,

³ DAVID GARLAND, *PUNISHMENT AND MODERN SOCIETY* (1990), provides a comprehensive overview of the works of Durkheim, Weber, Foucault, Rusche and Kirchheimer, and the concerns raised by their critics. Franklin Zimring and Gordon Hawkins likewise present a lucid summary of these issues. See FRANKLIN ZIMRING & GORDON HAWKINS, *THE SCALE OF IMPRISONMENT* (1991).

⁴ Émile Durkheim, *The Evolution of Punishment*, in DURKHEIM AND THE LAW 114 (Steven Lukes & Andrew Scull eds., 1983).

and professionalism.⁵ And Foucault, like Durkheim, explores how the prison developed at a macro level, tying its birth to the growing efforts by the state to control its citizens.⁶

Despite their importance to our understanding of the prison as an institution, these canonical sociological works provide little insight into the question of recent prison population trends. The one notable exception is the work of Rusche and Kirchheimer, which in fact explicitly considers prison population change during a given historical era. Rusche and Kirchheimer adopt a political-economic perspective, arguing that prison policies adapt to reflect changes in the labor market: the tighter the labor market, the less incarceration is used. While admitting that non-economic factors matter, they assert the centrality of economic forces.⁷

Given the general absence of scholarship looking at prison population trends—and one could never expect scholars writing before the 1970s and 1980s to predict the subsequent explosion—recent researchers have developed new theories, and built on the old, to explain prison growth. Their efforts fall into four broad schools of thought: the “crime theory,” which links prison populations to crime rates; the “economic theory,” to the importance of labor market and economics conditions; the “demographics theory,” to shifts in race and age; and the “political theory,” to changes in political ideology or manipulation by politicians seeking reelection.⁸ Four other, less overarching theories have been noted as well: the deinstitutionalization of the mental health system, the expansion of prison capacity, the imposition of population caps by federal courts, and the fiscal health of the states. Each of these theories also provides important insights into the mechanics of population growth. I will consider each in turn.

1. The Crime Theory

The crime theory posits that a powerful factor influencing incarceration rates is crime. Its simplest version is quite intuitive: rising crime rates should increase prison admissions and populations (and declines in crime should lead to corresponding reductions, perhaps with a lag). Yet

⁵ See GARLAND, *supra* note 3, at 177-92.

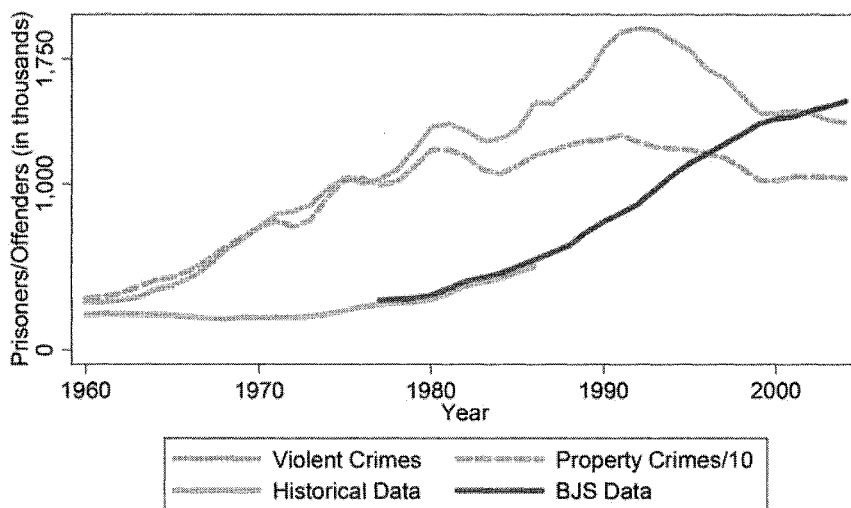
⁶ MICHEL FOUCAULT, *DISCIPLINE AND PUNISH* 231 (Alan Sheridan trans., Vintage Books 2d ed. 1995) (1977). Zimring and Hawkins note that “[a]part from parenthetical references Foucault does not discuss the determinants of prison population or demonstrate any interest in such matters.” ZIMRING & HAWKINS, *supra* note 3, at 45-48.

⁷ GEORG RUCHE & OTTO KIRCHHEIMER, *PUNISHMENT AND SOCIAL STRUCTURE* 3-7 (1939).

⁸ Michael Tonry, *Thinking About Crime*, ch. 2 (2004), lists eight, not four, schools of thought. His eight, however, are essentially incorporated within three of the categories here; he does not consider the economic theories.

this theory has often been viewed with suspicion.⁹ Perhaps this is due in part to the graphical evidence. As Figure 4 demonstrates, between 1960 and 2004 crime and incarceration did not move in a highly synchronized manner.

Figure 4
Offender and Prison Populations, 1960-2004



Source: BJS website: <http://www.ojp.usdoj.gov/bjs>.
Property crimes are divided by 10 to fit on same axes.

In the 1970s, Alfred Blumstein attempted to establish the lack of a crime/prison-population connection more rigorously. Along with Soumyo Moitra, he argued that a complex feedback loop linked crime and incarceration to keep the prison population constant. Their theory held that as crime rises and drives up the prison population, the minimum level of deviance needed for incarceration rises as a counterweight, keeping the prison population relatively fixed regardless of the crime rate.¹⁰ Blumstein later recanted this theory in light of the prison population boom that started

⁹ Zimring and Hawkins, along with Alfred Blumstein and Allen Beck, find the theory wanting while acknowledging that there is likely some relationship. Alfred Blumstein & Allen J. Beck, *Population Growth in U.S. Prisons, 1980-1996*, in *PRISONS* (Michael Tonry & Joan Petersilia eds., 1999); ZIMRING & HAWKINS, *supra* note 3, at ch. 5.

¹⁰ Alfred Blumstein & Soumyo Moitra, *An Analysis of Time Series of the Imprisonment Rate in the States of the United States: A Further Test of the Stability of Punishment Hypothesis*, 70 J. CRIM. L. & CRIMINOLOGY 376, 376 (1979).

in the 1970s,¹¹ but he has continued to argue that the relationship between crime and punishment is weak. In a recent article written with Allen Beck, he states that only 12% of the growth in state prison populations between 1980 and 1996 was tied to changes in offending (with the remaining 88% due to increased incarcerations and longer sentences).¹² And Blumstein is not alone. Zimring and Hawkins, for example, echo his views, pointing to evidence that suggests that a single incarceration occurs for every 100 felonies committed.¹³

Nonetheless, it appears that crime does in fact influence the incarceration rate. The studies below that include measures of the crime rate consistently find a positive relationship between crime—violent crime in particular—and prison populations or admissions. Moreover, recent work suggests that these estimates *understate* the true effect because they ignore the simultaneous relationship between prisons and crime.¹⁴

2. The Economic Theory

The economic theory suggests that underlying economic conditions are powerful forces driving prison populations. Rusche and Kirchheimer's book is often considered the seminal work in this field.¹⁵ It argues that penal practices respond to labor conditions: the more abundant labor is, the less care the penal system shows to those who fall within its grasp. David Rothman and Michael Ignatieff built on this political-economy theory to claim that the prison was developed to reassert control and social stability as capitalism undermined the *ancien régime* and its traditional forms of control.¹⁶

Rusche and Kirchheimer's theory, however, suffers from several shortcomings, which David Garland lays out clearly. In particular, prisons often operate at a loss and thus not (as Rusche and Kirchheimer claim) to the benefit of the state; penal practices often differ across states and countries with similar economic conditions; and ideology and politics

¹¹ See Alfred Blumstein, *Prisons*, in CRIME 388 (James Q. Wilson & Joan Petersilia eds., 1995). Kevin Reitz criticizes the article by Blumstein and Moitra, noting that its "stability thesis fit well with the prior trends of [Blumstein and Moitra's] own lifetime[s] up to the 1970s," but that it is dangerous to "generalize one's contemporary experiences to other eras and to the future." Kevin R. Reitz, *Zimring, Hawkins, and the Macro Problems of Imprisonment*, 87 J. CRIM. L. & CRIMINOLOGY 604, 610-11 (1997).

¹² Blumstein & Beck, *supra* note 9, at 43.

¹³ ZIMRING & HAWKINS, *supra* note 3, at 124.

¹⁴ Yair Listokin provides clear evidence of this, as discussed in Part III below.

¹⁵ See RUSCHE & KIRCHHEIMER, *supra* note 7.

¹⁶ MICHAEL IGNATIEFF, A JUST MEASURE OF PAIN: THE PENITENTIARY IN THE INDUSTRIAL REVOLUTION (1978); DAVID J. ROTHMAN, THE DISCOVERY OF THE ASYLUM: SOCIAL ORDER AND DISORDER IN THE NEW REPUBLIC (1990).

surely play a role.¹⁷ Furthermore, such a theory over-anthropomorphizes capitalism. Penal practices can shape and control the labor market only if there is a high degree of coordination between police, judges, and non-legal actors in the business world.¹⁸ Ignatieff, at least, later openly backed away from his own argument, admitting that revisionist histories of the prison, including his own 1978 work, “contained three basic misconceptions”: they overstated the state’s monopoly of social control, the centrality of the state’s “moral authority” to “social order,” and the extent to which “all social relations can be described in the language of subordination.”¹⁹

Note, however, that even if Rusche and Kirchheimer and their successors failed to develop a convincing overarching theory, they are right to draw our attention to the potential importance of economic conditions. Judges may be more willing to sentence the unemployed, and to sentence them to longer sentences.²⁰ And poverty can encourage crime and thus increase prison populations. In these cases, unemployment influences incarceration even though the judges are not involved directly in macro-level social control. When unemployment is higher, judges are simply more likely to face unemployed defendants at sentencing (and to give them longer sentences). Furthermore, greater income inequality may make the “underclass” appear more “different,” leading judges—perhaps unconsciously—to sentence such defendants more harshly. Thus while theories of coordinated social control are problematic, it remains important to evaluate how economic factors operate in less direct, but still important, ways.

¹⁷ GARLAND, *supra* note 3, at 105-10.

¹⁸ David Rothman criticizes similar theories that assert that the commonalities between Jacksonian-era prisons and factories came about as an effort to train workforces in prison. Rothman suggests that the more likely answer is that wardens and foremen alike operated from the same set of basic assumptions about organization. David J. Rothman, *Perfecting the Prison: United States, 1789-1865*, in *THE OXFORD HISTORY OF THE PRISON: THE PRACTICE OF PUNISHMENT IN WESTERN SOCIETY* 111-29 (Norval Morris & David J. Rothman eds., 1995).

¹⁹ Michael Ignatieff, *State, Civil Society, and Total Institutions: A Critique of Recent Social Histories of Punishment*, 3 *CRIME & JUST.* 153, 156-57 (1981).

²⁰ Of course, we need to ask why judges do this. The theory proposed by Rusche and Kirchheimer suggests that they do so in the name of society-level social control. Perhaps a more straightforward explanation is that judges take into account the social costs of their decisions, and the costs could be greater on average when incarcerating an employed person (since, for example, he may provide for his family more than an unemployed person). Or given the correlation between unemployment and recidivism, judges may act more preemptively in these cases. Furthermore, some state guidelines, such as those in North Carolina, explicitly instruct judges to consider employment as a mitigating factor.

3. Demographic Theories

Demographic trends, especially changing age and race distributions, are also thought to influence the incarceration rate. The theories for age are relatively straight-forward. That age profiles and criminality are correlated is well known. But even controlling for crime rates, there may be a link between age profiles and incarceration patterns. First, while criminal behavior often starts in the late pre- and early-teen years, the juvenile justice system handles almost all offenders below age eighteen. Second, evidence suggests that adult courts are less willing to sentence the youngest offenders. Thomas Marvell and Carlisle Moody, for example, have produced results indicating that judges are more likely to lock up offenders between the ages of twenty-five to thirty-four than between eighteen and twenty-four.²¹ Changing age patterns may therefore shift prison populations, independently of their effects on crime.

Given the interplay in American society among race, crime, and incarceration, it should come as no surprise that the theories linking race and prison populations are more complex (and more extensively discussed) than those examining age. Three strands in particular deserve attention. The first is the most direct, focusing on race's influence over day-to-day decisions concerning arrest, prosecution, and sentencing. This can be thought of as a theory of "institutional racism," with racial bias manifesting itself in the quotidian tasks that define the criminal justice system. Police may be more willing to arrest, district attorneys more willing to prosecute, juries more willing to convict, and judges more willing to incarcerate minorities (and to do so for longer terms). This would produce a positive correlation between the sizes of a jurisdiction's minority and prison populations. So far, however, most empirical studies examining institutional racism have focused on its impact on the racial *distribution* of prison populations, not on the populations' actual *sizes*; these studies have been inconclusive.²² Thus the question of how institutional racism has influenced the size of the overall prison population remains relatively open.

The second strand emerges from the political theory, linking more punitive criminal policies to a strategic political backlash against the social changes of the 1960s, including the civil rights movement. Katherine Beckett provides a clear exposition of this position, arguing that political campaigns such as Barry Goldwater's 1964 presidential bid attempted to tie

²¹ Thomas B. Marvell & Carlisle E. Moody, *Age-Structure Trends and Prison Populations*, 25 J. CRIM. JUST. 115, 121 tbl.4 (1997) [hereinafter Marvell & Moody, *Age-Structure Trends*].

²² Robert J. Sampson & Janet L. Lauritsen, *Racial and Ethnic Disparities in Crime and Criminal Justice in the United States*, in ETHNICITY, CRIME, AND IMMIGRATION 311, 362 (Michael Tonry ed., 1997).

crime to political dissent, using attacks on the former to quash the latter.²³ Randall Kennedy similarly notes that among the many roles race has played in criminal justice, one has been as a “thinly veiled code” to signal opposition to the social transformation started in the 1960s, especially with respect to civil rights.²⁴

This argument may appear to track Garland’s “late modernity” theory (discussed more below), but it differs in important ways. Garland asserts that the state turned to law enforcement to justify its authority when it failed to provide an effective social welfare net; protection from crime was an effort by the government to gain legitimacy. In a sense, then, the second strand of the race theory is a cynical variant of Garland’s, claiming that the turn to criminal policy was not a sincere effort to justify the government’s role, but rather a political ploy to undo undesired policies.

The third strand is closely related to the crime theory. Advanced most strongly by Michael Tonry, it asserts that racial bias helped motivate (and continues to motivate) the war on drugs, and that this war has subsequently played a dominant role in the prison population surge.²⁵ Tonry argues that changing arrest patterns fueled the growth in prison populations, with race playing an important role in these changes. While it is widely accepted that the war on drugs has had a serious impact on minority (especially urban) communities, such a result can be blamed on racism only if it was foreseeable to the drug war’s architects. Tonry makes just this claim, arguing that the effects of a supply-side, prohibitionist (rather than a demand-side, risk-reduction) approach on urban minority communities should have been clear to those who designed the policies. Note, though, that Tonry’s argument is equally consistent with racial *indifference* and with racial *animus*.

Though this nation’s long-running problems with race have surely influenced prison populations, each of the three strands above suffers from important reservations. First, each theory, particularly that of institutional racism, has a hard time untangling the correlation between race and class. Not only may police, prosecutors, juries, judges, and parole boards treat whites better than blacks, but they may treat the rich better than the poor; even in the absence of any class bias, the rich may be better able to avoid prison (or longer sentences) because they can hire better lawyers. To the extent that race and class are correlated, isolating the effect of race may be difficult, especially with aggregate state-level data.

²³ Katherine Beckett asserts that “[c]ategories such as street crime and law and order were used in an attempt to heighten opposition to the civil rights movement.” KATHERINE BECKETT, *MAKING CRIME PAY* 32 (1997).

²⁴ RANDALL KENNEDY, *RACE, CRIME, AND THE LAW* 4 (1997).

²⁵ MICHAEL TONRY, *MALIGN NEGLECT* (1995).

Second, the actual effect of the political and rhetorical use of “crime” on prison populations is unmeasured. The route from political device to effective policy is long and complex. Not only is it often difficult for a legislature to pass a new law, but even once passed administrative intransigence can thwart meaningful implementation. As Theodore Caplow and Jonathan Simon note, “It is undeniable that some white Americans were pleased to see the benefits of the civil rights movement and affirmative action partly cancelled by the rise in the incarceration of African Americans, but there is no evidence that such bigotry drives the trend.”²⁶

And finally, while it is undeniable that the war on drugs has had a disparate racial impact, two concerns with Tonry’s argument deserve attention. First, as work by researchers as diverse as Randall Kennedy and Steven Levitt has indicated, the impact of the war on drugs may have fallen disproportionately on the black population, but so too did the harms from the drug epidemics—crack in particular.²⁷ Second, the effect of the war on drugs on overall prison populations is often overstated. Despite large numbers of arrests, relatively few drug offenders are sent to prison. Drug offenders make up only 20% of state prisoners; property offenders comprise 20%, and violent offenders 50%.²⁸ Even if every prisoner in 1998 whose primary offense was a drug charge were released, the total population would have been approximately 1 million instead of 1.3 million: that is still more than triple the population in 1977.²⁹

²⁶ Theodore Caplow & Jonathan Simon, *Understanding Prison Policy and Population Trends*, in PRISONS, *supra* note 9, at 91. It should be pointed out, though, that Caplow and Simon provide no evidence in support of their own point other than the absence of evidence for the other side.

²⁷ Randall Kennedy, for example, once stated in the context of the disparate treatment of crack and powder cocaine that “the principle problem facing African-Americans in the context of criminal justice today is not over-enforcement but *under*-enforcement of laws.” Randall Kennedy, *The State, Criminal Law, and Racial Discrimination*, 107 HARV. L. REV. 1255, 1259 (1994). Steven Levitt and his co-authors have developed an index of crack use that suggests that such use was highly correlated with negative social outcomes such as higher murder rates among young men, teenage pregnancies, and low-birth weight babies, at least during the 1980s. Roland G. Fryer, Paul S. Heaton, Steven D. Levitt & Kevin M. Murphy, *Measuring the Impact of Crack Cocaine* 26-28 (Nat’l Bureau of Econ. Research, Working Paper No. 11318, 2005).

²⁸ See U.S. Dep’t of Justice, Office of Justice Programs, Bureau of Justice Statistics—Prisons Statistics, <http://www.ojp.usdoj.gov/bjs/prisons.htm> (last visited Oct. 30, 2006).

²⁹ There are two important caveats to this claim. First, a drug dealer who kills in the midst of a drug-market turf war is classified as a violent offender, not a drug-crimes offender. To the extent that the war on drugs destabilized drug markets and led to greater violence and more violent offenders, the war on drugs influences non-drug crime trends as well. Second, drug convictions themselves may not lead to incarceration, but they can count as prior felonies for the purpose of recidivist enhancements. Thus the time served by felons convicted of violent and property crimes is determined in part by the war on drugs.

Race has surely played a role in prison growth. The magnitude of its role, and where it operates, however, remain open questions.

4. *The Political Theories*

The ascendant theory concerning recent prison growth ties it to changes in the political culture of the United States over the past thirty years. The theory actually consists of three closely related claims. The first argues that increasingly punitive criminal policies reflect a rightward drift in political culture and a backlash against a perceived-as-ineffectual welfare state. The second is a more cynical variant of the first, stating that politicians from both sides of the aisle use crime scares to lure away (or keep) voters. And the third posits that crime policy results from the interaction of moral panics and cyclic views on crime.

David Garland has developed perhaps the most ambitious exposition of the rightward-drift theory.³⁰ During the 1970s, two forces intersected: on the one hand, commitment to the rehabilitative ideal in criminal sentencing (and elsewhere) collapsed, and on the other hand a series of profound cultural changes (the oil shocks, the decline of industrialization, changing marriage patterns and gender roles, increased suburbanization, the rise of the television, expanding civil rights, and growing moral relativism) shifted the political views of the populace to the right. This political realignment caused the criticisms of the rehabilitative ideal to induce not small adjustments in penal policy but a sea change. As Garland notes, "Broad social classes that had once supported welfare state policies . . . came to think and feel about the issues quite differently [in the 1980s and 1990s]."³¹ The political readjustment undermined support for the welfare state and led to the introduction of crime control as a substitute for welfare.³² Perceived as no longer successfully *providing* for their citizens, governments sought legitimacy by *protecting* people *from* risk, particularly crime. At the very least, tough-on-crime policies gave the perception that "something is being done," regardless of their actual effect.³³

The second theory argues that crime is an effective electioneering device, suggesting that tough-on-crime rhetoric does not necessarily reflect the ideal views of the candidate. Michael Tonry, for example, argues that "[i]n times of anticrime hysteria, the safest position on crime issues may be

³⁰ DAVID GARLAND, *THE CULTURE OF CONTROL* (2001).

³¹ *Id.* at 75-76.

³² *Id.* at 100. Garland states that "[i]f the watchwords of post-war social democracy had been *economic control* and *social liberation*, the new politics of the 1980s put in place a quite different framework of *economic freedom* and *social control*." *Id.*

³³ *Id.* at 133-35; see also Markus D. Dubber, *Recidivist Statutes as Arational Punishment*, 43 BUFF. L. REV. 689, 719 (1995).

the preemptive strike.”³⁴ Theodore Caplow and John Simon similarly claim that “[i]t is widely accepted that political candidates for statewide office must establish themselves as favoring more severe punishments to stand a chance of election.”³⁵ They assert that this is the result of a shift in political debates over the past thirty years from “traditional class and regional conflicts, towards conflicts about ‘values,’” and that crime is an effective campaign topic because it is one of the few issues that commands consensus.³⁶ Katherine Beckett uses survey data to claim that public concern about crime *lags*, rather than *leads*, politicians’ rhetoric about its importance, implying that politicians stoke the fear of crime as an electioneering device.³⁷

And the final theory is Michael Tonry’s claim that current penal policies arise from a complex relationship between cycles in sensibilities and moral panics.³⁸ At its heart, Tonry’s theory is one of legal endogeneity. Politicians do not observe a given societal problem—in this case, crime—and then set about adjusting the laws to fix it. Instead, general sensibilities and values change in response to the problem, and it is these changes that solve the problem. The legal changes occur at the same time, but in response to the cycle in sensibilities, not the social problem. Thus, rising crime rates harden people’s views of crime and criminals, and these changing social views help drive down crime rates. Legislators then act with a lag to pass tougher anti-crime measures, but the correlation between stricter laws and falling crime is (more or less) spurious. That the timing of laws is endogenous is well-known; Tonry makes a plausible argument that sentencing laws’ correlation with *crime* is at least somewhat coincidental.

³⁴ TONRY, *supra* note 25, at 180.

³⁵ Caplow & Simon, *supra* note 26, at 70; *see also* JONATHAN SIMON, GOVERNING THROUGH CRIME ch. 3 (2007).

³⁶ Caplow & Simon, *supra* note 26, at 79. Among the reasons for this consensus, they argue, is the bombardment of images on TV involving crime. *Id.* at 87-89. It is not immediately clear, however, that American society focuses more today on sensationalist news than it did in the past. Caplow and Simon cite the O.J. Simpson trial, but it is important to remember that the O.J. trial was at least the fifth “Trial of the Century” in the twentieth century, after Sacco and Venzetti, Leopold and Loeb, the Lindbergh Baby Case, and the Rosenbergs. Major cities at the turn of the century and before each had several tabloid newspapers that sought to out-sensationalize each other, at times publishing several editions a day. Television is blamed for introducing a qualitative change, but this point is only asserted, never proven, and it is perhaps complicated by the history of tabloid journalism.

³⁷ BECKETT, *supra* note 23, at 16.

³⁸ TONRY, *supra* note 8.

That the changes in sentencing laws had no impact on *incarceration*, however, seems harder to justify.³⁹

Despite their widespread appeal, however, these political theories suffer from several defects. Zimring and Hawkins are among the few scholars to pay attention to them, leveling three central criticisms: political debate is often symbolic, much tough-on-crime campaigning is waged by candidates for national office while most crime policy is local, and bureaucrats—not politicians—are generally in charge of implementing changes in policy.⁴⁰ Indeed, the studies advancing the political theory rarely if ever discuss the public-choice and principal-agent problems inherent in criminal law and policy. A politician may make a (sincere) promise to pass tough anti-crime legislation, for example, but as public-choice and interest-group theories demonstrate, the route from promise to passage to implementation is treacherous.

Beckett briefly addresses this issue, discussing the rising strength of the criminal justice lobbies, especially that of correctional officers in California.⁴¹ She does not, however, discuss the relative strength of countervailing anti-incarceration lobbies. These need not be defendants' rights groups (which are most likely weak), but may be any interest group—such as the education lobby—whose access to funding is threatened by prison growth. Political scientists who examine state politics closely, in fact, note that tough-on-crime groups such as prison guard unions and victims' rights groups, despite the attention they receive in the press, are rarely among the most powerful of groups; even their fellow travelers, such as gun rights groups, are generally weaker than educational lobbies and similar organizations with competing budgetary interests.⁴²

Moreover, even if a law is passed, there is no guarantee that it will be meaningfully enforced. As Zimring and Hawkins demonstrate, the decentralization of enforcement among police, prosecutors, and judges helps each shirk its respective duties.⁴³ Garland considers this possibility as

³⁹ Note that Beckett's findings challenge Tonry's sensibilities hypothesis: to the extent that politicians move first and only then do people seem to care about crime, Tonry's claim that politicians are *responding* to changing sensibilities appears incorrect.

⁴⁰ ZIMRING & HAWKINS, *supra* note 3, at 125-30.

⁴¹ BECKETT, *supra* note 23, at 97-101.

⁴² A good review of this literature is provided by Clive S. Thomas & Ronald J. Hrebenar, *Interest Groups in the States*, in *POLITICS IN THE AMERICAN STATES* 100 (Virginia Gray & Russell L. Hanson eds., 2004).

⁴³ They note that

[n]o single agency need take the blame [for a low level of criminal convictions]. What looks like a high level of chronic backbiting and buck-passing in the system [between police, prosecutors, and judges] is really a particular ecology of nonaccountability that works to shield the system from political responsibility on a more direct basis.

well, pointing out that the goals of these “administrative actors” are not those of politicians. In fact, administrative actors view the politicians as “a troublesome, external force, with different interests and agendas, rather than an integral part of the permanent organization,” and whose policies are not necessarily favored by the administrative actors.⁴⁴ And there is evidence that administrative actors in fact do not necessarily follow the policies handed down by politicians. Joanna Shepherd, for example, notes that while twenty-six states and the federal government passed three-strike laws in the 1990s, only California has used the law in a meaningful sense; as many as 90% of all criminals sentenced under a three-strike law have been sentenced in California.⁴⁵ And New York State prosecutors have often actively evaded the strict sentencing requirements of the Rockefeller Drug Laws.⁴⁶

That said, political shifts over the past thirty years have surely increased prison populations. But the theory must be modeled carefully. Shifting ideologies influence the behavior of many parties—not just legislators, but district attorneys, elected (and even appointed) judges, mayors, governors, even parole boards. And the actions taken by these actors can exhibit complicated dynamics.⁴⁷ Unfortunately, as I will show below, the approaches used to examine the political theory’s implications have generally failed to address these complexities.

5. Four Other Important Factors

While the factors central to the four schools have received much of the attention, at least four other relevant variables have been contemplated as well. The first is the deinstitutionalization of the mentally ill that has taken place since the 1970s. Steven Raphael notes that mental hospital inpatient rates generally declined over the period from 1971 to 1996, sometimes sharply (in Texas, for example, they fell from nearly 325 per 100,000 residents in 1971 to around 50 per 100,000 residents in 1996), and his results suggest that deinstitutionalization was responsible for between

ZIMRING & HAWKINS, *supra* note 3, at 127.

⁴⁴ GARLAND, *supra* note 30, at 111.

⁴⁵ Joanna M. Shepherd, *Fear of the First Strike: The Full Deterrent Effect of California’s Two- and Three-Strike Legislation*, 31 J. LEGAL STUD. 159, 159-60 (2002).

⁴⁶ See, e.g., MARC MAUER, THE RACE TO INCARCERATE (1999).

⁴⁷ Governor George Pataki of New York, for example, appointed several conservatives to long terms on the state parole board in the waning days of his last term. Sam Roberts, *Pataki Creates Legacy, Filling Scores of Posts*, N.Y. TIMES, Sept. 15, 2006, at A1. Thus even though Pataki was succeeded shortly thereafter by a Democrat, Eliot Spitzer, the effects of his conservative views linger.

48,000 to 148,000 of the prisoners incarcerated in 1996, or between 4.5% and 14% of the total prison population.⁴⁸

Two recent articles by Bernard Harcourt have demonstrated even more starkly the importance of deinstitutionalization.⁴⁹ Harcourt argues that focusing solely on prison as a form of incapacitation is incomplete: mental hospitals incapacitate as well and should therefore be included in any analysis of incapacitation's effect on crime. He shows that if we define "total commitments" as the sum of hospital confinements and prison populations, the U.S. has only just recently returned to the levels of per capita incapacitation that it experienced in the 1950s.⁵⁰ That some of the resurgence in confinement is likely due to substituting imprisonment for hospitalization indicates the need to account for deinstitutionalization.

A second important factor is prison capacity. Some researchers have suggested that not only are new prisons built in anticipation of greater prison populations, but that defendants are more likely to be incarcerated the more cell space is available: capacity can induce incarceration.⁵¹ The central idea is that, on the margin, a judge is more willing to incarcerate—and to do so more severely—the more space he thinks is available. Such a theory assumes that judges are aware of prison capacity in their jurisdictions and that they have the discretion to adjust their sentencing accordingly. The first seems reasonable, since judges certainly have contacts with various relevant actors in the criminal justice system. The second depends on a state's sentencing regime.

The third factor is related to capacity, namely federal court orders imposing population caps and other restrictions in response to severe overcrowding. The 1965 case of *Talley v. Stephens*⁵² marked the first foray by a federal court into state prison oversight. Ultimately forty-seven states

⁴⁸ Steven Raphael, *The Deinstitutionalization of the Mentally Ill and Growth in the U.S. Prison Populations: 1971 to 1996* 11-12, 14 (Sept. 2000), available at <http://socrates.berkeley.edu/~raphael/raphael2000.pdf> (unpublished manuscript) [hereinafter Raphael, *Deinstitutionalization*].

⁴⁹ Bernard E. Harcourt, *From the Asylum to the Prison: Rethinking the Incarceration Revolution*, 84 TEX. L. REV. 1751 (2006); Bernard E. Harcourt, *From the Asylum to the Prison: Rethinking the Incarceration Revolution—Part II: State Level Analysis* (Univ. of Chicago, John M. Olin Law & Econ. Working Paper No. 335, Mar. 2007), available at <http://ssrn.com/abstract=970341>.

⁵⁰ To some extent, then, what stands out is not the boom from the 1970s to the 2000s, but the precipitous decline in total commitments in the 1950s and 1960s.

⁵¹ See U.S. DEP'T OF JUSTICE, NAT'L INST. OF JUSTICE, *AMERICAN PRISONS AND JAILS: POPULATION TRENDS AND PREDICTIONS* (1980); ZIMRING & HAWKINS, *supra* note 3.

⁵² 247 F. Supp. 683 (E.D. Ark. 1965) (temporarily enjoining the use of corporal punishment on inmates, enjoining prison authorities from denying inmates access to courts, and enjoining prison authorities from retaliating against those inmates who file legal complaints).

and the District of Columbia have found their prison systems subject to judicial orders, all but seven to a significant degree. Most important for the question here, approximately twelve states have had their entire prison systems—rather than particular facilities—declared unconstitutional due (at least) to overcrowding and, as a result, have been subject to extensive federal judicial regulation; the subsequent population caps slowed population growth in those states.⁵³ It is thus important to properly control for such restrictions on prison growth.

The final important factor is the general financial and budgetary health of the state in question. Constructing and maintaining prisons is a costly endeavor, so wealthier states are in a better position to expand and maintain capacity. On the margin states are more likely to invest in the criminal justice system the better their economies and the more tax revenues they collect. Two recent studies by the Vera Institute of Justice indicate that states have recently begun looking for ways to slow, if not reverse, the growth of prisons in light of tighter post-2001 budgets.⁵⁴ In 2002, for example, twenty-five states cut expenditures on corrections; only higher education spending was cut in more (twenty-nine).⁵⁵ This is consistent with a positive relationship between a state's economic health and its incarceration rate.

The theories discussed above all set forth credible claims about the forces influencing prison population growth rates. But theory alone cannot determine the absolute or relative importance of these factors. For that, empirical analysis is required, to which I now turn.

⁵³ There is some disagreement here. Relying on data from the ACLU, Steven Levitt identifies twelve such states: Alabama, Alaska, Arkansas, Delaware, Florida, Mississippi, New Mexico, Oklahoma, Rhode Island, South Carolina, Tennessee, and Texas. Steven D. Levitt, *The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Legislation*, 111 Q.J. ECON. 319, 326 (1996). Margo Schlanger, however, using data from the Bureau of Justice Statistics' Census of State and Federal Correctional Facilities, lists fifteen states as being subject to system-wide court orders in 1995; while she does not include Arkansas, Florida, Oklahoma, and Tennessee, she does include the District of Columbia, Georgia, Kansas, Louisiana, New Hampshire, North Carolina, South Dakota, and Utah. Margo Schlanger, *Civil Rights Injunctions over Time: A Case Study of Jail and Prison Court Orders*, 81 N.Y.U. L. REV. 550, 577 (2006). Reconciling these claims is an important future project, especially since, as Levitt demonstrates, system-wide court orders provide an effective device for breaking the endogenous relationship between incarceration and crime. See Pfaff, *Prison Population Growth*, *supra* note 1.

⁵⁴ ROBIN CAMPBELL, DOLLARS & SENTENCES: LEGISLATORS' VIEWS ON PRISONS, PUNISHMENT, AND THE BUDGET CRISIS (2003); DANIEL WILHELM & NICHOLAS TURNER, ISSUES IN BRIEF: IS THE BUDGET CRISIS CHANGING THE WAY WE LOOK AT SENTENCING AND INCARCERATION? (2002).

⁵⁵ WILHELM & TURNER, *supra* note 54, at 2.

III. FINDINGS OF CURRENT STUDIES

In 1991, Franklin Zimring and Gordon Hawkins lamented the lack of empirical research on the scale of incarceration. They noted that only two theories—Rusche and Kirchheimer's unemployment claim and Blumstein's Weberian "homeostatic" hypothesis—had been tested, and both had (to them) come up short.⁵⁶ Since Zimring and Hawkins's complaint, however, the continued growth in the U.S. prison population has encouraged many researchers to explore empirically the causes of the boom.

A. THE PRIMARY STUDIES

Over the past twenty-five years, many investigators have attempted to empirically validate at least some, if not most, of the theories set forth above. In particular, an examination of every empirical article on prison growth published in fifty-nine journals⁵⁷ in criminology, economics, law, political science, and sociology between January 1990 and May 2006 turned up twenty major studies.⁵⁸ Table 1 lists the journals considered.

⁵⁶ ZIMRING & HAWKINS, *supra* note 3, at ch. 1. That Rusche and Kirchheimer's neo-Marxist approach lacks strong empirical support does not mean unemployment has no effect on prison populations, just that it is not the sole, or perhaps even the primary, engine.

⁵⁷ I did not limit my search to the fifty-nine journals listed in Table 1. I also looked at the bibliography of each potentially relevant article from those journals for other viable studies, and did the same for any such studies that I thus came across. Some of the twenty articles therefore do not come from the fifty-nine primary journals.

⁵⁸ The twenty studies are: Thomas M. Arvanites, *Increasing Imprisonment: A Function of Crime or Socio-Economic Factors?*, 17 AM. J. CRIM. JUST. 19 (1992); Thomas M. Arvanites & Martin A. Asher, *State and County Incarceration Rates: The Direct and Indirect Effects of Race and Inequality*, 57 AM. J. ECON. & SOCIOL. 207 (1998); Thomas M. Arvanites & Martin A. Asher, *The Direct and Indirect Effects of Socio-Economic Variables on State Imprisonment Rates*, 7 CRIM. JUST. POL'Y REV. 27 (1995); Katherine Beckett & Bruce Western, *Governing Social Marginality: Welfare, Incarceration, and the Transformation of State Policy*, 3 PUNISHMENT & SOC'Y 43 (2001); David F. Greenberg & Valerie West, *State Prison Populations and Their Growth 1971-1991*, 39 CRIMINOLOGY 615 (2001); David Jacobs & Jason T. Carmichael, *The Politics of Punishment Across Time and Space: A Pooled Time-Series Analysis of Imprisonment Rates*, 80 SOCIAL FORCES 61 (2001); David Jacobs & Ronald Helms, *Towards a Political Model of Incarceration: A Time-Series Examination of Multiple Explanations for Prison Admission Rates*, 102 AM. J. SOC. 323 (1996) [hereinafter Jacobs & Helms, *Political Model*]; David Jacobs & Ronald Helms, *Towards a Political Sociology of Punishment: Politics and Changes in the Incarcerated Population*, 30 SOC. SCI. RES. 171 (2001) [hereinafter Jacobs & Helms, *Political Sociology*]; Gloria T. Lessan, *Macro-Economic Determinants of Penal Policy: Estimating the Unemployment and Inflation Influences on Imprisonment Rate Changes in the United States, 1948-1985*, 16 CRIME, L. & SOC. CHANGE 177 (1991); Yair Listokin, *Does More Crime Mean More Prisoners? An Instrumental Variables Approach*, 46 J.L. & ECON. 181 (2003); Thomas B. Marvell, *Sentencing Guidelines and Prison Population Growth*, 85 J. CRIM. L. & CRIMINOLOGY 696 (1995); Marvell & Moody, *Age-Structure Trends*, *supra* note 21; Thomas B. Marvell & Carlisle E. Moody, *Determinate Sentencing and Abolishing Parole: The Long-*

Tables 2.1 and 2.2 provide a summary of the findings from these studies; Tables 3.1 and 3.2 distill the essential features of Tables 2.1 and 2.2, respectively. To be included in note 58, a study had to satisfy three conditions: it had to focus on state-level data, be written after 1990 (once the upward trend was sufficiently established), and control for at least several of the posited theories simultaneously.⁵⁹ These criteria exclude at least two types of studies, namely those looking at offender-level data and those solely decomposing growth into its constituent parts (changes in crime, in arrests per crime, in prosecution per arrest, and so on). Parts III.B and III.C, respectively, briefly discuss these methods.

Term Impacts on Prisons and Crime, 34 CRIMINOLOGY 107 (1996) [hereinafter Marvell & Moody, *Determinate Sentencing*]; Raymond J. Michalowski & Michael A. Pearson, *Punishment and Social Structure at the State Level: A Cross-Sectional Comparison of 1970 and 1980*, 27 J. RES. CRIME. & DELINQ. 52 (1990); Sean Nicholson-Crotty, *The Impact of Sentencing Guidelines on State-Level Sanctions: An Analysis over Time*, 50 CRIME & DELINQ. 395 (2004); Raphael, *Deinstitutionalization*, *supra* note 48; Kevin B. Smith, *The Politics of Punishment: Evaluating Political Explanations of Incarceration Rates*, 66 J. POLITICS 925 (2004); Jon Sorensen & Don Stemen, *The Effect of State Sentencing Policies on Incarceration Rates*, 48 CRIME & DELINQ. 456 (2002); Thomas D. Stucky, Karen Heimer & Joseph B. Lang, *Partisan Politics, Electoral Competition and Imprisonment: An Analysis of States over Time*, 43 CRIMINOLOGY 211 (2005); and William A. Taggart & Russell G. Winn, *Imprisonment in the American States*, 74 SOC. SCI. Q. 736 (1993).

⁵⁹ With the exception of those by Marvell, Moody, and Listokin, the articles considered here not only control for several theories at once but seek to test and measure multiple causal theories simultaneously. As I discuss in Part IV.B, multiple controls are essential, but each casual theory should be tested independently. See also Pfaff, *Prison Population Growth*, *supra* note 1.

Table 1
Journals Reviewed (January 1990-May 2006)

Criminology	Economics	Law	Political Science	Sociology
<ul style="list-style-type: none"> • Am. J. Crim. Just. • Crime & Delinq. • Crime & Just. • Crim. Just. Pol'y Rev. • Crim. Just. Rev. • Criminology • Criminology & Pub. Pol'y • Int'l Crim. Just. Rev. • J. Crime & Just. • J. Crim. L. & Criminology • J. Res. Crime & Delinq. • J. Quant. Criminology • Just. Q. • Prison J. • Punishment & Soc'y 	<ul style="list-style-type: none"> • Am. Econ. Rev. • Econ. J. • Econ. Pol'y • J. Econ. Literature • J. Econ. Persp. • J. Pol. Econ. • Q. J. Econ. • Rev. Econ. Stud. • Rev. Econ. & Stat. 	<ul style="list-style-type: none"> • J.L. & Econ. • J. Legal Stud. • Sup. Ct. Rev. • Cal. L. Rev. • Columbia L. Rev. • Cornell L. Rev. • Duke L.J. • Harv. L. Rev. • Harv. C.R.-C.L. L. Rev. • Minn. L. Rev. • NYU L. Rev. • Ohio St. J. Crim. L. • S. Cal. L. Rev. • Stan. L. Rev. • Tex. L. Rev. • UCLA L. Rev. • U. Chi. L. Rev. • U. Pa. L. Rev. • Vand. L. Rev. • Va. L. Rev. • Yale L.J. 	<ul style="list-style-type: none"> • Am. J. Pol. Sci. • Am. Pol. Sci. Rev. • J. Pol. • Legis. Stud. Q. • Pol. Res. Q. • Pol. Sci. Q. 	<ul style="list-style-type: none"> • Am. J. Soc. • Am. Soc. Rev. • Soc. Persp. • Soc. Methodology • Soc. Forces

Table 2.1
Studies Examining Prison Population/Incarceration Rates

		Michalowski & Pearson	Lessan ^(b)	Arvanites	Taggart & Winn	Arvanites & Asher
Study Year		1990	1991	1992	1993	1995
Dependent Variable		Rate	Rate	Rate ^(d)	Rate	Rate
No. of States in Sample		50	NTS	51	48	49/51
Years		70, 80 ^(a)	48-85	80, 88 ^(a)	84	90
Specifications		1/year	2	2/year	1	6 ^(g)
Variables						
Mechanistic/Crime Theory	Crime			+++ ^(e)		+++ ^(e)
	Violent Crime	++	--	+++ ^(e)	++	+++ ^(e)
	Property Crime					
	Drug Arrests					
	All Arrests					+++ ^(c)
	Probation Rate				+	
	Admits from Cond'l Release					
	Change in Pop./ Release Ratio		+			
	Police					
	Capacity of Prisons				-	
	Capacity of Alternatives				+	
	Mental Hosp. Rate					
	Prison Admissions					++
	Prison Releases					--
	Lagged Dependent Variable					
	2nd Lagged Depend't Variable					
Economic Theory	Unemployment	+	++ ^(c)	+		
	Employment					
	Poverty			NR	-	
	Personal Income					
	Non-White Median Income					
Dis- order	Inflation Rate		++			
	Var'n in Income					
	Gini Coefficient					NR
	Out-of-Wedlock Births					
	Divorce Rate					

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Michalowski & Pearson	Lessan	Arvanites	Taggart & Winn	Arvanites & Asher
Study Year		1990	1991	1992	1993	1995
Variables						
Demographics	% Pop., 14-24					
	% Pop., 18-29		+			
	% Pop., 25-34					
	% Pop., 35-45					
	% Male, 14-25					
	% Black			+++		
	% Young Black Male				+	
	% Latino					
	% Minority					+++
	Total Population					
	Urbanization	-				
	Density					
	Change in Density				-	
Politics	Conservatism					
	Republican Legislature				(f)	
	Religious Fundamentalism					
	Republican Pres.					
	Clinton Presidency					
	Duration of Republican Presidential Term					
	Republican Governor					
	Marijuana Decriminalization					
	Southern State	++				
	Welfare Spending		-			
	Educ'n Spending					
	Per Capita GDP					
	State Econ. Growth					
	State Revenue	NR				
	Presidential Election Year					
	Gubernatorial Election Year					

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Michalowski & Pearson	Lessan	Arvanites	Taggart & Winn	Arvanites & Asher
Study Year		1990	1991	1992	1993	1995
Variables						
Legal Factors	Structured Sentencing				—	
	Revenue-Focused Guidelines					
	Guidelines Without Revenue Focus					
	Voluntary Guidelines					
	Three Strike Laws					
	Court Order					
Endogeneity Control		No	Yes	No	No	No
Serial Correl. Control		■	Yes	■	■	■
Heterosked. Control		Unk	Unk	Unk	Unk	Unk
Diff-in-Diff Control		■	■	■	■	■

Note: The titles of the studies are provided in note 58. "Rate" means the article used the incarceration rate as its dependent variable, "NTS" stands for national-level time series rather than a panel of states, and "specifications" provides the number of models estimated in a given article. A coefficient that is positive and significant in all specifications is indicated by "+++" (and "— — —" if negative and significant in all); if positive in all specifications and significant in at least half, "++" (and likewise "— —" if negative); if positive in all specifications but significant in fewer than half, by "+" (and "—" for negative). If the sign is inconsistent across specifications, the coefficient is labeled "NR" (for "Not Robust"). "Endogeneity Control" indicates whether the article controlled for the endogenous relationship between prison populations and crime, "Serial Correl. Control" whether the article explicitly corrected for serial correlation, "Heterosked. Control" whether it explicitly addressed issues of heteroskedasticity, and "Diff-in-Diff Control" whether it accounted for the serial correlation problems raised by difference-in-difference estimators discussed in Bertrand et al., *infra* note 95. "■" indicates that the issue was immaterial for that paper, and "Unk" means that it is unclear from the article if the regressions control for the particular issue. (a) Each year is tested separately. (b) For Lessan, all variables except inflation are differences, not levels. (c) One specification looks at white unemployment, the other black. (d) Rate is with respect to population between the ages of eighteen and sixty-five, not population as a whole. (e) Terms are each used in separate regressions. (f) Scale not properly defined, meaning it is not possible to know implications of a positive or negative effect. (g) Two variations are run using crime, two using violent crime, and two using arrests.

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Marvell	Marvell & Moody	Marvell & Moody	Arvanites & Asher	Raphael
Study Year		1995	1996	1997	1998	2000
Dependent Variable		Ln(Rate)	Ln(Rate)	Ln(Rate)	Rate	Rate
No. of States in Sample		9	50	50	45/51	51
Years		76-93	72-93	71-94	93	71-96
Specifications		1	1	1	1/yr	3
Variables						
Mechanistic/Crime Theory	Crime				++ ^(e)	
	Violent Crime				++ ^(e)	+++
	Property Crime					---
	Drug Arrests					
	All Arrests					
	Probation Rate					
	Admits from Cond'l Release					
	Change in Pop./ Release Ratio					
	Police		+			
	Capacity of Prisons					
	Capacity of Alternatives					
	Mental Hosp. Rate					-
	Prison Admissions					
	Prison Releases					
Economic Theory	Lagged Depend't Variable			++		
	2nd Lagged Depend't Variable			-		
	Unemployment					
	Employment	-	-	+		-
	Poverty		+	+		NR
	Personal Income	+	-	--		NR
Dis- order	Non-White Median Income					
	Inflation Rate					
	Var'n in Income					
	Gini Coefficient				NR	
	Out-of-Wedlock Births					
	Divorce Rate					

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Marvell	Marvell & Moody	Marvell & Moody	Arvanites & Asher	Raphael
Study Year		1995	1996	1997	1998	2000
Variables						
Demographics	% Pop., 14-24	+(ⁱ)	+(ⁱ)	+(ⁱ)		(ⁱ)
	% Pop., 18-29					NR
	% Pop., 25-34	++	++	+		NR
	% Pop., 35-45			++		NR
	% Male, 14-25					
	% Black		++			---
	% Young Black Male					
	% Latino					
	% Minority				+	
	Total Population Urbanization		-		++	NR
	Density					
Change in Density						
Politics	Conservatism					
	Republican Legislature					
	Religious Fundamentalism					
	Republican Pres.					
	Clinton Presidency					
	Duration of Republican Presidential Term					
	Republican Governor					
	Marijuana Decriminalization					
	Southern State					
	Welfare Spending					
	Educ'n Spending					
Per Capita GDP						
State Econ. Growth						
State Revenue						
Presidential Election Year						
Gubernatorial Election Year						

Note: The titles of the studies are provided in note 58. “Rate” means the article used the incarceration rate as its dependent variable, “NTS” stands for national-level time series rather than a panel of states, and “specifications” provides the number of models estimated in a given article. A coefficient that is positive and significant in all specifications is indicated by “+++” (and “---” if negative and significant in all); if positive in all specifications and significant in at least half, “++” (and likewise “--” if negative); if positive in all specifications but significant in fewer than half, by “+” (and “-” for negative). If the sign is inconsistent across specifications, the coefficient is labeled “NR” (for “Not Robust”). “Endogeneity Control” indicates whether the article controlled for the endogenous relationship between prison populations and crime, “Serial Correl. Control” whether the article explicitly corrected for serial correlation, “Heterosked. Control” whether it explicitly addressed issues of heteroskedasticity, and “Diff-in-Diff Control” whether it accounted for the serial correlation problems raised by difference-in-difference estimators discussed in Bertrand et al., *infra* note 95. “■” indicates that the issue was immaterial for that paper, and “Unk” means that it is unclear from the article if the regressions control for the particular issue. (e) Terms are each used in separate regressions. (h) Effect depends on type of guidelines adopted. (i) Age here is 18-24, not 18-29. (j) Ages are < 15 (++) , 15-17 (---), and 18-24 (++) .

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Beckett & Western	Beckett & Western	Greenberg & West	Greenberg & West	Jacobs & Helms
Study Year		2001	2001	2001	2001	2001
Dependent Variable		Rate	Rate	Rate	Change	DD(Rate) ⁽ⁿ⁾
No. of States in Sample		44	44	49	49	NTS
Years		75,85,95	75,85,95	71,81,91	71,81,91	53-98
Specifications		4	4	3	2	9 ^(o)
Variables						
Mechanistic/Crime Theory	Crime					++
	Violent Crime	++	NR	+++	++	
	Property Crime	--	NR	++	+	
	Drug Arrests			NR	+	
	All Arrests					
	Probation Rate					
	Admits from Cond'l Release					
	Change in Pop./ Release Ratio					
	Police					
	Capacity of Prisons					
	Capacity of Alternatives					
	Mental Hospital Rate					
	Prison Admissions Prison Releases					
Economic Theory	Lagged Depend't Var.					
	2nd Lagged Depend't Var.					
	Unemployment	NR	NR	NR ^(s)	+++	-
	Employment					
	Poverty	+	NR			
	Personal Income					
	Non-White Median Income					
Dis- order	Inflation Rate					
	Var'n in Income					
	Gini Coefficient			NR	NR	++
Dis- order	Out-of-Wedlock Births					
	Divorce Rate					

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Beckett & Western	Beckett & Western	Greenberg & West	Greenberg & West	Jacobs & Helms
Study Year		2001	2001	2001	2001	2001
Variables						
Demographics	% Pop., 14-24					
	% Pop., 18-29					
	% Pop., 25-34					
	% Pop., 35-45					
	% Male, 14-25					
	% Black	++		++ ^(k)	+++ ^(k)	
	% Young Black Male					
	% Latino		NR	-	-	
	% Minority					+
	Total Population					
	Urbanization	NR	NR	-	NR	
Politics	Density					
	Change in Density					
	Conservatism			+++	+++	
	Republican Legislature	++	NR			
	Religious Fundamentalism			++	NR	
	Republican Pres.					+++
	Clinton Presidency					+++
	Duration of Republican Presidential Term					+++
	Republican Governor			NR	-	
	Marijuana Decriminalization Southern State			NR ^(l)	---	
	Welfare Spending	-	--	NR ^(l)	--	
	Educ'n Spending					
	Per Capita GDP					NR
	State Econ. Growth					
	State Revenue	NR	NR	+++	NR ^(m)	
	Presidential Election Year					+++
	Gubernatorial Election Year					

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Beckett & Western	Beckett & Western	Greenberg & West	Greenberg & West	Jacobs & Helms
Study Year		2001	2001	2001	2001	2001
Variables						
Legal Factors	Structured Sentencing			--	NR	
	Revenue-Focused Guidelines					
	Guidelines Without Revenue Focus					
	Voluntary Guidelines					
	Three Strike Laws					
	Court Order			-	+	
Endogeneity Control		No	No	No	No	No
Serial Correl. Control		No	No	Yes	Yes	Yes
Heterosked. Control		Unk	Unk	Yes	Yes	Yes
Diff-in-Diff Control		■	■	No	No	■

Note: The titles of the studies are provided in note 58. "Rate" means the article used the incarceration rate as its dependent variable, "NTS" stands for national-level time series rather than a panel of states, and "specifications" provides the number of models estimated in a given article. A coefficient that is positive and significant in all specifications is indicated by "+++" (and "--" if negative and significant in all); if positive in all specifications and significant in at least half, "++" (and likewise "--" if negative); if positive in all specifications but significant in fewer than half, by "+" (and "-" for negative). If the sign is inconsistent across specifications, the coefficient is labeled "NR" (for "Not Robust"). "Endogeneity Control" indicates whether the article controlled for the endogenous relationship between prison populations and crime, "Serial Correl. Control" whether the article explicitly corrected for serial correlation, "Heterosked. Control" whether it explicitly addressed issues of heteroskedasticity, and "Diff-in-Diff Control" whether it accounted for the serial correlation problems raised by difference-in-difference estimators discussed in Bertrand et al., *infra* note 95. "■" indicates that the issue was immaterial for that paper, and "Unk" means that it is unclear from the article if the regressions control for the particular issue. (k) The variable is (%black)^{1/3}. (l) The change in sign arises only in the equation interacting the particular variable with year dummies; the base value changes sign, but the interaction terms are generally of the same sign as the other specifications and larger than the base value. (m) The change in state revenue (as opposed to its level effect) is consistently positive and significant. (n) "DD" stands for second-difference. "Ln(AveRt)" means the natural log of the average incarceration rate across two years (1971-1972, 1981-1982, and 1991-1992). (o) All specifications include measures of Republican presidencies, Gini coefficients, and crime rates. Most other variables appear in only one specification each.

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Jacobs & Carmichael	Sorensen & Stemen	Nicholson- Crotty	Smith
Study Year		2001	2002	2004	2004
Dependent Variable		Ln(AveRt)	Rate	Rate	Rate
No. of States in Sample		50	50	50	49
Years		71,81,91	96	75-98	80-95
Specifications		8 ^(p)	1	1	1
Variables					
Mechanistic/Crime Theory	Crime		++	+	
	Violent Crime	+			+
	Property Crime				+
	Drug Arrests				
	All Arrests				
	Probation Rate				-
	Admits from Cond'l Release				
	Change in Pop./Release Ratio				
	Police				
	Capacity of Prisons				
	Capacity of Alternatives				
	Mental Hosp. Rate				
	Prison Admissions				
	Prison Releases				
	Lagged Dependent Variable				++
	2nd Lagged Dependent Variable				
Economic Theory	Unemployment	NR		-	+
	Employment				
	Poverty				-
	Personal Income	+		-	
	Non-White Median Income				
	Inflation Rate				
Dis-order	Var'n in Income				
	Gini Coefficient	NR			+
	Out-of-Wedlock Births				
	Divorce Rate				+

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Jacobs & Carmichael	Sorensen & Stemen	Nicholson- Crotty	Smith
Study Year		2001	2002	2004	2004
Variables					
Demographics	% Pop., 14-24			+(s)	
	% Pop., 18-29				
	% Pop., 25-34			++	
	% Pop., 35-45				
	% Male, 14-25				
	% Black	++	++	++	++
	% Young Black Male				
	% Latino	+			
	% Minority				
	Total Population			+	
Politics	Urbanization	+	++		
	Density			+	
	Change in Density				
	Conservatism	+++ ^(r)	++ ^(r)	+ ^(r)	+ ^(r)
	Republican	++ ^(q)			++
	Legislature				
	Religious				
	Fundamentalism				
	Republican Pres.				
	Clinton Presidency				
	Duration of				
	Republican				
	Presidential Term				
	Republican Governor	++ ^(q)			+
	Marijuana				
	Decriminalization				-
	Southern State				
	Welfare Spending				
	Educ'n Spending				--
	Per Capita GDP				
	State Econ. Growth				
	State Revenue				
	Presidential Election				-
	Year				
	Gubernatorial				++
	Election Year				

Table 2.1 (continued)
Studies Examining Prison Population/Incarceration Rates

		Jacobs & Carmichael	Sorensen & Stemen	Nicholson- Crotty	Smith
Study Year		2001	2002	2004	2004
Variables					
Legal Factors	Structured Sentencing	--	--		--
	Revenue-Focused Guidelines			--	
	Guidelines Without Revenue Focus			++	
	Voluntary Guidelines			+	
	Three Strike Laws				+
	Court Order				
Endogeneity Control		No	No	No	No
Serial Correl. Control		Yes	■	Yes	Yes
Heterosked. Control		Yes	Unk	Yes	Yes
Diff-in-Diff Control		No	■	No	No

Note: The titles of the studies are provided in note 58. "Rate" means the article used the incarceration rate as its dependent variable, "NTS" stands for national-level time series rather than a panel of states, and "specifications" provides the number of models estimated in a given article. A coefficient that is positive and significant in all specifications is indicated by "+++" (and "--" if negative and significant in all); if positive in all specifications and significant in at least half, "++" (and likewise "--" if negative); if positive in all specifications but significant in fewer than half, by "+" (and "--" for negative). If the sign is inconsistent across specifications, the coefficient is labeled "NR" (for "Not Robust"). "Endogeneity Control" indicates whether the article controlled for the endogenous relationship between prison populations and crime, "Serial Correl. Control" whether the article explicitly corrected for serial correlation, "Heterosked. Control" whether it explicitly addressed issues of heteroskedasticity, and "Diff-in-Diff Control" whether it accounted for the serial correlation problems raised by difference-in-difference estimators discussed in Bertrand et al., *infra* note 95. "■" indicates that the issue was immaterial for that paper, and "Unk" means that it is unclear from the article if the regressions control for the particular issue. (p) Four include extensive time interactions. (q) Variable defined as percent of legislature that is Republican times a dummy which equals 1 if governor is also Republican. (r) Actual coefficient in paper is negative since variable measures liberalism of population. (s) Range is 18-24, not 14-24.

Table 2.2
Studies Examining Prison Population/Prison Admission

		Marvell	Jacobs & Helms	Marvell & Moody	Marvell & Moody
Study Year		1995	1996	1996	1997
Dependent Variable		Ln(Rate)	Rate	Ln(Rate)	Ln(Rate)
No. of States in Sample		9	NTS	47	47
Years		74-93	50-90	74-93	74-94
Specifications		1	8 ^(b)	1	1
Variables					
Mechanistic/Crime Theory	Crime	++	++ ^(c)	++	
	Violent Crime				
	Property Crime				
	Drug Arrests				
	All Arrests				
	Probation Rate				
	Admits from Cond'l Release				
	Change in Pop./Release Ratio				
	Police			++	
	Capacity of Prisons				
	Capacity of Alternatives				
	Mental Hosp. Rate				
	Prison Admissions				
	Prison Releases				
Economic Theory	Lagged Dependent Variable				++
	2nd Lagged Dependent Variable				--
	Unemployment		+		
	Employment	+		+	+
	Poverty			-	+
	Personal Income	-		-	-
Dis-order	Non-White Median Income		-		
	Inflation Rate				
	Var'n in Income		+++		
	Gini Coefficient		-		
Dis-order	Out-of-Wedlock Births		+++		
	Divorce Rate				

Table 2.2 (continued)
Studies Examining Prison Population/Prison Admission

		Marvell	Jacobs & Helms	Marvell & Moody	Marvell & Moody
Study Year		1995	1996	1996	1997
Variables					
Demographics	% Pop., 14-24	-(e)		-(e)	+(e)
	% Pop., 18-29				
	% Pop., 25-34	++		++	+
	% Pop., 34-45				-
	% Male, 14-25		-		
	% Black			++	
	% Young Black Male				
	% Latino				
	% Minority				
	Total Population				
	Urbanization			-	
	Density				
	Change in Density				
Politics	Conservatism		+++		
	Republican Legislature				
	Competitiveness of Legislature				
	Contestedness of Seats				
	Religious Fundamentalism				
	Republican Pres.				
	Clinton Presidency				
	Duration of Republican Presidential Term				
	Republican Governor				
	Marijuana Decriminalization				
	Southern State				
	Welfare Spending				
	Educ'n Spending				
	Per Capita GDP				
	State Econ. Growth		-		
	State Revenue				
	Presidential Election Year		+++		
	Gubernatorial Election Year				

Table 2.2 (continued)
Studies Examining Prison Population/Prison Admission

		Marvell	Jacobs & Helms	Marvell & Moody	Marvell & Moody
Study Year		1995	1996	1996	1997
Variables					
Legal Factors	Structured Sentencing	(a)		(a)	
	Revenue-Focused Guidelines				
	Guidelines Without Revenue Focus				
	Voluntary Guidelines				
	Three Strike Laws				
	Court Order				
Endogeneity Control		No	No	No	Yes
Serial Correl. Control		Yes	Yes	Yes	Yes
Heterosked. Control		Yes	Yes	Yes	Yes
Diff-in-Diff Control		No	■	No	■
<p>Note: The titles of the studies are provided in note 58. "Rate" means the article used the incarceration rate as its dependent variable, "NTS" stands for national-level time series rather than a panel of states, and "specifications" provides the number of models estimated in a given article. A coefficient that is positive and significant in all specifications is indicated by "+++" (and "− − −" if negative and significant in all); if positive in all specifications and significant in at least half, "++" (and likewise "− −" if negative); if positive in all specifications but significant in fewer than half, by "+" (and "−" for negative). If the sign is inconsistent across specifications, the coefficient is labeled "NR" (for "Not Robust"). "Endogeneity Control" indicates whether the article controlled for the endogenous relationship between prison populations and crime, "Serial Correl. Control" whether the article explicitly corrected for serial correlation, "Heterosked. Control" whether it explicitly addressed issues of heteroskedasticity, and "Diff-in-Diff Control" whether it accounted for the serial correlation problems raised by difference-in-difference estimators discussed in Bertrand et al., <i>infra</i> note 95. "■" indicates that the issue was immaterial for that paper, and "Unk" means that it is unclear from the article if the regressions control for the particular issue. (a) Effect depends on type of guidelines adopted. (b) Includes each part of stepwise regression method used. (c) Stepwise method focused on crime², not crime.</p>					

Table 2.2 (continued)
Studies Examining Prison Population/Prison Admission

		Sorensen & Stemen	Listokin	Nicholson- Crotty	Stucky et al.
Study Year		2002	2003	2004	2005
Dependent Variable		Rate	Ln(Rate)	Rate	Log-odds ^(t)
No. of States in Sample		50	48	50	49
Years		96	85-97	75-98	78-96
Specifications		1	1	1	2
Variables					
Mechanistic/Crime Theory	Crime	++	+	+	
	Violent Crime				+
	Property Crime				++
	Drug Arrests				
	All Arrests				
	Probation Rate				
	Admits from Conditional Release	++			
	Change in Pop./ Release Ratio				
	Police		-		
	Capacity of Prisons				
	Capacity of Alternatives				
	Mental Hosp. Rate				
	Prison Admissions				
Economic Theory	Prison Releases				++
	Lagged Dependent Variable				
	2nd Lagged Dependent Variable				
	Unemployment		-	+	+
	Employment				
Dis- order	Poverty		-		
	Personal Income		-	+	
	Non-White Median Income				
	Inflation Rate				
	Var'n in Income Gini Coefficient				
Dis- order	Out-of-Wedlock Births				
	Divorce Rate				

Table 2.2 (continued)
Studies Examining Prison Population/Prison Admission

		Sorensen & Stemen	Listokin	Nicholson- Crotty	Stucky et al.
Study Year		2002	2003	2004	2005
Variables					
Demographics	% Pop., 14-24			+	_(g)
	% Pop., 18-29				
	% Pop., 25-34			++	
	% Pop., 34-45				
	% Male, 14-25				
	% Black	++		++	-
	% Young Black Male				
	% Latino				+
	% Minority				
	Total Population			++	++
	Urbanization			-	
	Density				
	Change in Density				
Politics	Conservatism	++ ^(d)		_(d)	
	Republican Legislature				-
	Competitiveness of Legislature				-
	Competitiveness of Seats				-
	Religious Fundamentalism				
	Republican Pres.				
	Clinton Presidency				
	Duration of Republican Presidential Term				
	Republican Governor				++
	Marijuana Decriminalization				
	Southern State				
	Welfare Spending				--
	Educ'n Spending				
	Per Capita GDP				+
	State Econ. Growth				
	State Revenue				
	Presidential Election Year				
	Gubernatorial Election Year				

Table 2.2 (continued)
Studies Examining Prison Population/Prison Admission

		Sorensen & Stemen	Listokin	Nicholson- Crotty	Stucky et al.
Study Year		2002	2003	2004	2005
Variables					
Legal Factors	Structured Sentencing	--			-
	Revenue-Focused Guidelines			--	
	Guidelines Without Revenue Focus			++	
	Voluntary Guidelines			+	
	Three Strike Laws				
	Court Order				
Endogeneity Control		No	Yes	No	No
Serial Correl. Control		■	Yes	Yes	Unk
Heterosked. Control		No	Yes	Yes	Unk
Diff-in-Diff Control		■	Yes	No	■
<p>Note: The titles of the studies are provided in note 58. "Rate" means the article used the incarceration rate as its dependent variable, "NTS" stands for national-level time series rather than a panel of states, and "specifications" provides the number of models estimated in a given article. A coefficient that is positive and significant in all specifications is indicated by "+++" (and "--" if negative and significant in all); if positive in all specifications and significant in at least half, "++" (and likewise "--" if negative); if positive in all specifications but significant in fewer than half, by "+" (and "-" for negative). If the sign is inconsistent across specifications, the coefficient is labeled "NR" (for "Not Robust"). "Endogeneity Control" indicates whether the article controlled for the endogenous relationship between prison populations and crime, "Serial Correl. Control" whether the article explicitly corrected for serial correlation, "Heterosked. Control" whether it explicitly addressed issues of heteroskedasticity, and "Diff-in-Diff Control" whether it accounted for the serial correlation problems raised by difference-in-difference estimators discussed in Bertrand et al., <i>infra</i> note 95. "■" indicates that the issue was immaterial for that paper, and "Unk" means that it is unclear from the article if the regressions control for the particular issue. (d) Actual coefficient in paper is of the opposite sign since variable measures liberalism of population. (e) Age here is 18-24, not 14-24. (f) The dependent variable is the log of the admissions rate divided by one-minus-the-rate. (g) Stucky et al. use the marriage rate, not the divorce rate, and find a positive effect. (h) Contestedness measures the number of legislative seats that face meaningful competition.</p>					

Table 3.1

Study	M&P	L	Arv.	T&W	A&A	M
Year	1990	1991	1992	1993	1995	1995
Theories						
Mechanistic						
Crime	•	•	•	•	•	
Arrests						
Police						
Admit/Release		•		•		
Capacity				•		
Other						
Economic						
Unemployment	•	•	•			•
Income Levels	•		•	•		•
Income Variation					•	
Other		•				
Social Disorder						
Illegit. Births						
Divorce						
Demographics						
Age		•				•
Race			•	•	•	
Population	•			•		
Politics						
Conservatism				•		
Elections						
State Spending		•				
Other			•			
Legal Factors						
Structured Sent'g				•		•
Court Orders						

Note: The articles are listed here in the same order as in Table 2.1. A bullet (•) indicates that the article uses some variable to capture the general theory listed for that row.

[illegible]

Table 3.1 (continued)*Inclusion of General Theories in Incarceration Rate Models*

Study	J&H	J&C	S&S	N-C	S
Year	2001	2001	2002	2004	2004
Theory					
Mechanistic					
Crime	•	•	•	•	•
Arrests					
Police					
Admit/Release					•
Capacity					
Other					•
Economic					
Unemployment	•	•		•	•
Income Levels		•		•	•
Income Variation	•	•			•
Other					
Social Disorder					
Illegitimate Births	•				
Divorce					•
Demographics					
Age				•	
Race	•	•	•	•	•
Population		•	•	•	
Politics					
Conservatism	•	•	•	•	•
Elections	•				•
State Spending	•				•
Other					•
Legal Factors					
Structured Sent'g		•	•	•	•
Court Orders					
Note: The articles are listed here in the same order as in Table 2.1. A bullet (•) indicates that the article uses some variable to capture the general theory listed for that row.					

Table 3.2
Inclusion of General Theories in Admissions Models

Study	M	J&H	M&M	M&M
Year	1995	1996	1996	1997
Theory				
Mechanistic				
Crime	•	•	•	
Arrests				
Police			•	
Admit/Release				
Capacity				
Other				•
Economic				
Unemployment	•	•	•	•
Income Levels	•	•	•	•
Income Variation		•		
Other				
Social Disorder				
Illegitimate Births		•		
Divorce				
Demographics				
Age	•	•	•	•
Race			•	
Population			•	
Politics				
Conservatism		•		
Elections		•		
State Spending		•		
Other				
Legal Factors				
Structured Sent'g	•		•	
Court Orders				

Table 3.2 (continued)
Inclusion of General Theories in Admissions Models

Study	S&S	L	N-C	S et al.
Year	2002	2003	2004	2005
Theory				
Mechanistic				
Crime	•	•	•	•
Arrests				
Police		•		
Admit/Release	•			•
Capacity				
Other				
Economic				
Unemployment		•	•	•
Income Levels		•	•	
Income Variation				
Other				
Social Disorder				
Illegitimate Births				
Divorce				•
Demographics				
Age			•	
Race	•			•
Population			•	•
Politics				
Conservatism	•		•	•
Elections				•
State Spending				•
Other				
Legal Factors				
Structured Sent'g	•		•	•
Court Orders				

The claims summarized in Tables 2.1 and 2.2 must be viewed as preliminary. Admittedly, a fair degree of consensus exists across the studies for several factors. Most articles, for example, find that prison population and admission rates rise with crime rates, and likewise with the fraction of the population that is young and that is black. But many of the studies suffer from methodological errors that make identification difficult. Almost all, for example, fail to properly control for endogenous relationships between several variables. And despite a handful of consistent findings, the effects of most variables differ widely across models, even within particular articles. This instability may be the result of omitted variable bias, or it may arise from deeper structural concerns with the regressions. Furthermore, there are conceptual problems with the choice of variables, such as failing to properly address the complicated intertemporal

dynamics of total prison population. And finally, all the studies essentially rely on the same set of data: rather than testing the models against alternate sources of information, all use the same basic set of observations.

This Part provides a summary of the findings from the studies listed in note 58, along with brief comments on the particular empirical concerns they raise. Rather than looking at each study in isolation, I will look at what the studies as a whole imply about each major theory, as well as how well-identified any effects appear to be. Part III then addresses how, in general, to rectify the concerns discussed here (as well as other shortcomings).

1. Prison and Crime Rates

With the exception only of the articles by Marvell and Moody,⁶⁰ every study in note 58 includes a measure of crime rates, and by and large each finds crime positively correlated with both prison populations and admissions. Because of several conceptual and methodological limitations, however, these results do not necessarily identify the actual magnitudes of the relationships.

The most important conceptual issue implicates the fourteen studies that examine the relationship between crime and total prison population (as opposed to admissions).⁶¹ The total stock of prisoners is the product of complex dynamics. Total prison population rates in 1995 are driven by crimes rates not just in 1995, but in 1994, 1993, and even 1985. More crime in 1985 leads to more prisoners admitted to prison in 1985 (and in 1987, since the criminal justice system moves slowly), and thus to more prisoners still serving time in 1995. The more 1985 suffers from severe crimes with long attendant sentences, the more important this lagged effect will be, since a greater fraction of those admitted in 1985 will still be serving their sentences in 1995. Yet none of the models above includes

⁶⁰ To avoid including too many footnotes, which can distract from the flow of the argument, I will not provide specific cites below for the articles listed *supra* note 58 unless absolutely necessary to avoid confusion. The specific empirical results mentioned below can be found in the following locations: Beckett & Western at 50-55 and Tables 1-3; Greenberg & West at 629-38 and Tables 2-3; Jacobs & Carmichael at 70-75 and Tables 1-3; Jacobs & Helms, *Political Model*, at 333-42 and Tables 2-4; Jacobs & Helms, *Political Sociology*, at 178-88 and Tables 2-3; Lessan at 184-87 and Table 2; Listokin at 189-97 and Tables 1-2 and 5; Marvell at 700-04 and Table 1; Marvell & Moody, *Determinate Sentencing*, at 113-18 and Table 3; Marvell & Moody, *Age-Structure Trends*, at 119-22 and Table 4; Nicholson-Crotty at 402-05 and Table 2; Raphael at 6-10 and Tables 1-2; Sorensen & Stemen at 461-63 and Tables 1-2; Smith at 929-32 and Table 1; and Stucky et al. at 220-25 and Tables 1-2.

⁶¹ These are all the studies listed in Table 2.1, with the exception of those by Marvell and Moody.

more than a single year's crime rate. That lone coefficient does not lend itself to easy interpretation.

The six models focusing on admission rates that consider crime suffer much less from this problem.⁶² Prison *admissions* in 1995 are not significantly influenced by crime rates in 1993, much less 1985; the more sophisticated models address those lags that persist.⁶³ But with one exception, these models still suffer from an important empirical shortcoming: they fail to control for the endogenous relationship between prison admission rates and crime rates. (This problem affects the studies looking at total population rates as well.⁶⁴) While crime rates influence prison admissions, prison admissions likewise affect crime rates. Failure to control for this simultaneity biases the coefficients in the models, causing them to understate any true effect.

Yair Listokin's article—the only one to control for endogeneity effectively—provides a sense of the magnitude of this problem.⁶⁵ Listokin takes advantage of the discovery by John Donohue and Steven Levitt that abortion rates in the 1970s influenced crime rates in the 1990s, allowing him to use abortion as an instrumental variable.⁶⁶ His own results suggest that this correction nearly doubles the magnitude of crime's effect on prison

⁶² Table 2.2 lists these articles. Note that Marvell & Moody, *Age-Structure Trends*, *supra* note 21, omit crime, although their other articles include it in their admissions regressions.

⁶³ Listokin, for example, does not use the crime rate of a single year, but rather a moving average of two years' crime rates, to reflect the fact that crimes do not lead immediately to admissions.

⁶⁴ The exceptions again are those articles by Marvell alone and Marvell & Moody, which omit the crime variable from their regressions explicitly to avoid simultaneity bias. However, as a result they trade simultaneity bias for omitted variable bias. *See infra* Part IV.A.

⁶⁵ Listokin uses abortion as an instrument for crime. Lessan, like Listokin, uses an instrumental variable, but her choice of instrument is unconvincing. She relies on "a measure of change in the ratio of deflated state correctional expenditures per reported violent crime." Lessan, *supra* note 58, at 185. She does not explain how she chose this instrument or why in theory it should be uncorrelated with prison populations; that her analysis suggests violent crime rates are unresponsive to prison populations and changes in prison populations fall as changes in violent crime rates rise caution that her specification may be problematic.

⁶⁶ John J. Donohue III & Steven D. Levitt, *The Impact of Legalized Abortion on Crime*, 116 Q.J. ECON. 379 (2001) [hereinafter Donohue & Levitt, *Legalized Abortion*]. Some have taken exception to the magnitude of the effect found by Donohue and Levitt. *See, e.g.*, Ted Joyce, *Did Legalized Abortion Lower Crime?*, 39 J. HUM. RESOURCES 1 (2004). But even if these critiques are valid—and Donohue and Levitt argue they are not—the instrumental relationship survives. *See* John J. Donohue III & Steven D. Levitt, *Further Evidence that Legalized Abortion Lowered Crime: A Reply to Joyce*, 39 J. HUM. RESOURCES 29 (2004).

admissions.⁶⁷ Compared to the results from Marvell and Moody, the only other articles besides Listokin's reporting elasticities, his correction *quadruples* the effect of crime (from approximately 0.26 to 1.07). Though Nicholson-Crotty and Sorensen and Stemen measure absolute effects, not elasticities, their results—when roughly translated into elasticities—are between one-third (for Nicholson-Crotty) and two-thirds (for Sorensen and Stemen) smaller than Listokin's corrected values.⁶⁸

The findings on crime, then, can be briefly summarized. It appears that crime rates influence admission rates, although all results but Listokin's systematically underestimate the size of the effect; Listokin finds that a 1% increase in crime leads to an approximate 1% increase in admissions, although the effect is not statistically significant. That crime rates positively affect admission rates suggests that they should also positively affect overall incarceration rates, and the positive coefficients returned in almost every model are consistent with this.⁶⁹ But the total-population models as currently designed are incapable of estimating bounds on the magnitude of this effect. Even setting aside the problem of endogeneity, the dynamic relationship between crime and total incarceration is complex and, in all the models above, ignored.⁷⁰

2. Prison and Economics

Examinations of the importance of economic conditions—in particular employment, income levels, and income inequality—produce less consistent results than those looking at crime. Coefficients vary widely (even with respect to sign) not just across articles, but across specifications within articles. Consider the effect of the percent unemployed. In the total-population regressions, estimates range from approximately -11.5 to +6.5;

⁶⁷ In the uncorrected regressions, he finds an elasticity of 0.59 (i.e., a 1% increase in crime leads to a 0.59% increase in prison populations); with the corrections, this elasticity rises to 1.07.

⁶⁸ Relying on Listokin's summary statistics, which report a mean admissions rate of 141 per 100,000 and a mean crime rate of 1880 per 100,000, Sorensen and Stemen's coefficient of 0.024 translates into an elasticity of approximately 0.35 at the means; Nicholson-Crotty's 0.05, an elasticity of 0.67.

⁶⁹ The only negative coefficients are for property crime when violent and property crimes are separated.

⁷⁰ Listokin looks briefly at the issue, regressing crime rates on releases as well as admissions. But this simply tells us how today's crime rates influence today's releases, not how patterns of offending two, three, or ten years ago affect releases today. In other words, as Listokin notes, such a regression measures only any crowd-out effect of today's crime rates on today's prison population. Listokin, *supra* note 58, at 200. Data from the National Corrections Reporting Program, which allows investigators to directly calculate the time served by individual offenders, provides a way to measure changes in release patterns more directly. See Pfaff, Trends in Prison Release, *supra* note 1.

in the admissions regressions, from approximately -0.7 to $+13.27$.⁷¹ The sensitivity of these findings to small changes in model structure can be seen clearly in the article by Beckett and Western: adding year dummies and terms interacting the year dummies with welfare spending and percent-minority cause the coefficient on unemployment to swing from approximately -9 and fairly significant to almost $+3$ and significant.

Marvell, Moody, and Raphael are the only authors to consider employment rather than unemployment. The results here are unclear as well. Marvell and Moody consistently find that employment is (perhaps surprisingly) positively correlated with prison admissions, but often that it is negatively correlated with the total prison population. None of the terms, however, is statistically or numerically significant. Raphael finds slightly stronger negative relationships between employment and total prison population.

A potential problem with these results is that there may be an important, and wholly unacknowledged, source of endogeneity at work. The U.S. government does not count prisoners as unemployed, so every unemployed person sent to prison increases the prison population by one and reduces the unemployment rolls by the same. This endogenous relationship can be significant. Bruce Western recently demonstrated that correcting for this endogeneity eliminates the apparent employment gains of young black men during the 1990s.⁷² In the models here, the simultaneity biases the coefficient downward. If the true relationship is, as theorized, positive—that rising unemployment drives up prison populations and admissions—then the reported coefficients will understate the true effect.

This “statistical” endogeneity, however, is not the only type of endogeneity present. A “causal” form exists as well. Incarceration can influence unemployment rates, even those adjusted to reflect prison populations, in two contradictory ways. First, it can (with a lag) reduce the employment opportunities of those incarcerated; it may even (perhaps without a lag) reduce the employment options for all young black men, regardless of whether they have been incarcerated, as a result of statistical (or direct) discrimination.⁷³ Second, it may increase employment opportunities for those living where prisons are located. Thus, while the

⁷¹ A coefficient of -11.5 implies that a one-percentage-point increase in unemployment leads to an 11.5-point decline in the incarceration rate (per 100,000). The value of -0.7 is derived from Listokin, who uses \ln (admission rate per 1000) as his dependent variable. His coefficient of -0.005 implies a one-percentage-point change in unemployment leads to a 0.5% decline in the admission rate. At the average admission rate of 141 per 100,000, this translates into a 0.7-point decline in the admissions rate.

⁷² BRUCE WESTERN, PUNISHMENT AND INEQUALITY IN AMERICA (2006).

⁷³ See *id.*

direction of the bias from statistical endogeneity is identifiable, that from the causal endogeneity is not. If the effect on those incarcerated is stronger than that on those guarding them, then the bias pushes the coefficient up; if the effect on those incarcerated is weaker, then the bias pushes the coefficient down.

Drawing inferences about the magnitude or range of other relevant economic factors is equally difficult. The results for the effect of poverty, for example, are inconsistent. In a single study of total population (Beckett and Western), the coefficient on poverty ranges from approximately -3 to $+7$; in the admissions articles, the effect is generally close to zero. Part of the problem here is likely endemic to the poverty measure: it exhibits little variation across states, and even less within a particular state over time. Listokin, the only article to provide relevant summary statistics, reports a mean poverty rate of 13.4, with an overall standard deviation of 4.03, but a within-state standard deviation of only 1.8. With little variation and in the presence of state- and year-fixed effects, it is perhaps not surprising that the results are weak and inconsistent.

The remaining economic variables are similarly unstable, at least in the total-population equations. Five models look at personal income, five at inequality measured by the Gini coefficient, and one at both. The effect of personal income ranges from -5.9 to $+7.9$ in Raphael's study alone. Similarly, Marvell finds a rough absolute effect of $+0.0008$,⁷⁴ Nicholson-Crotty of -471 (!); Nicholson-Crotty provides no summary statistics, but I have to believe that he uses a different scale than Raphael or Marvell and Moody, and that a comparative value would be -4.71 .⁷⁵ Results for the Gini coefficient exhibit a similar pattern. It should perhaps be expected that the total-population results are unstable. As noted before, total population is a function of numerous lags that a single period of, say, unemployment cannot capture. Total-population regressions are likely highly model dependent and thus quite sensitive to functional form.⁷⁶

In the admissions equations, however, the results are more stable. Putting aside one study, the effects of personal income are consistently negative, though the magnitude of the effects differs greatly. Listokin, for

⁷⁴ Moody's results are elasticities. Using a mean personal income of \$22,000 (from Listokin) and a mean prison population rate of 209 (from Raphael), this implies that, at the means, a \$220 (1%) change in income leads to a change of 0.1672 (0.08%) in the incarceration rate. This translates into an absolute effect at the means of 0.0008.

⁷⁵ Nicholson-Crotty's results consistently differ from the others by several orders of magnitude, and it is unclear why this is so. In general, insufficient attention is paid in most of the articles to the implications of the reported magnitudes.

⁷⁶ See Daniel Ho et al., *Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference*, 155 POL. ANALYSIS 199 (2007); see also *infra* Part IV.B.

example, returns an elasticity of -1.3 ; both articles by Marvell and Moody, an elasticity of -0.15 . The one exception to the negative results is the article by Nicholson-Crotty, which provides an absolute effect of $+339$, although it is unclear exactly what this means.⁷⁷

We thus know only a little about the importance of the economic theories. The most consistent effect is that of personal income on admissions: it appears to be negative, though the estimates differ widely. The impact of unemployment is less stable, but it suffers from potentially important, and unaddressed, sources of endogeneity. That of poverty is likewise unstable, but then the variable does not vary much.

3. Prison and Demographics

A few regularities appear among the demographic variables. First, consider age. The regressions looking at total prison population, with the exception of those by Raphael, return positive coefficients on the age variables, with stronger effects for the older groups.⁷⁸ In general, the results suggest that a one-percentage-point increase in the share of those between eighteen and twenty-four increases the incarceration rate by approximately 1 or 2 per 100,000; of those between twenty-five and thirty-four, 4 to 8 per 100,000.⁷⁹ Roughly similar patterns obtain in the admissions regressions. For those between the ages of twenty-five and thirty-four, the results indicate that a one-percentage-point increase raises the admissions rate by between 5 and 18 per 100,000.⁸⁰ Some results, however, suggest that the effect is negative for those under twenty-four. In both the total population and admission regressions, only the results for those between twenty-five and thirty-four are consistently statistically significant, and even then not always.

⁷⁷ Nicholson-Crotty's results seem to indicate that a one dollar increase in average personal income raises the admissions rate by 339 per 100,000. Given that, according to Listokin, the mean admissions rate is 141 per 100,000 and the standard deviation on personal income is \$3652, this cannot be the case. Even a coefficient of 3.39 seems too large.

⁷⁸ Note that both Marvell and Moody articles omit crime from their regressions looking at total population (to avoid problems with simultaneity). Given the correlations between age and crime, it is likely that the age coefficient is picking up some of the crime effect; the nature of the bias will vary with age group.

⁷⁹ Again, this is converting the Marvell and Moody elasticities into absolute effects at the means.

⁸⁰ The causal mechanism here is unclear. The age of a defendant may directly influence a judge's willingness to incarcerate him or to do so for longer. But it is also possible that on average an older defendant has a longer record and thus automatically faces a greater risk of prison or a greater risk of a longer sentence.

The results also suggest that prison populations and prison admissions grow with the percent of the population that is black.⁸¹ In the total population regressions, the results imply that a one-percentage-point increase in the percent black raises the incarceration rate by around 2.5 to 6 per 100,000.⁸² Beckett and Western find an effect of +393, but this likely means a change of 3.93 per 100,000.⁸³ For admissions, the results lie between 0.6 and 4.2 per 100,000.⁸⁴ The coefficients in both types of regressions tend to be statistically significant.

The last demographic term that appears in multiple studies is a control for urbanization. These results are often neither significant nor robust. In Beckett and Western, for example, one set of regressions indicates that the effect of urbanization shifts from a significant -2.78 to an almost-significant +0.83 just by changing the assumption about the error distribution (from a normal to a t-distribution). Greenberg and West find consistently negative but highly insignificant results in the levels regressions, but in the first-difference regressions the sign flips from negative to positive (insignificant in both cases) with the addition of a single, relatively unrelated term (an interaction between narcotics arrests and a year dummy).

The current studies, then, appear to suggest two things about the effect of demographics on prison populations: admission and population rates rise with the percent of the population that is between twenty-four and thirty-five and with the percent of the population that is black. The magnitude of the effects on total population appears to be similar, on the order of about 5 per 100,000. For admissions, the effect of age is slightly larger, of race slightly smaller. Unfortunately, there is little discussion of how much these variables vary within a state over time, so it is unclear how much these factors explains trends within, as opposed to across, states.

⁸¹ The only study to get a negative effect was Raphael. It is unclear why this is the case.

⁸² Stucky et al. also find a positive coefficient for race. Their choice of dependent variable, however, makes it difficult to compare their results—for race or any other explanatory variable—to those of the other articles discussed here.

⁸³ The incarceration rate appears to change by 393 if the percent black, which must fall between zero and one, increases by one, not one percentage point. In other words, when the percent black changes from its mean value of 0.118 to 1.118, the incarceration rate rises by 393 per 100,000 (ignoring extrapolation problems). This is, of course, impossible.

⁸⁴ The value of 4.2 is derived from the elasticity of 0.36 in Marvell & Moody, *Determinate Sentencing*, *supra* note 58. Nicholson-Crotty reports a coefficient of 64, but as stated *supra* note 75, I believe this translates to 0.64 per 100,000.

4. Prison and Politics

The claim that politics has driven prison growth implicates at least two separate theories: (1) that a more conservative polity inherently demands more punishment, and (2) that politicians cynically use crime (or the fear of crime) as a “wedge” issue to secure votes. The two theories are not wholly independent—crime works as a wedge issue only if people care about it—but they do lead to significantly different implications. The first, for example, suggests that more conservative states should adopt more severe punishments; the second suggests that legislatures with unstable majorities should pass harsher laws regardless of political orientation. By and large the studies considered here have focused only on the first possibility, and even then it is unclear how well they have identified its effect.

The only factor studied with any regularity is the conservatism of the electorate. Four of the six studies examining the importance of citizen ideology use the scale developed by William Berry and his co-authors;⁸⁵ two others use less reliable measures.⁸⁶ The effects are somewhat robust. For total prison populations, all four studies find that more liberal states incarcerate less, although the effect is not always statistically significant. In the admissions regressions, Sorensen and Stemen again find positive effects for conservatism, but Nicholson-Crotty now finds a negative effect which, while statistically insignificant, is twice the absolute size of Sorensen and Stemen’s.

The terms are difficult to interpret because the scale does not have any inherent meaning. The Berry index scores states from 0 to 100, with 100 being the most liberal and 0 the most conservative.⁸⁷ The sample-wide standard deviation is approximately 16, though intrastate standard deviations are generally smaller, often less than 10. A coefficient of 0.15,

⁸⁵ William D. Berry et al., *Measuring Citizen and Government Ideology in the American States, 1960-93*, 42 AM. J. POL. SCI. 327 (1998). The data are available at ICPSR (<http://www.icpsr.umich.edu>) and have been updated through 2004. The four studies are those by Jacobs and Carmichael, Nicholson-Crotty, Smith, and Sorensen and Stemen.

⁸⁶ Greenberg and West use a scale that is available for a single year, imposing the assumption that political attitudes in the states changed little between 1970 and 1990; this is directly contradicted by the end-of-modernity hypothesis. Jacobs and Helms, *Political Model*—which relies on national time series data instead of panel data—uses an index of the percent of governors and members of (only) Congress who are Republican, along with Gallup Poll responses, to measure conservatism. Unfortunately, they multiply this index by an indicator for whether the president is Republican, effectively ignoring political shifts during Democratic presidential regimes. Furthermore, their focus on national political actors seems inappropriate for an issue that is predominantly a state matter.

⁸⁷ The results in Tables 2.1 and 2.2 provide the sign of (1 – index), so that the sign reflects the effect of increasing conservativeness, since that is how the political theory issue is regularly posed.

the results in Nicholson-Crotty, would imply that a one-standard-deviation increase in conservatism increases a state's incarceration rate by approximately 0.75 per 100,000 (if $sd = 5$) to 2.25 per 100,000 (if $sd = 15$).

No other political variable receives much sustained attention.⁸⁸ Three studies of total population, however, consider the importance of the Republican-ness of the state legislature. The models generally indicate that states with more-Republican legislatures incarcerate more, but the results are often small, both numerically and statistically. And two total-population models attempt to measure the effect of electoral pressures by looking at presidential election years; one (Jacobs and Helms, *Political Sociology*) finds small positive effects, the other (Smith) small negative effects. The latter model also includes an indicator for gubernatorial elections, which is positive, suggesting perhaps that what matters more are state electoral pressures.

Only one article, that by Stucky et al., tests the importance of the electoral competition theory alongside that of the conservatism theory. It considers two measures of electoral pressures: a simple one ("contestation") based on the percent of legislative races that involved at least two candidates; and a more complex one ("competition") that employs an index involving the average margin of victory, the number of safe seats (a margin of victory of at least twenty percentage points), and the number of contested seats. In general, their results indicate that prison admissions grow with the Republican-ness of the legislature, that this effect has grown stronger over time, and that it grows stronger still the more competitive the state's legislative (but apparently not gubernatorial) politics; these results obtain for both the "contestation" and "competition" models.

Clearly, we have only begun to explore the importance of political effects. The focus on state ideology and legislative composition is a promising start, but it suffers from several shortcomings. The first is that, with one exception, little effort has been made to separate the conservative hypothesis from the electioneering hypothesis. The second is closely tied to the first. Only presidents, governors, and state legislators have been noted so far, despite the fact that district attorneys, state attorneys general, state judges, and mayors (all of whom—or, for judges, many of whom—are elected like legislators and governors) may influence prison growth at least as much as, if not more than, the three sets of included actors. Mayors may hire more police in difficult elections,⁸⁹ district attorneys may prosecute

⁸⁸ For the admissions models, no other political variable *at all* is considered, except for the effect of presidential election years in one study.

⁸⁹ Steven Levitt has found evidence that urban police forces rise during election years regardless of the crime rate, and that gubernatorial and mayoral elections have similar effects: police forces rise by about 2% in both mayoral and gubernatorial election years.

more aggressively in election years, and judges up for re-election may be less sympathetic to criminal defendants.⁹⁰ Furthermore, each of these actors faces a different set of incentives and may be loyal to different segments of the electorate.⁹¹

One reason for the focus on presidents, governors, and legislators is surely a matter of data. My own efforts have revealed that while data on these elections are easily available, those for the other actors mentioned above are significantly harder to find, especially in a useable form. The unavailability of such data, however, indicates that more attention must be given to working around, and to exploring the epistemic implications of, their absence.

Finally, it should be noted that none of the models addresses Tonry's theory of cycles in sensibilities. This, however, may not be problematic. The sensibilities theory focuses on explaining either how crime trends encourage people to become more receptive to conservative values (if conservatism's relative toughness towards crime is essentially constant), or how these trends encourage conservatives to adopt increasingly hard-line views towards crime (if their toughness is flexible but generally harsher than liberals'). It thus tries to explain why (and when) we should expect a positive coefficient on measures of political and social conservatism, but it does not necessarily suggest different specifications for the empirical models themselves. In other words, that the models do not explicitly account for Tonry's claim does not mean, if he is correct, that the models suffer from an omitted explanatory variable.

Ultimately, then, the current studies have just begun to examine the importance of the political theories. They provide some indication that more conservative states tend to incarcerate more people, and that increases in conservatism are correlated with increases in prison populations. But they leave unanswered many important questions that future work must

Steven D. Levitt, *Using Electoral Cycles in Police Hiring to Estimate the Effect of Police on Crime*, 87 AM. ECON. REV. 270, 275 (1997).

⁹⁰ Jason Czarnezki has demonstrated that Wisconsin Supreme Court justices often adopt noticeably harsher attitudes toward criminal defendants in the year each is up for re-election. Jason J. Czarnezki, *Voting and Electoral Politics in the Wisconsin Supreme Court*, 87 MARQUETTE L. REV. 323 (2003).

⁹¹ Consider New York State. At least in the 1980s, New York City accounted for 72% of the state's prisoners. Jeffrey A. Fagan, Valerie West & Jan Holland, *Reciprocal Effects of Crime and Incarceration in New York City Neighborhoods*, 30 FORDHAM URB. L.J. 1551, 1568 (2003). But less than half of New York State's legislators came from New York City. See *Who Are Your Elected Officials?*, <http://nymap.elections.state.ny.us/nysboe/> (last visited July 1, 2008). Thus, the groups passing the state's criminal laws were elected by a (state) population that differed systematically from the (city) population that elected those most responsible for enforcing the laws.

address.⁹² For example, they do not explain *how* increased conservatism induces higher prison rates: it may lead to a greater focus on enforcement, longer sentences in the code, harsher prosecutors and thus less favorable pleas, or increasingly conservative judges, among other possibilities. Nor in general do the studies test the importance of the electioneering theory. Moreover, by focusing on the legislature, the governor, or the President, it is unclear if these models are examining changing patterns in the institutions most central to the administration of criminal justice.

5. Prisons and Four (or Five) Other Theories

Four other theories have also been advanced to explain prison growth: mental hospital deinstitutionalization, changes in prison capacity, court orders reducing (or fixing) effective prison capacity, and overall state fiscal health. Unfortunately again, the studies in Tables 2.1 and 2.2 do not address any of these theories in any consistent manner. For example, despite widespread concern about deinstitutionalization's role in prison population growth, only Raphael considers it, finding evidence that it is responsible for approximately 4.5% to 14% of the total prison population in 1996. Similarly, Taggart and Winn's is the only study to include a direct measure of prison capacity,⁹³ and Greenberg and West's is the only one to include a variable for court orders restricting prison populations. Neither finds significant results.

State economic health has received slightly more attention, with four studies looking at some measure of state revenue, state economic growth, or per capita gross state product ("GSP") as a proxy for the state's tax base. The results, though, are weak. Greenberg and West find small but significant positive effects in their regression in levels, but almost no effect at all in their regression in first-differences. Beckett and Western produce numerically small, insignificant, and unstable coefficients on their measure of fiscal health. Both articles by Jacobs and Helms yield numerically and statistically insignificant results. That these models fail to identify any effect, however, does not conclusively indicate its absence. Per capita

⁹² If, as preliminary efforts at gathering data seem to suggest, not all these theories are testable, then investigators need to think more carefully about precisely what their models are in fact describing. A particular coefficient, for example, may be conflating more than one theory. See Pfaff, *Prison Population Growth*, *supra* note 1.

⁹³ Capacity, of course, introduces serious risks of endogeneity as well. Increasing prison populations may encourage states to expand capacity, but expansions in capacity may conversely induce greater use of prison. Moreover, accurate measures of capacity are difficult to come by.

income and per capita state GSP, for example, are poor proxies for tax receipts.⁹⁴

Finally, it should be noted that several studies have also examined the effect of structured sentencing laws on prison growth. In general, these results return numerically and statistically significant negative results, although in all cases statistical significance is likely overstated.⁹⁵ Interpreting these results, however, is difficult, since not all guidelines are the same: some, for example, focus on constraining correctional expenditures, others on constraining leniency.⁹⁶ Nicholson-Crotty separates those guidelines concerned with expenditures from those not, and he finds a negative effect for the former (on the order of 6 per 100,000 fewer total prisoners and 30 per 100,000 fewer admissions) and a positive effect for the latter (of around 32 per 100,000 for total populations and 11 per 100,000 for admissions). Marvell and Moody, who separate out individual states in their articles, likewise find that guideline effects vary significantly across states.

B. A BRIEF ASIDE: SMALLER SCALE STUDIES

Those familiar with the literature on prison population growth may feel that the list of studies in note 58 is incomplete. Much has been written, for example, on the relationship between unemployment and incarceration.⁹⁷ The focus here, however, is on those articles that evaluate several theories at the same time rather than focusing on the importance of a single outcome.⁹⁸ As a result, I exclude those studies that use only a very

⁹⁴ William D. Berry & Richard Fording, *Measuring State Tax Capacity and Effort*, 78 SOC. SCI. Q. 158 (1997).

⁹⁵ None of the studies appears to control for an important source of serial correlation that afflicts indicator variables for legal changes. See Marianne Bertrand, Esther Duflo & Sendhil Mullainathan, *How Much Should We Trust Difference-in-Difference Estimators?*, 119 Q.J. ECON. 249 (2004).

⁹⁶ See Marvell, *supra* note 58; Marvell & Moody, *Determinate Sentencing*, *supra* note 58; Nicholson-Crotty, *supra* note 58; see also John F. Pfaff, *The Continued Vitality of Structured Sentencing Following Blakely: The Effectiveness of Voluntary Guidelines*, 54 UCLA L. REV. 235 (2006).

⁹⁷ For a survey of these studies, see Theodore G. Chiricos & Miriam A. Delone, *Labor Surplus and Punishment: A Review and Assessment of Theory and Evidence*, 39 SOC. PROBS. 421 (1992).

⁹⁸ Under this definition, neither Moody nor Marvell themselves may have included their articles here. In each of their articles, they note that they are focusing on a single question (the importance of age, the importance of structured sentencing, the importance of parole abolition) and include variables reflecting the other theories simply as controls to minimize potential bias in their particular coefficients of interest. Nonetheless, their studies may provide some insight, at least to the direction and significance—if not the precise magnitudes—of multiple (secondary) effects.

small number of controls. If nothing else, the failure to control for wide ranges of explanatory factors increases the risk of omitted variable bias, casting doubt on the ability of such narrowly designed articles to accurately identify even their particular parameters of interest.

The focus here is also on macro-level studies. Many articles examine sentencing and incarceration trends at the individual level, such as those that look at how the race or sex of an individual defendant influences a judge's punishment decision.⁹⁹ These studies do inform the questions considered here—if individual-level studies suggest that black defendants receive longer sentences (all else constant), then they can yield some insight into the relationship between the size of a state's black population and its prison population—but the connection is indirect. That said, as future work at the macro level yields more accurate estimates, the individual-level studies will provide a valuable source for checking robustness.

C. A BRIEF ASIDE: DECOMPOSITIONAL STUDIES

A common theme across all studies considered here is that they focus solely on the final outcome, either the incarceration or the admission rate. If properly estimated, then, the coefficients explain how, say, a one-percentage-point change in the black population changes the overall incarceration rate. But the criminal justice system is highly decentralized. That one knows the effect of an increase in the black population on the incarceration rate does not imply knowledge of how that effect comes about. Are blacks more likely to commit crimes, more likely to be arrested by the police, more likely to be prosecuted or offered stiffer plea bargains by prosecutors, more likely to be given prison sentences—or longer sentences—by judges, or more likely to be denied parole by a parole board? The relevant actors, and the incentives they face, differ at every level of the criminal justice system, and a complete understanding of prison growth requires a disaggregated study.

As Greenberg and West note, as of 1999 no such multilevel study had been conducted, and that does not appear to have changed in the intervening years.¹⁰⁰ But there have been some studies that have sought to examine the

⁹⁹ Two comprehensive reviews of this literature are Cassia C. Spohn, *Thirty Years of Sentencing Reform: The Quest for a Racially Neutral Sentencing Process*, and Marjorie S. Zatz, *The Convergence of Race, Ethnicity, Gender, and Class on Court Decisionmaking: Looking Towards the 21st Century*, both in NATIONAL INSTITUTE OF JUSTICE, 3 POLICIES, PROCESSES, AND DECISIONS OF THE CRIMINAL JUSTICE SYSTEM (2000). Sampson & Lauritsen, *supra* note 22, also provide a good summary of these studies.

¹⁰⁰ See Greenberg & West, *supra* note 58, at 641. This is likely due in part to limitations in the data. Attempts to find useful data on prosecutorial behavior have generally turned up little. The only national-level dataset on prosecutorial decisions is the National Prosecutor

importance of each level of the process to the overall growth rate. In other words, these “decompositional” studies, such as those by Blumstein and Beck and by Scott Boggess and John Bound,¹⁰¹ do not try to measure the importance of race or politics or unemployment to changing patterns in incarceration, but they look to explain how changing patterns in crimes, arrests per crime, commitments per arrest, and time served (measured roughly) explain changing patterns in overall incarceration rates.¹⁰² Thus while informative, these studies provide little information about the possible *causes* of the examined trends.

IV. THE PATH FORWARD

The twenty articles considered here have shed light on the forces influencing prison growth over the past three decades. But as Part III establishes, methodological and conceptual shortcomings limit the findings. It is essential, therefore, to develop more accurate and effective approaches.

This Part discusses steps for improving the models we use. In particular, I focus on four issues: simultaneity bias, other sources of bias and model dependency, conceptual problems with the chosen variables, and the need to consider different sources of data. The goal here is not to address the existence of the problems—that endogeneity can significantly bias coefficients is well known—but rather to examine how they can be confronted in this particular context, what information is lost in the process, and what bounds on knowledge we confront.

A. ENDOGENEITY BIAS AND IDENTIFICATION

A common empirical problem in this literature is the failure to account for endogenous relationships between prison population and several explanatory variables. It should not be surprising that an institution such as the U.S. penal system interacts with social, economic, and political forces in complex ways. But almost none of the twenty studies addresses these feedback effects. Only four authors even acknowledge that crime and

Survey (“NPS”), but it is of little use here. First, it is available only biannually from 1990 to 1996, and then again in 2001. Second, the public release masks the identity of the reporting jurisdiction. And third, the NPS consists only of a random sample of prosecutors’ offices, and the sample is drawn at the national, not state, level. Thus, a key stage of the process currently appears to defy examination.

¹⁰¹ See Blumstein & Beck, *supra* note 9; Scott Boggess & John Bound, *Did Criminal Activity Increase During the 1980s?: Comparisons Across Data Sources*, 78 SOC. SCI. Q. 725 (1997).

¹⁰² Noticeably absent is prosecutions per arrest, convictions per prosecution, and commitments per conviction. Again, this is surely due to the lack of prosecutorial data.

prison populations are endogenous,¹⁰³ and with one exception none considers any other form of endogeneity.¹⁰⁴

At least with respect to crime, an effective device for breaking endogeneity has already been identified. As Listokin has demonstrated, abortion is a successful instrument for crime. While abortion rates are clearly correlated with crime rates, they are unlikely to shift prison populations except through their effects on crime. Listokin finds that instrumenting in this way doubles the effect of crime rates on prison admissions (from an elasticity of approximately 0.5 to approximately 1.0).

This instrument, however, is not without its costs, even beyond those normally associated with instrumentation.¹⁰⁵ In particular, it cannot be used before the early 1990s, since legalized abortion does not appear to influence crime rates prior to then.¹⁰⁶ As a result, regressions looking at data from the 1970s and 1980s cannot use abortion to overcome endogeneity bias. And since extrapolation may be risky, it is possible that empirical models will simply be unable to identify the effects of crime prior to the 1990s. Franklin Zimring, for example, has hypothesized that the forces driving prison growth have varied across three distinct periods: 1973-1984 (general crime), 1985-1992 (drugs), and 1993-present (politics).¹⁰⁷ If correct, this theory cautions against extrapolation. In this case, it is important to acknowledge the limits of the empirical model. At least until the discovery of an alternative identification technique, the effects of crime on prison populations may be identifiable only from the 1990s onward.

The abortion instrument can impose other, less obvious costs as well—for example, it may prevent researchers from using data below the state level (the level at which the abortion data are generally available). Some investigators, such as Richard Frase, have started to look at county-level data to examine the intrastate politics of prison growth; even putting aside

¹⁰³ Listokin explicitly controls for the endogenous relationship; Lessan does so as well, but unpersuasively. Both articles by Marvell and Moody control for it (improperly, I feel) in regressions involving prison populations but not in those involving prison admissions. All other studies in Tables 2.1 and 2.2 fail to address the issue altogether. Charles Cappell and Gresham Sykes also note the presence of endogeneity but try to control for it using (unconvincing) exclusion restrictions. Charles Cappell & Gresham Sykes, *Prison Commitments, Crime, & Unemployment: A Theoretical and Empirical Specification for the United States, 1933-1985*, 7 J. QUANTITATIVE CRIMINOLOGY 155 (1991).

¹⁰⁴ Only Marvell & Moody, *Determinate Sentencing*, *supra* note 58, acknowledges that the adoption of sentencing laws may be endogenous, even though six other studies include a measure of structured sentencing.

¹⁰⁵ Instrumentation, for example, often increases the standard errors, reducing the power of the test. This is more true the weaker the instrument.

¹⁰⁶ Donohue & Levitt, *Legalized Abortion*, *supra* note 66.

¹⁰⁷ Franklin Zimring, *Imprisonment Rates and the New Politics of Criminal Punishment*, 3 PUNISHMENT & SOC'Y 161 (2001).

the more fundamental concerns about the reliability of county-level crime data,¹⁰⁸ the lack of abortion data may hinder identification at the county level.

Note, however, that the lack of a viable instrument for crime prior to the 1990s need not imply that *no* effects during the preceding decades can be identified, only that certain ones (here, crime) cannot be. Listokin's results, for example, indicate that the measured effects of unemployment, personal income, and poverty are robust to crime's endogeneity, with the coefficients changing only slightly when he controls for endogenous crime rates.¹⁰⁹ Caution is still needed, of course—*ln*(police per capita), for example, does change with the correction, from -0.039 to -0.138 , with the difference statistically significant—but at least some forces will likely remain amenable to longer-run analysis. Fleshing out these implications more fully, along with their effect on our ability to rigorously test the forces driving prison populations, is a goal of future work.

While a clear—if limited—solution exists for the problem of endogenous crime rates, the same cannot necessarily be said for endogenous unemployment rates. At the very least, one can follow Bruce Western's lead and adjust unemployment and employment ratios to reflect the undercounting of the incarcerated.¹¹⁰ This breaks the statistical endogeneity, but it still leaves in place the causal endogeneity. Like endogenous crime rates, endogenous unemployment rates call out for an instrument: some shock to unemployment that is exogenous with respect to prison populations. A possible option, for example, may lie in federal military spending in the states—fewer contracts may exogenously increase unemployment rates—but the reliability of this instrument is unclear and needs to be investigated more carefully.¹¹¹ In the absence of a viable instrument, however, it may be that it is simply impossible to reliably

¹⁰⁸ Michael D. Maltz and Joseph Targonski provide a good explanation of the limitations of county-level Uniform Crime Reports data. See Michael D. Maltz & Joseph Targonski, *A Note on the Use of County-Level UCR Data*, 18 J. QUANTITATIVE CRIMINOLOGY 297 (2002); see also MICHAEL D. MALTZ, *ANALYSIS OF MISSINGNESS IN UCR CRIME DATA* (2006), <http://www.ncjrs.gov/pdffiles1/nij/grants/215343.pdf>.

¹⁰⁹ Controlling for simultaneity causes the coefficient on unemployment to shift from -0.0052 to -0.0053 , on *ln*(income per capita) from -1.060 to -1.32 (with the difference statistically insignificant), and on the poverty rate from -0.013 to -0.010 .

¹¹⁰ See WESTERN, *supra* note 72; see also Bruce Western & Becky Pettit, *Incarceration and Racial Inequality in Men's Employment*, 54 INDUS. & LAB. REL. REV. 3 (2000).

¹¹¹ This instrument is proposed in Steven Raphael & Rudolf Winter-Ebmer, *Identifying the Effect of Unemployment on Crime*, 44 J.L. & ECON. 259 (2001). Steven D. Levitt, *Alternative Strategies for Identifying the Link Between Unemployment and Crime*, 17 J. QUANTITATIVE CRIMINOLOGY 377 (2001), however, raises some concerns with it.

identify the effect of unemployment on prison populations, given the ambiguous direction of the bias.¹¹²

While crime and unemployment are two of the most obviously endogenous variables, they are not the only ones. The adoption of sentencing guidelines, for example, may be correlated with the incarceration rate; this can be treated, at least in part, by using state-year trends.¹¹³ Prison capacity may be endogenous as well: more prisoners lead to more cells, but more cells may lead to more prisoners. Measures of social disorder such as illegitimate births or divorce rates may likewise be endogenous, and even factors such as state economic growth. In many of these cases, the direction of the endogeneity is identifiable: capacity is biased upwards, economic growth likely downwards. But the magnitude of the bias cannot be determined, implying that, absent some sort of exogenous shift, only bounds on the true variable will be estimable. As discussed in Part IV.B below, this suggests that greater focus should be placed on isolating particular causal mechanisms and seeking out relevant quasi-natural experiments.

B. MODEL SPECIFICATION AND INSTABILITY

As noted above, for many variables the estimated effects are highly unstable, both within and across articles. In some cases, such instability may arise simply from weak or insignificant underlying effects; in these cases, the instability is not problematic, but indicative. In others, however, it may reflect shortcomings in model design. Some of these can be corrected, but others may result from inherent limitations in the models considered here.

Instability manifests itself in two ways. First, and more problematic, the sign of the effect may be unclear. As is clear in Tables 2.1 and 2.2, the coefficients on unemployment, for example, range widely from positive to negative, making it difficult to understand the very nature of any effect. Second, and less problematic, is instability with respect to magnitude. The effect of crime (or at least violent crime) is consistently positive, but the magnitudes vary significantly. Such instability allows us to know the

¹¹² Like poverty, unemployment exhibits relatively low levels of variation, especially intrastate. As a result, the instability of the coefficients may reflect a weak direct effect (especially in the presence of fixed state and year effects), not problems of bias. But it is impossible to test that claim with the current data.

¹¹³ See Pfaff, *supra* note 96. Marvell & Moody, *Determinate Sentencing*, *supra* note 58, include these types of trends. Leona Friedberg, *Did Unilateral Divorce Raise Divorce Rates? Evidence from Panel Data*, 88 AM. ECON. REV. 608 (1998), discusses this approach in depth.

direction of the effect but nothing more. How can we derive more precise measurements?

The easiest-to-correct source of instability is apparent in Tables 2.1 and 2.2: the wide range in variables used across articles. Models use different measures of crime, economic conditions, race, age breakdowns, conservatism, and political pressures. Some of these differences are grounded in theoretical debates, but many, if explained at all, are justified as omissions needed to correct for several empirical concerns, particularly collinearity, measurement error, and insufficient degrees of freedom.¹¹⁴ Unfortunately, none of these methodological justifications is convincing. For collinearity, the solution is not to omit variables but rather to test joint significance. If variables are collinear, we simply *cannot* isolate individual effects; this is an epistemic, not a methodological, problem.¹¹⁵ When the concern is with errors in the variables, dropping variables replaces attenuation bias (whose sign is known) with omitted variable bias (whose sign is not).¹¹⁶ And insufficient degrees of freedom indicate the need for more observations, not fewer variables; it should come as no surprise that this issue arises only in time-series or cross-sectional studies. Future models should exclude variables only with great care, relying on joint tests and expanded sources of data; and they should omit observations only when tests, such as the Griliches-Hausman test, indicate that it is efficient to do so.

The lack of stability across models, however, may reflect a deeper structural problem. As empiricists increasingly acknowledge, the linear regression model can yield results that are sensitive to functional form (a

¹¹⁴ For example, articles that drop terms to avoid collinearity include Greenberg and West; Marvell and Moody, *Age-Structure Trends*; and Beckett and Western. The articles by Jacobs and Helms, and Carmichael omit drug offenses as a variable due to concerns about measurement error. And both Jacobs and Helms, *Political Model*, and Sorensen and Stemen rely on stepwise regressions due to a lack of degrees of freedom. (Jacobs and Helms rely on national time series data, Sorensen and Stemen on a single cross-section of states.)

¹¹⁵ Omission is often justified in the name of precision. But this precision is, of course, ephemeral, since the purported precision is purchased by biasing the coefficient on the retained variable.

¹¹⁶ Some articles omit *observations* to avoid measurement error (in cases like Greenberg and West, Beckett and Western, and Jacobs and Carmichael, as many as 90% of the observations are dropped when they limit themselves to one year per decade). This does not introduce bias, but it does reduce efficiency. This is, in theory, an acceptable tradeoff. However, tests exist to determine whether the gains in reduced attenuation compensate for the loss of precision, but none of the articles discussed here employs them. Zvi Griliches and Jerry Hausman establish the basic method. Zvi Griliches & Jerry Hausman, *Errors in Variables in Panel Data*, 33 J. ECONOMETRICS 93 (1986). Steven D. Levitt provides an intuitive explanation in *Why Do Increased Arrest Rates Appear to Reduce Crime: Deterrence, Incapacitation, or Measurement Error?*, 36 ECON. INQUIRY 353 (1998).

concern that applies to all empirical models, not just those relating to prison growth).¹¹⁷ The prison system's complex interactions with the posited causal factors suggest that this could be a particular problem—and the range of results returned across the twenty studies is consistent with this fear.

At least two adjustments in future work will help minimize instability and functional-form dependence. First, it is important for empirical models to focus on one causal factor at a time, rather than trying to test several causal theories with a single regression.¹¹⁸ Single-cause models are more amenable to the nonparametric matching protocols suggested by Daniel Ho and others, the use of which leads to substantially more stable results.¹¹⁹ These matching protocols confront an important problem: in their absence, the “treated” and “control” groups can differ systematically, which leads to instability. One would not trust a medical experiment for an anti-obesity drug in which the treated group consisted of overweight fifty-year-old men and the control group underweight twenty-year-old women. A test that effectively uses New Hampshire as a control for Texas is equally suspect; preliminary results imply that this problem exists in empirical studies of prison growth.¹²⁰

The matching algorithms confront this problem by culling the “treated” and “untreated” groups—for example, those states that adopt guidelines and those that do not—so that the two sets are as similar as possible to each other across all covariates (except that of the treatment, i.e., presence of guidelines). As Ho et al. demonstrate, matched results are significantly more stable and reliable, thus indicating the importance of attacking one causal theory at a time while still including all covariates. Each regression for each causal factor will include all the same set of covariates, but the “treated” and “control” groups—and thus the matching results—will differ for each.

¹¹⁷ See, e.g., Ho et al., *supra* note 76; Alberto Abadie et al., *Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program* (NBER Technical Working Paper No. 335), available at <http://www.nber.org/papers/t0335>. These articles deal with difference-in-difference regressions, but there is no reason to assume that the instability is any less prevalent in other types of regressions.

¹¹⁸ The models should still include all relevant covariates, but causal inferences should be drawn only one at a time. As this Part demonstrates, the distinction is not semantic.

¹¹⁹ See Ho et al., *supra* note 76; Abadie et al., *supra* note 117. For those familiar with the technical issues involved in matching, it is important to note that the methods discussed in these articles do *not* involve propensity scores. In fact, they have been explicitly designed to deal with the critical shortcomings of such.

¹²⁰ In Pfaff, *Sentencing Guidelines*, *supra* note 1, I find that states adopting guidelines differ systematically from those that do not for many, though not all, relevant covariates (including population, violent crime rates, personal income, percent of population that is young men, and citizen ideology).

In some cases, the treatment variable is inherently discrete, so defining “treated” and “untreated” is simple: states either have guidelines or do not, and a year is either an election year or is not. But many other variables do not lend themselves to such easy categorization, such as the percent of the population that is black, the per-capita income, or the number of drug arrests. In some cases, balancing methods can be adapted to continuous variables,¹²¹ or the variable can be artificially “dichotomized” (into “high” or “low” numbers of drug arrests, say). In other cases, exogenous shocks to continuous variables—a discrete break in the time series—may provide effective quasi-natural treatments. Changes in federal military contracts, for example, may act as exogenous shocks to unemployment.¹²² These shocks shed light on how (roughly) exogenous changes to these variables influence prison growth, and they lend themselves more easily to definitions of “treated” and “control.”

Note, too, a collateral benefit of equal importance that arises from using these shocks: they mitigate the problem of endogeneity. As long as the shock is uncorrelated with prison population, it breaks the simultaneous relationship between prison populations and the affected causal factor.

These quasi-experiments, however, do not come without costs. First, they can be hard to find. A few options suggest themselves—military expenditures as a shock to unemployment, national political issues as shocks to state-level elections¹²³—but for many important causal factors no viable instrument has yet appeared.¹²⁴ Plus, the effect may be identified for only a short duration. The unemployment rate in year t will change exogenously with respect to prison populations if a military contract is lost in that year, but by the next year some endogeneity will already begin to return. Second, quasi-experimental approaches return fewer results than structural models. They may provide better estimates of the mean effect, but they provide less information on other relevant results, such as the variability of impact.¹²⁵ They also may have difficulty measuring interaction effects (although most models listed in Tables 2.1 and 2.2 do not account for any such effects either).

¹²¹ See, e.g., Kosuke Imai & David A. van Dyk, *Causal Inference with General Treatment Regimes: Generalizing the Propensity Score*, 99 J. AM. STAT. ASS'N 854 (2004).

¹²² See Raphael & Winter-Ebmer, *supra* note 111.

¹²³ For example, several state legislatures changed from Republican to Democratic control in the 2006 election, perhaps in part due to anti-Republican sentiments arising from dislike of federal, less than state, policies.

¹²⁴ Identifying more such shocks is, of course, an important issue for future work.

¹²⁵ A good discussion of the limitations on such “experimental” approaches is provided by James J. Heckman & Jeffrey A. Smith, *Assessing the Case for Social Experiments*, 9 J. ECON. PERSP. 85 (1995).

Finally, single-cause models require some way of aggregating results. The goal is to understand the combined effect of several causal factors on overall prison rate, and few of the causal factors operate independently. Guidelines, for example, may influence prison populations directly (by restraining how judges sentence) as well as indirectly through any independent effect on the crime rate. To measure the implications of this indirect effect, we need to know both the effect of guidelines on crime and the effect of crime on prison populations.¹²⁶ It will thus be essential to synthesize the results from numerous separate quasi-experiments, which may be challenging.¹²⁷

The second significant adjustment needed in future work is the use of more sophisticated efforts at assessing the robustness of results. Model sensitivity is an important epistemic limitation, and the standard errors returned in regression models usually understate the true magnitude of uncertainty. The errors capture the uncertainty of the estimate *given* the particular functional form, but they do not reflect any uncertainty in that choice of functional form. Daniel Ho and his coauthors provide one way to explore the significance of this latter form of uncertainty.¹²⁸ They regress the variable of interest (percent black, for example) on the dependent variable using every possible combination of all other explanatory variables.¹²⁹ They then plot the distribution of the coefficients returned for percent black, providing a clear sense of the coefficient's sensitivity to functional form.

A second approach is to employ Bayesian Model Averaging ("BMA").¹³⁰ Like the approach set forth by Ho et al., BMA runs regressions under numerous specifications of the model. Rather than graphing the resulting set of coefficients, however, BMA uses Bayesian

¹²⁶ Pfaff, *Sentencing Guidelines*, *supra* note 1, develops a model to capture these direct and indirect effects.

¹²⁷ At one crime and punishment conference, a presenter talking about studies examining the causes of the crime drop of the 1990s quipped that if one were to add up each study's claim of the percent of the drop which that study explained, we have by now explained well over 200% of the drop. The same risk lurks here.

¹²⁸ See Ho et al., *supra* note 76.

¹²⁹ If there are k explanatory variables besides the one of interest, they run $\sum_j \binom{j}{k} - 1$ additional regressions.

¹³⁰ See, e.g., Jeffrey A. Fagan & John F. Pfaff, *Model Uncertainty and the Effect of Incarceration on Crime: A Bayesian Analysis* (2008) (unpublished manuscript, on file with the author); see also William A. Brock, Steven N. Durlauf & Kenneth D. West, *Model Uncertainty and Policy Evaluation: Some Theory and Empirics*, 136 J. ECONOMETRICS 629 (2005); Ethan Cohen-Cole et al., *Model Uncertainty and the Deterrent Effect of Capital Punishment*, AM. L. & ECON. REV. (forthcoming 2008); Jeff Strnad, *Should Legal Empiricists Go Bayesian?*, 9 AM. L. & ECON. REV. 195 (2007).

techniques to develop an average effect across all models and, perhaps more important, a set of augmented standard errors that accounts for uncertainty both within and across models. These results are more reliable than those produced by a single specification.¹³¹

C. VARIABLE CHOICE AND CONCEPTUAL CONCERNS

Thus far I have focused on technical statistical concerns. I want to address now conceptual limitations in the models considered here. Even if there were no concerns about, say, endogeneity or omitted variable bias, it is not always clear that the included variables properly measure the effects they are intended to proxy. The complexity of the prison system indicates that variables must be chosen carefully, and in many of the studies variables suffer from significant shortcomings.

Three key problems in particular stand out. The first, and most significant, applies to regressions looking at the total prison population (as opposed to admissions). It is likely that no regression, no matter how well specified, will be able to identify well the forces influencing the total population, due to the complicated dynamics involved. The second is that some variables do not necessarily align with the relevant theory: one variable is used to test two competing claims, or the variable may simply be too blunt to capture the effect under consideration. And the third is that using aggregate prison populations or admission rates as the dependent variable complicates the interpretation of any causal effect, given the decentralized, hierarchical nature of the criminal justice system.

First, regressions looking at total prison population may not be able to properly identify any causal effects, suggesting that nineteen of the twenty-seven models listed in Tables 2.1 and 2.2 provide little information. Total prison population is a stock variable, and it is therefore the product of current and past admissions and releases; and these variables (with the possible exception of current admissions) are all functions of past explanatory variables. In other words, the prison population in 2000 is a function of (among many other things) the number of admissions in 1995 and the number of releases in 1998, and thus a function of the various forces influencing those admissions and releases (along with the admissions and releases in other years). With the median sentence length in the United States hovering around two years, at least two years of lagged variables, if not more, are needed to begin to explain current total populations. As

¹³¹ See Cohen-Cole et al., *supra* note 130; Fagan & Pfaff, *supra* note 130.

Listokin has noted, the dynamics involved in such a model imply that estimation is challenging at best, and likely impossible.¹³²

This problem may also apply to prison releases, though perhaps not as strongly. Unlike the total prison population, releases are a flow, not a stock. But releases are still highly sensitive to lags: the number of prisoners released in 1999 is a function of previous years' sentencing choices, which are shaped in part by past crime rates, distributions of crimes, economic conditions, demographics, and political pressures. Some simplifying assumptions, however, may provide an avenue for deriving useful results.¹³³

Nonetheless, these dynamic concerns imply that the primary focus must be on measuring prison *admissions*. Unlike total population and releases, admission rates are much less dependent on lagged factors. That is not to say they are immune from lagged effects—judges may serve long terms, for example, so political variables at the time of sentencing may not reflect the politics that placed a particular judge on the bench (and thus the sentencing behavior of that judge)—but their impact should be markedly less.

The second key problem is that too many variables may measure the relevant qualitative factor poorly. Two problems in particular merit attention. First, a single variable may measure a theory with multiple prongs. Politics, for example, can influence prison populations either through the demands of an increasingly conservative polity (as Garland's end-of-modernity thesis suggests) or through the efforts of politicians using crime as a wedge issue. Most studies, however, include a single measure, such as "ideology of citizens" or, more problematically, "percent of the legislature that is Republican." Such a variable is an amalgam of the two theories, but it is unclear in what way.¹³⁴ A similar problem exists with

¹³² As Listokin puts it, "disentangling the effects of various lags of the crime rate on the incarceration rate may be nearly impossible." Listokin, *supra* note 58, at 186. This concern applies to all explanatory variables.

¹³³ See Pfaff, Trends in Prison Release, *supra* note 1. Assuming that the number of prisoners released in 1985 who were committed in 1980 is primarily a function of conditions in 1980 and 1985 only, for example, opens the door for generating tractable results. Data from the National Corrections Reporting Program, a surprisingly underused dataset, allow me to calculate the number of inmates admitted in year t released in year $t+k$, as well as to observe several inmate-specific covariates, such as the class of offense for which each was incarcerated, and the race and sex of the inmate. These data can then be merged with state-level data for years t and $t+k$ to measure the effect of offender- and state-level variables on release decisions.

¹³⁴ That a state with a legislature 60% Republican incarcerates more than one with a legislature 55% Republican does not necessarily validate the conservative-electorate theory. Some seats are more stable than others: if more seats in the 60% state are competitive than in the 55% state, a positive coefficient may in fact be the result of the wedge-issue theory. A quadratic term may be able to shed some light on this—the wedge-issue theory would

race. Despite the presence of at least three separate theories linking race to incarceration—through quotidian racism, through a reaction to civil rights laws, and through the war on drugs—most studies simply include the percent of the population that is black. Such a variable may be sufficient for the first theory, but it is too crude to isolate any role of the latter two. Either terms must be found that isolate the competing effects, or the interpretation of the results must confront directly the inherent ambiguity.

The second problem with variable choice is that too little attention is given to whether more refined variables are available and, if not, what the coarser variables mask. Turning again to race, the variable commonly used is the percent of the population that is black. If (as Bruce Western's work seems to suggest) what matters more is the percent of the state's population that is young, black, male, and uneducated, what does a simple "percent black" term tell us? It could be that this variable is the best we have, but more work is needed to see if more refined terms exist; if they do not, the analyses need to confront more explicitly the epistemic limitations imposed by the cruder measures. This is also a problem in the political context, where attention to legislators and governors overlooks the importance of mayors, district attorneys, and judges.¹³⁵ The focus on the federal government is misplaced as well—while several articles consider the importance of presidential elections, none examines the role of federal financial carrots, such as the funding provided by the Violent Crime Control Act of 1994 to states that adopted strict truth-in-sentencing laws. Given that penal policy is predominantly a state endeavor, the latter is likely more important than the former. And other examples of insufficiently detailed measures can be found: guidelines, for example, differ in terms of their focus on resource consumption or punitiveness, but they are generally modeled as a simple "have/don't have" binary variable.¹³⁶

Again, the point is not to establish an unattainable ideal and then find all current studies wanting; that crosses the line from idealism to nihilism. In no model looking at the sociological, political, and economic forces that drive complex social institutions will the quantitative variables reflect perfectly the underlying qualitative forces. The key concern here is that the articles listed in note 58 spend too little time assessing the implications of

predict it to be negative—but no model has yet considered this possibility. Stucky et al., *supra* note 58, suggests a more direct way to disaggregate these two effects.

¹³⁵ This is surely due in part to limitations in the data. Searches have turned up no consolidated dataset on electoral returns for or party compositions of judges, mayors, or district attorneys. Many state secretaries of state have data on such returns, but it is unclear at this time whether they are in electronic format or would have to be hand-coded.

¹³⁶ Pfaff, Sentencing Guidelines, *supra* note 1, develops a more sophisticated measure of guideline design.

their variable choices. If sufficiently detailed data are available, they ought to be used. If not, close attention should be paid to exactly what a particular coefficient is in fact telling us.

The third conceptual problem arises from the decentralized nature of the criminal justice system. Even with a well-designed regression, it is hard to know exactly what the coefficients on the explanatory variables tell us. For example, assume that a regression with no identification problems returns a (well-estimated) coefficient on "Percent Black" of 2.0, meaning that a one-percentage-point increase in the state's black population raises prison populations by 2 inmates per 100,000 people. What does this mean? Put differently, if one wanted to change this racial effect, what policy recommendations emerge from this regression?

The difficulty is that the criminal justice system is composed of numerous somewhat-independent actors. Once someone commits a crime, he must be arrested, indicted, convicted, and sentenced; when measuring the total stock of prisoners, his eventual release date becomes relevant as well. Each of these stages implicates a separate administrative bureaucracy with different values: the police, the prosecutors, the courts, the parole boards. That one additional percentage point of the population being black increases the incarceration rate tells us little about *where* that increase originates: is it due to racially biased decisions by the police, by prosecutors, by jurors, by judges, by parole boards, or by some combination of the five? Eliminate the assumption that the regression suffers from no (other) identification problems, and the question becomes even trickier.

D. LOOKING UNDER THE STREETLIGHT: THE CHOICE OF DATA

A final concern with all the studies considered here is that, despite their differences, all ultimately look for answers in the same place. Each study effectively uses the same dependent variable, the Bureau of Justice Statistics' one-day counts of prison populations or admissions drawn from approximately fifty states over twenty to sixty years. The explanatory variables are likewise state-level measures often taken from the same sources. Specifications may differ, but the models are all roughly the same. This makes outside verification of results difficult. As others have noted in the crime context, it is important to consider alternate sources of data.¹³⁷

There are two productive routes to consider for future work, namely looking at other jurisdictions and using different U.S. data sets. There are (clearly) two jurisdictional options: go smaller or go larger. To go smaller, one can look at county- or city-level data; Robert Weidner and Richard

¹³⁷ See, e.g., William Spelman, *What Recent Studies Do (and Don't) Tell Us About Imprisonment and Crime*, 27 CRIME & JUST. 419 (2000).

Frase in particular have started working in this direction with respect to counties.¹³⁸ County-level data allow for more explicit measures of factors such as urbanization (and its interactions with race and class). They also permit direct measures of jurisdictional moral hazard problems in criminal justice. In most states, district attorneys and judges respond to local political concerns while the state as a whole incurs the cost of each additional incarceration; concerns with free riding quickly emerge.¹³⁹ State-level data cannot pick up such intrastate politics well. Jeffery Fagan and others have also demonstrated the benefits of city-level data, which allow researchers to develop highly contextual models of the forces that influence prison growth.¹⁴⁰ But such approaches introduce important costs as well. County and city data, for example, may be less reliable, and errors that average out at the state level are less likely to do so at smaller levels; this is a particular concern with crime data.¹⁴¹ Also, certain relevant controls (or instruments, such as abortion rates) may not be available at the county or city level.

The opposite approach is to look bigger, to the international level. Here, however, the costs are more noticeable. The underlying differences between nations are far greater than those between U.S. states, counties, or cities, and nation-fixed effects can control for these differences only if they are constant (and nation-year trends only if they change linearly), a powerful assumption. The relevant factors, then, for which one must control are much larger. Moreover, nations vary greatly in the type of data collected and the quality of such data. Nonetheless, some research, such as that by John Sutton, suggests that valuable insights can be gained by looking beyond national borders.¹⁴²

The second important alternative to consider is different sources of data within the United States. The National Corrections Reporting Program

¹³⁸ Robert Weidner, Richard Frase & Iain Pardoe, *Explaining Sentence Severity in Large Urban Counties: A Multilevel Analysis of Contextual and Case-Level Factors*, 84 PRISON J. 184 (2004); Robert Weidner & Richard Frase, *A County-Level Comparison of the Propensity to Sentence Felons to Prison*, 1 INT'L J. COMP. CRIMINOLOGY 1 (2001).

¹³⁹ See, e.g., Robert L. Misner, *Recasting Prosecutorial Discretion*, 86 J. CRIM. L. & CRIMINOLOGY 717 (1996).

¹⁴⁰ Fagan et al., *supra* note 91.

¹⁴¹ See Maltz & Targonski, *supra* note 108. It is worth noting, however, that even state-level crime data appear to be declining in quality. A good summary is given in Jeffrey A. Fagan, *Death and Deterrence Redux: Science, Law, and Causal Reasoning on Capital Punishment*, 4 OHIO ST. J. CRIM. L. 255 (2006).

¹⁴² John Sutton, *The Political Economy of Imprisonment in Affluent Western Democracies, 1960-1990*, 69 AM. SOC. REV. 170 (2004); John Sutton, *Imprisonment and Social Classification in Five Common-Law Democracies, 1955-1985*, 106 AM. J. SOC. 350 (2000).

("NCRP"), for example, compiles annual offender-level data starting in 1983 on every admission to and release from prison for a (growing) sample of states.¹⁴³ And also since 1983, the biannual National Judicial Reporting Program ("NJRP") gathers a random sample of offender-level data from a random sample of counties on criminal case dispositions. Furthermore, and perhaps most potentially fruitful, many states collect detailed data themselves on their criminal justice systems. A wider range of data sources will either increase confidence in the results obtained (if regularities emerge from well-designed studies) or help isolate problems in the data or in the models (if irregularities arise).

V. CONCLUSION

Zimring and Hawkins optimistically noted twenty-five years ago that the lack of empirical research into the growth of prison populations was not because the question was impossible but simply because few had considered it.¹⁴⁴ Since then, many studies have focused on this significant issue, and they have started to shed light on the driving forces. But while we have learned some from these articles, much still needs to be done.

First, what have we learned? The models looking at admissions appear to be more robust than those looking at total population; this is not surprising, given the complicated lags influencing the stock of total prisoners. Results from the admissions models suggest that unemployment increases the admissions rate (though the magnitudes vary greatly and are not always statistically significant), as do the shares of the population that are between twenty-five and thirty-four years old and that are black. Listokin's results also indicate that a 1% increase in the crime rate leads to approximately a 1% change in admissions, and there is evidence that admissions rates fall as personal income rises. In the models looking at total prison population, the only consistent results are for the percent of the population that is between twenty-five and twenty-four years old and that is black. No other effect is robust across the articles.

But future work can take several steps to generate more accurate results. To summarize briefly:

1. Closer attention must be given to issues of endogeneity, not just with respect to crime rates, but unemployment, sentencing guidelines, and other factors.
2. More care is required in confronting sensitivity to functional form. At the very least, the data should be balanced and rigorous robustness

¹⁴³ Pfaff, Trends in Prison Release, *supra* note 1, uses the NCRP to examine in more detail the forces influencing release rates since the mid-1980s.

¹⁴⁴ ZIMRING & HAWKINS, *supra* note 3.

checks applied. These will allow us to better appreciate the epistemic implications of structural sensitivity. But other improvements are possible as well. One goal is to adopt more consistent sets of estimators to avoid omitted variable bias. A more significant improvement is to look for more quasi-natural shocks to many variables of interest.

3. The particular choice of variables demands more consideration. Prison admissions—and, in certain cases, releases—need to replace total prison population as the dependent variable. More effort is required to develop increasingly precise proxies, and ones that can isolate competing theories related to a particular causal force (such as whether increasing conservatism or electioneering drives the relationship between politics and prisons). Furthermore, while focusing on the aggregate admissions rate is informative, more useful still is a model that examines the role of the causal factors at each stage of the process (arrest, prosecution, conviction, admission, release).

4. Studies should seek out alternate sources of data, either looking to different jurisdictions (counties, nations) or different sets of data (NCRP, NJRP, state-gathered data).

The current models may not have answered the question of population growth fully, but they have begun an important process. With more powerful techniques, better appreciation of particular structural problems, clearer understandings of how the data look, and careful selection of relevant explanatory variables, we can—and will—derive increasingly reliable estimates of the forces shaping prison populations today.

