Book Review of *Prisoner Reentry and Crime in America*

David M. Bierie
University of Maryland, College Park

In *Prisoner Reentry and Crime in America*, Travis and Visher set out to review and update the field regarding facts, trends, and ideas surrounding prisoner reentry. Individual authors focus on describing facts, interpreting those facts, and suggesting implications for policy and research. More specifically, the book provides detailed information on prisoner reentry within three areas. First, it offers a rich account of the people who make up this population (demographics and needs). Second, the book provides a detailed discussion of the institution of parole, how it works and how it has changed over time. Third, it suggests that intense use of incarceration and the resulting high rate of prisoner reentry may bode ill for crime rates and public safety for several reasons (i.e., prison may diminish bond-potential of inmates, further damage disadvantaged neighborhoods, etc.). Perhaps the most important contributions of this book is the way in which it directs readers to new and emerging themes in the literature; ideas that may prove fertile ground for future research (such as the specific study of 'churning' parolees, or the potential age-graded impacts of correctional experiences). Below, I summarize individual chapters while focusing on key methodological issues and new ideas that each chapter offers. The goal here is not an exhaustive summary of any chapter. Nor is it to delineate technical critiques of chapters. Rather, my goal is to present those themes and ideas that got me excited as I read; the ideas that had me scurrying to write down notes for new papers I want to write; to present those ideas that will (hopefully) inspire others to read this book.

Summary of Individual Chapters

Petersilia's chapter describes the nation's inmate population, focusing on changes over time in demographics and needs (such as substance abuse, mental health, and educational deficits, etc.). The chapter is motivated by the belief that, "Understanding offenders' crimes and life circumstances is a necessary precursor to designing successful reintegration programming" (p.15). She also argues that incarceration can make people better, have no impact, or make people worse (three outcomes). Which outcome occurs is tied to several factors. It involves (a) programming issues, such as identifying the needs of specific inmates, tailoring treatment to those needs, and ensuring effective delivery of interventions. Likewise, outcomes will be influenced through (b) the culture of institutions, as these have an independent impact on personality development over time and generating contexts that are either conducive or antithetical to prison programming. Tailoring her description to the arena of reentry, she uses data from a classic BJS inmate survey but focuses her analyses on the subset of inmates who are to be released within 12 months. Using these data, she describes the demographics and needs of these inmates (including a discussion of employment and education deficits, substance abuse needs, and mental health considerations). This work is similar to her previous book, *When Prisoners Come Home: Parole and Prisoner Reentry* (2003), and readers who are captivated by Petersilia's writing style and command of the literature in this chapter should consider reading her previous book as well.

Maruna and Toch continue the discussion of incarceration and set out an ambitious chapter with three goals. First, they offer a more theoretically informed discussion of desistance than is usually found in this type of volume; presenting new terminology and new approaches to desistance. For example, they present the terms "primary" and "secondary" desistance to illustrate the idea that there may be distinct causal processes to (a) slowing down versus (b) full desistance from crime.[1] Second, they tie their theoretically-informed discussion to the issue of prisons themselves: how they work and how they impact the process of desistance. Third, they present a bold challenge to the field in their review of the incarceration literature by suggesting a drastic reorganization of the way prison management is approached in the U.S. (i.e., inmate democracies, prisons without guards, and faith based prisons).[2] Whereas Petersilia focuses on describing people in prisons, Maruna and Toch focus more on how the system deals with those prisoners.

Turning their attention to post-release events, Piehl

and LoBuglio provide a well-written and balanced review of the dominant issues in the study of parole. They use their time to fully elaborate the nuances and relevance of supervision style to criminal behavior (i.e., discretionary and mandatory release schemes), the role and meaning of technical violations, monitoring technologies, changes over time, and the implication of these issues for policy and especially research. This includes discussions of differences across states in terms of parolee composition, agency terminology, and revocation policies/patterns. They also note the pitfalls of relying on methodologically flawed research designs to compare policy decisions (i.e., using non-randomized designs to compare recidivism between discretionary and mandatory release inmates) and present suggestions for shoring up the quality of extant research. Building on their discussion of the research, trends, and facts of the field, the authors conclude with an innovative suggestion: public safety may best benefit from dividing post-release supervision into two agencies. One would focus on monitoring and constraint, the other on delivery of services and social welfare of parolees (p.135). This suggestion is grounded in the observation that the current system of assigning both tasks to one office (parole) has often resulted in role conflicts in which supervision and control have won out as dominant paradigms at the cost of service delivery.

Complementing this discussion, Blumstein and Beck provide a data-driven account of changes over time in parole population growth, the nature of parolee cohorts, and their contribution to the growth in prison populations across and between states. They ground the discussion in their classic article (Blumstein and Beck 1999) which articulated the contribution of various stages of the justice process to prison population growth from 1980–1996. However, they improve upon their earlier analysis by using the same data in that article as well as adding in new data (1996–2001) to extend the analysis an extra 5 years. In doing so, they identify some divergent trends from their earlier paper (i.e., that the contribution of "parole recommitments" to prison population growth has increased from 15% between 1980 and 1992, to 60% between 1993 and 2001). Delving further into the data, they find that first time parolees have a recidivism rate of approximately 60 percent. In contrast, recidivism increases to 75 percent on each subsequent parole period (i.e., a 75% chance of recidivism on second parole terms, on third parole terms, on fourth parole terms, etc.). The chapter will serve as motivation for future researchers to investigate the causal mechanism(s) for this change (i.e., selection, causal, etc.).

Rosenfeld, Wallman, and Fornangi focus on assessing the popular claim that "recent crime-rate spikes in large cities are a function of increased numbers of parolees returning from prison and causing havoc." To tease out the issue, they reanalyze a landmark data set which followed exiting prison cohorts (N=30,431 parolees) from 13 states in 1994–1997 (Langan and Levin 2002). Their data imply ex-prisoners do offend at higher rates than the general public; exhibiting odds of arrest that are 18–26 times higher than the average non-parolee (depending on the crime type examined). Importantly, they show the finding hinges on the observation that the general population crime rate has decreased over time (i.e., parolee 'contribution' would increase even if parolee recidivism is constant over time when the general population crime rate drops). Further, the data indicate an interaction with race. The difference in arrests rates between parolees and the general population is far more pronounced for whites (20–29 times higher arrests rates among parolees) relative to blacks (7–9 times larger arrest rates among parolees). However, despite their higher proclivity towards arrests, parolees contribute only between 4.5–6.5 percent of the crime rate in the year following their release. This is because parole populations are dramatically small compared to the population of the nation, a fact which far outweighs the higher arrest rates of parolees.

Next, the authors estimate marginal effects of "release type" on recidivism after controlling for demographics, criminal history, and fixed effects across states. Holding all other independent variables at their mean, they find discretionary-release parolees have 36 percent fewer arrests for violent crimes than inmates who max out their sentence (released unconditionally). Likewise, mandatory-release inmates have 19 percent fewer violent arrests than inmates who maxed out their sentence (although the latter difference is not statistically significant).

There are at least two interpretations of these findings. First, the release type could be causing the difference (i.e., inmates who qualify for discretionary release may have more motivation to participate in programming, and thus have lower recidivism rates than mandatory release inmates or maxed out inmates). This interpretation hinges on the assumption that the authors' control for propensity (criminal history, age, gender, race, and fixed effects of states) is near perfect. A second possibility is that there are still unmeasured differences among the groups making up the release types which are driving the differences observed. For example, the authors do not have measures of employment history, substance abuse history, educational history, mental health history, family background, social capital, social bonds, self control,

characteristics of neighborhoods parolees returned to or came from, etc. In as much as these variables matter above and beyond those which were included in the equation (and in as much as these variables impact which release type occurs), the differences may be attributable to selection bias rather than a causal force of parole release type.

The authors assumed the first interpretation (that the type of release is indeed causal) and then proceed to ask, "Suppose we were to shift 10% of the prisoners in our sample who would have been released on discretionary parole to the category of unconditional release. What effect would this have on the overall incidence of arrest?" (p. 97). They go on to show that there would be substantively large increases in recidivism if more and more offenders had been shifted from discretionary release to unsupervised (they repeat the exercise under the rubric of shifting inmates from discretionary release into mandatory release as well). The authors conclude that these analyses "lend support to the continued use of discretionary parole release for purposes of reducing recidivism" relative to other release types (p. 99).[3]

Continuing to focus on the relationship between parole and crime rates, Clear, Warring and Scully tackle the issue of mass-incarceration from a community perspective. Relying on data from Tallahassee, they show that specific neighborhoods (the most disadvantaged) are the subject of the most intense removal of citizens to prison, as well as the most concentrated influx of parolees released from prison. They argue that concentrated cycling of residents in and out of jail reduces social capital of the parolees, their families, and their communities. A crucial idea they offer is that managing the cycling process substantially drains community resources by injecting cynicism or anti-social norms into an area, as well as depleting the time and energy of residents to exert effort on neighborhood goals. When citizens are constantly tackling issues related to returning prisoners (or citizens exiting to prison), the collective efficacy of an area may be severely damaged. The chapter's major contributions rest in the authors' ability to conceptualize, articulate, and test assertions about community level impacts of "reentry cycling" on crime at the group and individual level. The chapter will also serve to motivate future scholars to determine under what conditions and to what degree reentry cycling both hinders and helps these areas. For example, Kennedy (1998) argues that intense arrest rates are supported by many non-criminal residents in urban areas plagued by crime. Likewise, others take pains to articulate the conditions under which intense policing can reduce crime under certain conditions (Kelling and

Coles 1996). Thus, there is a clear need to build on the work Clear and his colleagues have done by continuing to articulate the diverse ways in which "reentry cycling" impacts communities.

Uggen, Wakefield, and Western approach an analogous topic to Clear et al., as they focus in on the deleterious impact incarceration can have on family formation and employment potential of offenders. They argue that prison often causes (a) an erosion of job skills, and (b) stigma which inhibits employers from hiring ex-cons. Likewise, they suggest mass-incarceration may generate decreased marriage rates because (c) it takes people out of the marriage pool, and (d) makes people less marriageable once they return to the community. Inasmuch as incarceration places obstacles to marriage and work, incarceration is viewed as a dangerous intervention, potentially increasing crime. As I read this chapter, I see it as presenting several challenges to the field. First, it challenges researchers to identify the frequency by which incarceration is iatrogenic, when it has no impact, and when it generates a more sound, healthy, and compliant citizen. The authors set the stage for such an investigation by noting that key pathways to varying outcomes likely involve employment and marriage (see Laub and Sampson 2003 for an important discussion of these and other pathways). In as much as prisons can increase GED rates among drops outs, teach and certify inmates in trades, treat inmates for substance abuse problems, life skills deficiencies, and cognitive deficits, prisons may actually improve prisoners' potential marriage-desirability or employability. By focusing on the harm prisons can cause within these realms, the chapter motivates future research to articulate how often this happens, why, and what can be done to make prisons improve people's lives.

Finally, the authors conclude with a relatively new and exciting idea: what if correctional experiences are age-graded? They argue that the timing of experiences, such as incarceration, may have a differential impact because the social construction of age generates differential motivation among inmates to participate in programming with sincerity. Ultimately, the implication is that correctional interventions may be more or less effective with certain subgroups of offenders (the young versus the older, men versus women, parents versus non-parents, etc.). In reading this chapter, it seems that the challenge to the field will not only be to identify if there are interactions, but also to develop the theoretical framework to understand and predict them. For example, an age-interaction may be tied to the social construction of age (as the chapter implies). However, perhaps age correlates with (a) differing average-needs, and/or (b)

differing amenability to specific intervention delivery styles (i.e., the young and older inmates may respond differently to a boot camp experience). In this case, correctional programming may be made far more efficient by using this information to target programming types (i.e., interventions designed to target specific needs) and delivery styles to subgroups of offenders.

Summary

This book was an exciting read, and I would recommend it to policy makers, graduate students, and especially to academic researchers. I give such a broad recommendation because the authors have clearly written each chapter with an eye to providing something important to each of these audiences. Perhaps the most exciting aspect of the book is that the editors have woven diverse topics and formats into a readable whole. Each chapter provides direction on how to improve the current research (either by example, or in essay form). Likewise, each author was careful to outline the policy implications of ideas and data presented in their respective chapters. As I read the book, I found that I spent just as much time writing notes for this review as I spent writing down notes for myself; notes on how to answer questions the authors raised ("gaps" in the field yet to be tackled) or notes on other "gaps" that may not have been mentioned in the book explicitly, but become present after thinking critically about the implications and assumptions of what these authors were saying. I hope this book has the same effect on other readers, as I am sure it will.

Endnotes

1. The authors ground their presentation of the terms "primary and secondary" desistance in symbolic interaction traditions, and to some degree this may bias some readers against the presentation of the idea (because some will be more or less apt to buy into the implications of terms grounded in Lemert's specific conceptions of the way people work). For example, Maruna and Toch could just as easily have chosen the terms "absolute and restrictive" desistance to draw out the same ideas (but that would have brought with it rational choice baggage, which could pose similar problems for some readers). In short, it is important to separate the descriptive insight (that we should investigate whether there are differing pathways to small and large reductions in offending) from theoretical baggage implied in the terms. Otherwise, it can be too easy to dismiss an idea due to its associations rather than its merits.

2. Their discussion generates creative ideas regarding management. However, I would urge caution when endorsing the suggestions presented in light of other work which articulates unintended consequences such as prison disorder as well as increased victimization and exploitation that may result from prisons which rely on inmates for security or operational roles in management. Interested readers should see Bottoms 1999 and especially DiIulio 1987 for ideas that may qualify/counter some of the assertions made by Maruna and Toch.

3. To be clear, a skeptic may interpret these findings as implying that "we would have a great increase in recidivism if we had crime-prone offenders". Again, interpretation of these data hinge on whether the reader is willing to buy the assumption that the items used to capture propensity create the equivalent of a randomized experiment. In as much as they fail to do this, the estimates may reflect the impact of criminal propensity on recidivism, rather than a causal force tied to release type. To be fair, the truth probably lies somewhere in between the two extreme interpretations. The innovative and detailed analysis in this chapter will no doubt serve as motivation for future research geared at teasing out which of these interpretations is more accurate.

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