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# EDITORIAL INTRODUCTION

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## DISADVANTAGE AND SENTENCING OF BLACK DEFENDANTS

### Examining the “Life Course” of Criminal Cases

#### A New Frontier in Sentencing Research

**Brian D. Johnson**

*University of Maryland*

Few issues in crime, law, and justice have garnered greater attention from legal scholars, politicians, or the American public than racial disparities in the criminal justice system. African-American defendants are overrepresented at each stage of the justice system—they are disproportionately arrested, detained, and incarcerated relative to their proportion in the general population (Walker, Spohn, and Delone, 2012). Recent years have witnessed growing momentum for more nuanced empirical approaches to the study of racial disproportionality with an emergent emphasis on cumulative disadvantages that accrue across the life course of criminal case processing decisions (Kutateladze, Andiloro, Johnson, and Spohn, 2014). Much like life-course criminology emphasizes changing life events and their differential effects on criminal behavior across developmental stages (e.g., Sampson and Laub, 1993; Thornberry, 1987), emergent perspectives on criminal case processing stress the differential and cumulative impact that race and other offender characteristics exert across the “life course” of successive decision-making stages in the justice system.

The research article and policy essays presented herein highlight the importance of moving from single-stage analyses that present only a snapshot of individual outcomes toward more dynamic, multistage investigations that account for both the direct and the indirect causal pathways that can contribute to racial disadvantage. They offer important contributions to our understanding of racial disparity, provide unique insights into cumulative and offsetting racial impacts, and contribute new evidence to ongoing policy debates over the locus and magnitude of racial disproportionality in the justice system.

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Professors Wooldredge, Frank, Goulette, and Travis (2015, this issue) provide several noteworthy contributions. Their work examines a series of interrelated decision-making stages, including bond and detention decisions, charge reductions, and sentencing outcomes, using a large sample of felony defendants. It employs sophisticated analytical techniques that combine multilevel and path analytic approaches to assess both direct and indirect effects of race on punishment. It includes seldom-investigated indirect sources of racial disparity, including bond amounts, attorney type, and criminal history, and it compares cumulative disadvantages not only for Blacks and Whites but also for young, Black males specifically. The weight of the evidence from their research suggests significant disadvantages accumulate for Black defendants, much of which is attributable to presentence differences in criminal history and detention status, and for young, Black men, disparities in financial bond amounts. Through these collective findings, Wooldredge et al. (2015) demonstrate that indirect race effects are highly consequential, leading them to conclude that “racial disparities in imprisonment can persist in a correctional system even when a defendant’s race is excluded from consideration by judges at sentencing.”

In her thoughtful policy essay, Cassia Spohn (2015, this issue) provides a brief historical overview of empirical research on racial disparity in sentencing, noting important methodological advancements that have been made along the way and reiterating the importance of addressing the issue of cumulative disadvantage. She provides a concise summary of key findings from Wooldredge et al.’s (2015) study and observes that the most direct policy implication is a reduction in the use of financial release mechanisms during pretrial release decisions. Spohn (2015) also discusses the importance of addressing other racially charged sentencing policies, including mandatory minimums, three-strikes laws, and the routine acceptance of criminal history as a legally relevant and race-neutral factor in sentencing. She notes that additional empirical work is clearly needed that incorporates these issues into future analyses of cumulative disadvantage and that future policy initiatives are also needed that are aimed at reducing racial disproportionality in the criminal justice system.

Schlesinger (2015, this issue) argues in her policy essay that the social and financial cost of detaining criminal defendants prior to trial is substantial and suggests that available resources may be better used to support social services for these individuals. She highlights the negative impact of pretrial detention on future case processing and broader life outcomes, placing the U.S. experience in valuable international context and astutely pointing out the moral and constitutional contradictions involved in detaining suspects who have not yet been convicted of a crime. Schlesinger (2015) also identifies several promising directions for improving our current pretrial release system. In particular, she discusses the use of risk-assessment tools as an alternative to fixed bond amounts, noting some important caveats and potential pitfalls, as well as recent reform efforts in the juvenile justice system. She recommends policies of universal release to help reduce mass incarceration and racial disparity in the justice system. Finally, she also recommends pronounced changes in policing

tactics, including hot-spots, gang initiatives, and stop-and-frisk policies, along with the discontinuance of all consideration of defendants' prior records in criminal case processing. Collectively, these recommendations are bold, innovative, and thought provoking, and they offer a valuable starting point for future policy debates.

The collective policy and research implications of these works are substantial. First, they suggest that cumulative racial disadvantages in the criminal justice system have far-reaching societal consequences. Disproportionate detention and confinement of large numbers of young minority men feed into other types of social stratification, contributing to racialized patterns of employment, family formation, and residential segregation (Western, 2007), and reducing the ability of local communities to self-regulate and effectively maintain informal social control (Clear, 2007). Second, overreliance on financial release mechanisms and criminal records may have unintended racial consequences. Pretrial release based on financial bonds is directly related to ability to pay and therefore disadvantages penurious defendants, whereas the specter of biased policing practices hangs over racial differences in criminal records, which contribute to more severe punishment outcomes. It may be time to consider a new era of bail reform aimed at reducing reliance on pretrial detention and increasing fairness and consistency in pretrial release decisions, especially for defendants with limited financial means. It is also necessary to be racially sensitive to the various factors that contribute to prior records, which suggests additional research is needed on the underlying sources of racial differences in criminal history.

Finally, the findings from this research raise important normative concerns for both criminal justice policy and the future of sentencing research. Estimates of racial inequality that are based on single decision-making stages may offer an incomplete picture of the cumulative disadvantages associated with the systemic and processual nature of the criminal justice system. It is first necessary to identify the magnitude and locus of unwarranted disparity before evidence-based policy initiatives can be developed and implemented to address it. As one recent report concluded, "The problem of racial disparity is one which builds at each stage of the criminal justice continuum . . . [w]ithout a systemic approach to the problem, gains in one area may be offset by reversals in another" (Sentencing Project, 2008: 2). Public policies and future research targeting racial disparity must therefore continue to focus on the impact that race and other relevant sentencing factors exert not only at one decision-making stage, but also across the entire "life course" of cases in the criminal justice system.

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**Brian D. Johnson** is an associate professor of criminal justice and criminology at the University of Maryland. He is a former recipient of the ASC Ruth Shonle Cavan Award and the ASC Division on Corrections and Sentencing New Scholar Award. His research interests involve the study of race and social inequality in court actor decision making, with recent work focusing on the influence of offender appearance in sentencing, as well as the causes and consequences of contextual variations in criminal punishment.



# Is the Impact of Cumulative Disadvantage on Sentencing Greater for Black Defendants?

**John Wooldredge**

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## Research Summary

*We examined race-group differences in the effects of how felony defendants are treated at earlier decision points in case processing on case outcomes. Multilevel analyses of 3,459 defendants nested within 123 prosecutors and 34 judges in a large, northern U.S. jurisdiction revealed significant main and interaction effects of a defendant's race on bond amounts, pretrial detention, and nonsuspended prison sentences, but no significant effects on charge reductions and prison sentence length. Evidence of greater "cumulative disadvantages" for Black defendants in general and young Black men in particular was revealed by significant indirect race effects on the odds of pretrial detention via type of attorney, prior imprisonment, and bond amounts, as well as by indirect race effects on prison sentences via pretrial detention and prior imprisonment.*

## Policy Implications

*The consideration of cumulative disadvantage is important for a more complete understanding of the overincarceration of Blacks in the United States. Toward the end of reducing racial disparities in the distribution of prison sentences, courts might (a) reduce reliance on money bail, (b) consider bail amounts for indigent defendants more carefully, and (c) increase the structure of pretrial decision making to reduce the stronger*

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*effects of imprisonment history and type of attorney on the odds of pretrial detention for Black suspects.*

### **Keywords**

*sentencing disparities, cumulative disadvantage, bail, pretrial detention*

Great strides have been made over the last two decades in our understanding of race-group differences in sentence severity across state and federal courts in the United States (for an overview of major and emerging themes in the empirical literature, see Ulmer, 2012). Recent discussions of possible directions for future research have underscored the need to examine empirically the “cumulative” disadvantage throughout the court system, or how minorities might be treated at earlier decision points (e.g., pretrial detention) and the impact of those dispositions on sentencing (Baumer, 2013; Bushway and Forst, 2013; Kutateladze, Andiloro, Johnson, and Spohn, 2014; Rehavi and Starr, 2012; Spohn, 2009; Stolzenberg, D’Alessio, and Eitle, 2013; Sutton, 2013; Ulmer, 2012). The ability to examine decision making at multiple points in the system permits an assessment of both the direct and indirect effects of a defendant’s race on the severity of case outcomes (Spohn, 2009). The idea that outcomes at earlier decision points in the criminal justice system can impact outcomes at later decision points and that such accrued disadvantages can be harsher for minorities is not new (Chambliss and Seidman, 1971; Hagan, 1974; Klepper, Nagin, and Tierney, 1983; Lizotte, 1978; Zatz, 1987), but empirical tests of this idea have only recently become much more feasible because of the improvements in accessing data on multiple decision points for large samples combined with advancements in path modeling.

An examination of cumulative disadvantage in case processing differs from the more traditional empirical focus on cross-sectional models of separate decision points. The latter focus remains important because it reveals whether race could be significantly linked to particular dispositions and outcomes as well revealing the magnitude of those linkages, including the degree to which a defendant’s race might *condition* the effects of other demographic and legal factors. However, a more traditional focus alone ignores the extent to which race effects on a particular outcome (e.g., a prison sentence) are direct versus indirect via preceding dispositions (e.g., pretrial detention) (Lizotte, 1978; Spohn, 2009). A focus on *indirect* race effects can supplement analyses of cross-sectional models with assessments of how disadvantages might accrue differently throughout case processing for different race groups. Spohn (2009) demonstrated the relevance of examining both direct and indirect race effects for a more comprehensive understanding of racial disparities in sentencing. We incorporated analyses of both direct and indirect race effects on case dispositions and outcomes for a sample of 3,459 persons indicted on felony charges in a northern U.S. urban jurisdiction.

### **Cumulative Disadvantage in Case Processing**

“Cumulative disadvantage” reflects a sequence of undesirable events whereby the occurrence of earlier negative events increases the odds of subsequent negative events. For example, offenders who are sent to prison face greater hardship in gaining meaningful employment after release, which in turn can impact other quality-of-life factors (advancing one’s education, achieving financial independence, etc.) (Bushway and Sweeten, 2007). Applied in the context of criminal case processing, less desirable dispositions at earlier decision points in a court system (pretrial detention) could increase the likelihood of less desirable outcomes at later decision points (conviction and imprisonment). Sutton (2013: 1208) referred to this process as “accelerating bias” while noting that a common theme in the sentencing literature is that pretrial detention is often linked to higher odds of prison sentences after conviction (for a summary of this literature as well as contrary findings, see Reitler, Sullivan, and Frank, 2013).

The argument of cumulative disadvantage in case processing applies to all individuals regardless of race, but the magnitude of cumulative disadvantage might be greater for Black suspects in light of extant findings of harsher sentences for Blacks than Whites (for reviews, see Kutateladze, Andiloro, Johnson, et al., 2014; Stolzenberg et al., 2013; Ulmer, 2012). Granted, these harsher sentences could result strictly from judges’ or prosecutors’ explicit or implicit considerations of a defendant’s race in sentencing decisions and plea agreements (see Kutateladze, Andiloro, and Johnson, 2014, for a discussion of court actors’ “implicit” considerations of a defendant’s race), but they could also reflect the impact of harsher dispositions for minorities at earlier decision points if those earlier decisions influence subsequent decisions apart from a defendant’s race. In other words, consideration of a defendant’s race at separate decision points might compound race effects on sentence severity. From this perspective, existing theories of extralegal disparities in separate case-processing decisions might also be applicable to an understanding of cumulative disadvantage.

Scholars have argued that generally harsher sentences for Blacks could result from court actors’ use of discretion and the often limited information available for decision making (Albonetti, 1987, 1991; Steffensmeier, Ulmer, and Kramer, 1998). In the absence of all relevant information regarding a suspect’s culpability, risk for reoffending, and danger to the community, prosecutors and judges might base their decisions on past decisions in similar cases (Albonetti, 1987, 1991; Hawkins, 1981). This could lead court actors to stereotype certain types of defendants, resulting in prosecutors dropping charges against defendants that (they anticipate) jurors are more likely to sympathize with, and judges incarcerating convicted defendants perceived to be more dangerous to the community. Regarding the latter, Steffensmeier et al. (1998) discussed judges’ considerations of offenders’ extralegal attributes in the context of “focal concerns” and how they weigh the impact of their decisions for crime control.

These perspectives of court actors’ use of discretion could be used to frame the relevance of cumulative disadvantage in case processing because outcomes at earlier decision points

could be “risk” factors considered by judges to reduce uncertainty in their sentencing decisions (Sutton, 2013), which is consistent with the idea that “the punishment process (is) a dynamic set of interrelated decision-making points” (Kutateladze, Andiloro, Johnson, et al., 2014: 515). Specifically, defendants detained prior to trial because of the denial of bond or an inability to post bond could be perceived by trial court judges as more dangerous offenders. If Black suspects also face higher odds of pretrial detention because of higher bond amounts stemming from greater perceived “threats” to the community (following Albonetti, 1987; Hawkins, 1981; and Steffensmeier et al., 1998), then the combination of higher odds of pretrial detention for Black suspects and higher odds of prison sentences for pretrial detainees in general could result in higher odds of imprisonment for convicted Black defendants apart from judges’ explicit or implicit considerations of a defendant’s race in their sentencing decisions. Pretrial detention resulting from failure to post bond leads to unemployment, lack of housing in society, and strains on family bonds that render a defendant a less suitable candidate for probation as opposed to incarceration at sentencing. Spohn (2009: 881) succinctly described this as a greater “detention penalty” for Black offenders.

It is possible that Black suspects, on average, are less able to post even similar bond amounts to those for White suspects because of the disproportionate overrepresentation of Blacks relative to Whites in more poverty-stricken environments (e.g., Rose and Clear, 1998). By no means are we saying that being Black is a necessary condition of impoverishment, but a history of racial oppression and segregation in the United States has produced a seemingly pervasive link (albeit imperfect) between race and socioeconomic status. As such, pretrial release might be less attainable for Blacks even without racial disparity in bond amounts.<sup>1</sup>

### *Evidence of Cumulative Disadvantage*

Few empirical studies of race-group differences in cumulative disadvantage have been published (Chen, 2008; Kutateladze, Andiloro, Johnson, et al., 2014; Rehavi and Starr, 2012; Schlesinger, 2007; Spohn, 2009; Stolzenberg et al., 2013; Sutton, 2013; the term is not specifically used by some of these scholars), and only a handful have examined this issue directly with individual-level data. Chen’s (2008) study was an aggregate-level analysis, and Schlesinger (2007) implied cumulative disadvantage from significant effects of race on pretrial detention and, in turn, between pretrial detention and sentencing.

Rehavi and Starr (2012) examined race-group differences in charges initially filed by federal prosecutors between 2007 and 2009 to understand racial disparities in prison sentence length. They found a strong link between racial disparities in charge severity and

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1. An example of economic inequities between the samples of Blacks and Whites examined here can be found in Table 1. Note the smaller portion of Black defendants with hired counsel (0.15) relative to White defendants (0.25).

sentence length for Black males, who were twice as likely as White males to face charges with mandatory minimum prison terms. Black males therefore faced more severe prison sentences in general because of the racially disproportionate disadvantages stemming from more severe charges at arraignment. Restricted data provided by the Bureau of Justice Statistics offered a rare opportunity to examine data compiled from arrests through sentencing.

Also related to prosecutorial discretion in charging decisions, Shermer and Johnson (2010) found no significant racial or ethnic disparities in charge reductions after initial charging in U.S. federal courts. Although charge reductions were, in turn, inversely related to sentence severity, their finding suggested no *greater* cumulative disadvantage to minorities as a consequence of racial disparities in charge reductions accompanying plea agreements.

Stolzenberg et al. (2013) examined Bureau of Justice Statistics data on cases processed in the largest 65 U.S. counties between 1990 and 2004. They examined race effects on bail decisions, pretrial detention, felony adjudications, and sentencing. Race effects were significant only in the sentencing models (odds of incarceration and sentence length), but a meta-analysis of their estimated race effects on all eight outcomes revealed that the odds of more severe sanctions *in general* were 42% higher for Blacks than for White Anglos. However, Stolzenberg et al. did not specifically examine how earlier dispositions impacted sentencing decisions. When using some of the same data for cases processed in the year 2000 across 75 urban counties, Sutton (2013) found support for a stronger effect of cumulative disadvantage for Black defendants in that Blacks detained prior to trial were 26% more likely to go to prison than White Anglos detained prior to trial.

Related to our specific focus on the indirect effects of race on sentencing via pretrial detention, Spohn (2009) examined these effects on the length of imprisonment for a sample of federal drug offenders. She found a significant indirect race effect in conjunction with a nonsignificant direct race effect on sentence severity, noting that an analysis limited to direct race effects would have missed an important link between a defendant's race and sentencing. The most recent related study to date by Kutateladze, Andiloro, Johnson, et al. (2014) revealed greater disadvantages for Black and Latino defendants (relative to Whites) in New York County at pretrial, during plea negotiations, and at sentencing, and that certain combinations of these decisions posed even greater disadvantages for one or both minority groups (such as one third of all Black suspects experiencing pretrial detention, no dismissals, and sentences of incarceration vs. 28% of White Anglos).

Studies conducted to date on this topic have been ambitious, but additional studies are needed given their limited number in conjunction with the relatively modest pool of control variables tapping legally relevant case factors (limited by the data available to these scholars, but see Rehavi and Starr, 2012; Shermer and Johnson, 2010). Kutateladze, Andiloro, Johnson, et al. (2014) also observed the potential importance of exploring alternative statistical methods for examining cumulative disadvantage, such as the use of path analysis and hierarchical modeling. These particular methods were adopted for the study described here.

Related studies to date also have not examined whether cumulative disadvantage is even greater for young, Black males (Steffensmeier et al., 1998). A theme underlying the research conducted over the past 16 years is that the stereotypes of more dangerous offenders held by public officials might not be based *solely* on a suspect's race but on a combination of demographic factors reinforcing images of offenders at higher risk for recidivism, such as young, Black males (Steffensmeier et al., 1998). Many studies conducted at the state and federal levels have produced evidence favoring these ideas (reviewed by Johnson, Ulmer, and Kramer, 2008; Ulmer, 2012).

Aside from studies of cumulative disadvantage per se, analyses of stages of case processing prior to sentencing have expanded considerably (examples from the past decade other than those described previously include Demuth and Steffensmeier, 2004; Shermer and Johnson, 2010; Schlesinger, 2007; Ulmer, Eisenstein, and Johnson, 2010; Wooldredge, 2012), but the number of related studies still pales in comparison with studies focused solely on sentencing. Most scholars have also typically focused on one particular stage of case processing, such as pretrial release *or* plea bargaining *or* sentencing. Although still very important, analyses of single decision points cannot reveal the disadvantages that accumulate *across* decision points to create harsher final outcomes (Stolzenberg et al., 2013). For example, race effects on bond amounts for pretrial release could make it more difficult for Blacks to obtain release. Pretrial detention, in turn, might increase the odds of conviction (Goldkamp, 1979), and higher odds of conviction mean greater eligibility for imprisonment. The ability to follow the same defendants through pretrial detention, conviction, and sentencing permits an examination of how earlier decisions influence later decisions. Although pretrial detention is not an official "decision," the bond amount set by a judge *is* an official decision, and examining how bond amounts influence pretrial detention is important for assessing cumulative disadvantage based on the *decisions* of court actors. Failure to post bond is one thing, but the decision to impose a bond that is too high for the defendant to meet is different. For this reason, and given the punitive nature of pretrial detention, we also examined indirect race effects on pretrial detention via bond amounts.

Other factors aside from bond amounts and pretrial detention might also contribute to greater cumulative disadvantages for Black defendants. First, minority suspects could be less likely to hire their own attorneys because of their overrepresentation as indigent defendants (Stolzenberg et al., 2013), and scholars have argued that hired attorneys (as distinct from private attorneys who also serve as court-appointed attorneys) are better able to secure less severe sentences after conviction (Casper, 1972). Second, histories of imprisonment are more common among Black suspects and are significantly linked to more severe sentences (Welch, Gruhl, and Spohn, 1984). Although prior imprisonment is a legitimate consideration in sentencing decisions in the jurisdiction examined, a stronger link between race and prior imprisonment would necessarily serve as an even greater disadvantage to minority defendants. This might also be important to consider given that

empirical models of sentencing always include controls for prior record and, as such, could actually water down the main effects of a defendant's race on sentencing. That is, if race is linked to criminal history, then controlling for criminal history might remove variation in sentencing that is still ultimately tied to race. Incorporating prior record as a mediator in related studies might be useful for decomposing race effects.

### *Extant Findings on Racial Disparities in Pretrial Release and Sentencing Decisions*

A preliminary step to an analysis of race-group differences in the effects of cumulative disadvantage on sentencing requires an analysis of each case-processing stage separately to assess race effects at each stage. In other words, indirect race effects on sentencing will be null if any of the *direct* race effects on possible mediators are null (e.g., a null race effect on pretrial detention would render a null indirect race effect on sentencing via pretrial detention). It is therefore important to review the empirical literature on racial disparities in both pretrial and sentencing outcomes.

Some studies have suggested no direct race effects on pretrial detention when controlling for offense seriousness and prior record (Albonetti, 1989; Frazier, Bock, and Henretta, 1980; Holmes, Daudistel, and Farrell, 1987; Holmes, Hosch, Daudistel, Perez, and Graves, 1996; Nagel, 1983; Stryker, Nagel, and Hagan, 1983), whereas others have uncovered racial disparities even with these controls (Ayles and Waldfoegel, 1994; Chiricos and Bales, 1991; Demuth, 2003; Demuth and Steffensmeier, 2004; Katz and Spohn, 1995; Kutateladze, Andiloro, Johnson, et al., 2014; LaFree, 1985; Lizotte, 1978; Patterson and Lynch, 1991; Spohn, 2009; Sutton, 2013). Race  $\times$  sex interaction effects have been found, in which White females have the lowest odds of pretrial detention (Bickle and Peterson, 1991; Demuth and Steffensmeier, 2004; Patterson and Lynch, 1991). The first set of null race effects suggests that race might only be spuriously linked to either bond amounts or pretrial detention if these are dictated primarily by offense seriousness and prior record. On the other hand, the second set of significant race effects implies that bond amounts and the odds of pretrial detention could be higher for minorities regardless of these other factors.

The analysis presented in this study also focuses on the magnitude of charge reductions between indictment and conviction for all convicted defendants. As mentioned, Shermer and Johnson (2010) found no significant racial or ethnic disparities in charge reductions after initial charging in U.S. federal courts. By contrast, Wooldredge and Griffin (2005) uncovered *higher* odds of reduced charges among Blacks than for Whites who were processed through Ohio Common Pleas Courts. Consistent with this finding, Holmes et al. (1987) found that Blacks in Delaware County, Pennsylvania, were more likely than Whites to obtain charge reductions via guilty pleas, and they speculated that this might be a result of the initial overcharging of minorities. It is possible, however, that prosecutors were more anxious to secure their convictions and so they offered more attractive plea "bargains" in order to do so. The strategy of securing convictions with more attractive charge and sentence reductions was discussed by Casper (1972), Eisenstein and Jacob (1977), and Heumann

(1978), among others. In contrast, Moore and Miethe (1986) found that Blacks charged with more serious offenses were less likely than Whites to receive negotiated sentences in Minnesota, suggesting that minorities might not have been offered the same types of bargains as Whites.

Whether a convicted defendant is sent to prison and for how long have been the most common foci of related research. As mentioned, judges at sentencing hearings and prosecutors during plea negotiations might recommend prison sentences as well as longer prison terms for minorities than for Whites if minorities are perceived as greater “threats” to the community (e.g., Hawkins, 1981). In contrast, from symbolic interactionist perspectives such as “focal concerns” (Steffensmeier et al., 1998) and “uncertainty avoidance” (Albonetti, 1987, 1991), images of more dangerous offenders might be shaped by a defendant’s race in conjunction with other extralegal factors such as sex or age (e.g., Johnson, 2005, 2006; Johnson et al., 2008; Koons-Witt, 2002; Spohn and Holleran, 2000; Steen, Engen, and Gainey, 2005; Steffensmeier et al., 1998; Ulmer and Johnson, 2004). As reviewed by Kutateladze, Andiloro, Johnson, et al. (2014), Stolzenberg et al. (2013), and Ulmer (2012), there is much evidence to suggest that convicted Black defendants are more likely than convicted Whites to be sent to prison (although exceptions to this general theme have been found). Moreover, racial disparities in imprisonment have been found to be particularly dramatic for young Black men (Kutateladze, Andiloro, Johnson, et al., 2014). Evidence on sentence length, however, seems to be much more mixed.

## Predictions

Following the preceding discussion, we hypothesized that Black defendants *in general* and young Black males in particular would receive worse dispositions and sentences than White defendants (i.e., higher bond amounts for bond-eligible suspects, higher odds of pretrial detention for those referred to the county prosecutors’ office [CPO], less dramatic charge reductions between indictment and conviction among those convicted, higher odds of a nonsuspended prison sentence for those convicted, and longer prison sentences for those sent to prison). Pertinent to our focus on cumulative disadvantage, we also hypothesized that Black defendants *in general* and young Black males in particular would face higher odds of pretrial detention because of (a) higher bond amounts, (b) lower odds of retaining hired attorneys, and (c) higher odds of having previously served time in prison. These demographic groups should also face higher odds of incarceration in prison and longer prison terms because of their higher odds of (a) pretrial detention, (b) having previously served time in prison, and (c) not hiring private attorneys.

## Methods

### *Sample and Data*

The current study focuses on a jurisdiction in the United States ranking among the largest 50 cities in the country as of 2013, with Blacks constituting one third of the metropolitan area



(United States Bureau of the Census, 2014). In 2009, the county prosecutors' office (CPO) contacted us to ask about the possibility of an evaluation of the impact of defendants' race on felony case processing in the county.<sup>2</sup> We had access to a public database that provided a portion of the information needed for the study, and we were granted access to the county prosecutor's nonpublic database for the remaining information. Data collection began in fall 2010.

Our goal was to draw a simple random sample of all persons referred to the CPO for felony offenses during 2009. The sampling frame included all 18,407 persons referred for criminal review in 2009. An electronic frame was provided by the CPO and ordered by date of arrest. The available resources permitted data collection for 4,000 referrals (22% sample). A review of court statistics for previous years revealed that approximately 35% of all persons convicted on felony charges in the county consisted of drug offenders. So as not to overwhelm the analyses with these types of cases, we oversampled by 1,000 referrals and skipped every other drug suspect in the sample.

A simple random sample of 5,000 persons was selected. Skipping every other drug suspect generated slightly more than 4,000 persons. Approximately 150 of these referrals either had missing data or appeared more than once in the sampling frame. Removing these incomplete and redundant cases resulted in 3,852 available referrals (21% sample).<sup>3</sup> The study described in this article focused only on Black non-Latinos and White Anglos who were ultimately *indicted* on felony charges, permitting analyses of 3,459 felony defendants (after excluding nonindictments on felony charges as well as other race/ethnic groups, including 1% Latinos).

### *Measures*

All measures for the analysis are described in Table 1 for Black non-Latinos and White Anglos separately ("Black" and "White" hereafter). Some of the measures listed in the table are relatively straightforward, whereas others need more explanation. Regarding the outcomes for the analysis, the statistical analyses had two parts (described next) involving differences in sample sizes. The first part examined each outcome as a discrete decision point, and so each of the separate models was estimated only for applicable cases (e.g., convicted defendants only for the analysis of imprisonment). The second part of the analysis involved examining race effects across decision points in a path model, and so this part included all indicted felony defendants to determine how race influences whether an *indicted* felon

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2. The CPO's interest in the study stemmed from public criticism that Black suspects were treated more severely than White suspects by the police and courts.
  3. This pool of referrals was determined to be representative of the 2009 felony referrals based on the percentage of those processed by each prosecutor, the percentage of those processed by each judge, and the percentage of referrals from each police precinct or agency in the county (sample vs. population distributions available upon request).

**T A B L E 1**

**Description of Samples**

Measures	Black, non-Latino			White Anglo		
	Mean	SD	Valid N	Mean	SD	Valid N
<b>Outcomes</b>						
Bond amount (log <sub>e</sub> ) <sup>a</sup>	8.82	1.51	2,376 (bond eligible)	8.55	1.41	989
Pretrial detention	0.31	0.46	2,443 (indicted)	0.26	0.44	1,016
Charge reductions (square root) <sup>a</sup>	2.54	2.30	2,067 (convicted)	2.52	2.54	867
Prison (nonsuspended)	0.43	0.50	2,067 (convicted)	0.35	0.48	867
Prison term in months (log <sub>e</sub> ) <sup>a</sup>	1.42	0.52	890 (sent to prison)	1.45	0.52	303
<b>Demographics</b>						
Male	0.83	0.38	2,443	0.78	0.42	1,016
Age 18–29	0.51	0.50		0.45	0.50	
Age 30–49	0.39	0.49		0.44	0.50	
Age 50+ (reference)	0.10	0.30		0.11	0.31	
<b>Legal Factors</b>						
Most serious charge indicted			2,443			1,016
Felony 1	0.20	0.40		0.14	0.35	
Felony 2	0.16	0.36		0.18	0.38	
Felony 3	0.17	0.37		0.15	0.35	
Felony 4	0.15	0.35		0.16	0.37	
Felony 5 (reference)	0.32	0.47		0.37	0.48	
Total indicted charges (square root) <sup>a</sup>	1.65	0.56	2,443	1.64	0.57	1,016
<b>Specifications in indictment</b>						
Firearms	0.08	0.27	2,443	0.03	0.16	1,016
Drugs	0.00	0.02		0.00	0.00	
Forfeitures	0.20	0.40		0.13	0.33	
Repeat violent offender	0.03	0.16		0.02	0.13	
Repeat sex offender	0.01	0.12		0.02	0.13	
<b>Offenses in indictment</b>						
Murder	0.00	0.05	2,443	0.00	0.05	1,016
Rape	0.01	0.09		0.00	0.06	
Robbery	0.03	0.17		0.01	0.12	
Burglary	0.01	0.08		0.00	0.07	
Theft	0.07	0.25		0.12	0.33	
Fraud	0.03	0.18		0.04	0.19	
Drug	0.12	0.33		0.08	0.27	
Assault	0.04	0.20		0.04	0.19	
Kidnapping	0.04	0.19		0.03	0.18	
Arson	0.00	0.03		0.00	0.00	
Vandalism	0.01	0.08		0.01	0.08	
Breaking and entering	0.00	0.06		0.00	0.06	
Domestic violence or nonsupport	0.03	0.16		0.03	0.16	
Obstruction of justice	0.02	0.12		0.01	0.12	
Weapons under disability	0.02	0.15		0.00	0.05	

(Continued)

**T A B L E 1**

**Continued**

Measures	Black, non-Latino			White Anglo		
	Mean	SD	Valid N	Mean	SD	Valid N
Carrying concealed weapon	0.01	0.10		0.01	0.08	
Most serious conviction			2,067			867
Felony 1	0.07	0.26		0.04	0.19	
Felony 2	0.08	0.28		0.07	0.25	
Felony 3	0.20	0.40		0.18	0.39	
Felony 4	0.22	0.41		0.25	0.43	
Felony 5 (reference)	0.43	0.50		0.45	0.50	
Total convicted charges (square root) <sup>a</sup>	1.31	0.29	2,067	1.33	0.30	867
Specifications convicted on			2,067			867
Firearms	0.06	0.23		0.01	0.11	
Drugs	0.00	0.03		0.00	0.00	
Forfeitures	0.23	0.42		0.13	0.34	
Repeat violent offender	0.01	0.09		0.00	0.00	
Repeat sex offender	0.00	0.06		0.00	0.00	
Offenses convicted on			2,067			867
Murder	0.02	0.12		0.01	0.07	
Rape	0.01	0.08		0.01	0.09	
Robbery	0.08	0.26		0.02	0.15	
Burglary	0.07	0.25		0.09	0.29	
Theft	0.13	0.34		0.21	0.41	
Fraud	0.05	0.22		0.05	0.21	
Drug	0.26	0.44		0.20	0.40	
Assault	0.10	0.30		0.10	0.32	
Kidnapping	0.02	0.14		0.02	0.14	
Arson	0.00	0.07		0.00	0.06	
Vandalism	0.01	0.12		0.03	0.16	
Breaking and entering	0.03	0.18		0.03	0.18	
Domestic violence/nonsupport	0.08	0.28		0.11	0.34	
Obstruction of justice	0.07	0.25		0.04	0.21	
Weapons under disability	0.03	0.18		0.01	0.10	
Carrying concealed weapon	0.04	0.20		0.01	0.09	
Any codefendants in case	0.26	0.44	2,443	0.25	0.44	1,016
Number of victims of violence <sup>a</sup>	0.25	0.59	2,443	0.24	0.54	1,016
Victim a stranger	0.09	0.28	2,443	0.06	0.24	1,016
Number of victims of property crime <sup>a</sup>	0.38	0.65	2,443	0.45	0.67	1,016
Type of attorney			2,443			1,016
Public defender	0.28	0.45		0.24	0.43	
Assigned by court (reference)	0.57	0.50		0.51	0.50	
Hired	0.15	0.36		0.25	0.43	
Disposition mode			2,067			867
Pled guilty	0.95	0.21		0.98	0.15	

(Continued)

**T A B L E 1**

**Continued**

Measures	Black, non-Latino			White Anglo		
	Mean	SD	Valid N	Mean	SD	Valid N
Jury trial	0.03	0.16		0.01	0.11	
Bench trial (reference)	0.02	0.14		0.01	0.08	
Failure to appear in court	0.10	0.30	2,067	0.09	0.29	867
Uncooperative with prosecutors	0.02	0.15	2,067	0.02	0.15	867
Prior prison term	0.45	0.50	2,443	0.30	0.46	1,016

Note. SD = standard deviation.

<sup>a</sup>Ratio scale. Measures not marked as such are binary (0/1) scales, where cases scoring 1s reflect the variable label and cases scoring 0s do not.

ultimately ends up in prison. The following description of the outcomes and the statistics displayed in Table 1 reflect the first part of the analysis.

The outcome measure *bond amount* was examined for bond-eligible defendants only ( $N = 3,365$ ), excluding those denied release or who were released on their own recognizance. The  $\log_e$  of bond amount was taken based on the right-skewed distribution of cases on the original scale (Fox, 2008: 55).<sup>4</sup> The binary measure of *pretrial detention* was examined for all felony indictments ( $N = 3,459$ ) and compared persons who were released (bond posted or released on their own recognizance) with those detained prior to trial (no bond posted or denied release).

*Charge reductions* is a ratio scale capturing differences in charges between indictment and conviction, so it was examined for convicted defendants only ( $N = 2,934$ ). Felony and misdemeanor levels were scored 1 through 9 for every count a person was indicted on and for every count a defendant was convicted on. These values ranged from 1 (misdemeanor 4) to 9 (felony 1). These scores were summed across all counts per defendant. There were two separate scales: one for indicted charges and one for convicted charges. Subtracting the convicted sum from the indicted sum produced a difference in the magnitude of charge reductions. Although crude, the measure should have captured the more important differences in charge reductions across defendants. A potential drawback to this scale is that a defendant indicted on a very large number of less serious charges might be scored comparable with (if not higher than) a defendant charged with homicide at either indictment or conviction. Because our interest is in how these charges *changed* throughout the process, however, the actual scores at indictment versus conviction were irrelevant to an analysis

4. Removing the skew was necessary to permit the use of generalized least-squares regression.

of the *differences* in these scores. The square root of the measure was taken based on the right-skewed distribution of cases on the original scale (Fox, 2008: 55).<sup>5,6</sup>

*Prison (nonsuspended)* reflects whether a convicted defendant received a nonsuspended prison term and was examined for convicted defendants only ( $N = 2,934$ ). The reference group includes both alternatives to incarceration and jail sentences. In all, 112 convicted defendants were sent to jail, which was not sufficient to support a separate analysis of jail sentences. Analyses of this outcome measure with and without jailed offenders revealed similar findings in terms of the magnitude and significance of the population estimates, and so convicted defendants sent to jail were combined with those not sent to prison.<sup>7</sup>

*Prison term in months* applies only to individuals with nonsuspended prison sentences ( $N = 1,193$ ). The drop in the number of cases when moving from indictment to conviction (from 3,459 to 2,934 cases) is not nearly as severe as the drop between conviction and imprisonment (from 2,934 to 1,193 cases). This raises the possibility of biased regression coefficients in the analysis of sentence length as a result of potential sample selection bias, where the cross section of imprisoned offenders might differ from the cross section of all referrals in ways not controlled for in the analysis.<sup>8</sup> A Heckman adjustment for sample selection bias, or the hazard of nonimprisonment, was generated in Stata (StataCorp, College Station, TX) and is included as a predictor in the model of sentence length.<sup>9</sup>

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5. The square root was used instead of a log transformation because the skew of the original distribution was not as severe as for bond amount. Still, the transformation was necessary for meeting the assumptions of generalized least-squares regression.
  6. Piehl and Bushway (2007) provided a novel and useful way of measuring the magnitude of charge "bargaining" following plea agreements for convicted defendants. Using state court data from Washington State and Maryland, they generated state-specific regression models of expected sentence length including indicators of criminal history and crime severity, the latter reflecting charges actually convicted on. These equations were then employed to predict sentence lengths using the charges at arraignment instead of at conviction. No difference between the two values for an individual would indicate no charge bargain, whereas higher predicted sentences using the arraigned charges would reflect some charge bargaining. Moreover, the magnitude of the difference between the two values for an individual should indicate the sentence "discount" for pleading guilty. Their measure is closer than ours to how we think about the relevance of charge reductions for impacting sentencing. However, without revealing too much about the jurisdiction examined in this study, we did not adopt this measure only because of the amount of error in predicting sentence length in the regression model ( $PRE \approx .35$ ). Piehl and Bushway (2007) might have had greater correspondence between the actual and predicted sentences at conviction perhaps because of the sentencing schemes in place at the time. As such, we used the measure of charge reductions described previously.
  7. Jailed offenders were not grouped with offenders sent to prison because approximately half of the jail terms were 3 weeks or less, and 90% of these terms were less than 3 months. Prison terms typically spanned more than a year, so jailed offenders might be considered more similar to nonincarcerates in terms of sentence severity.
  8. There is also potential selection bias in any of the analyses without the full sample of referrals, but the possibility could be greatest in the analysis of sentence length because of the loss of so many cases.
  9. For a discussion of the components of the hazard function and how it adjusts for such bias, see Berk (1983). Including a Heckman adjustment can sometimes decrease the efficiency of population

The “demographic” binary measures of *age* follow Steffensmeier et al. (1998). They found a nonlinear relationship between age and sentence severity in Pennsylvania state trial courts, where the oldest group (our reference group of 50 years of age and older) was treated the least severely. Regarding “legal factors,” there were two sets of similar measures for indictments versus convictions including *most serious charge*, *total charges*, *specifications*, and particular *offenses*. The *offense* measures were selected based on whether they improved prediction beyond the *most serious charge* measures.<sup>10</sup> *Specifications* involve sentence enhancements for use of firearms, repeat drug dealing, repeat violent offending, and repeat sex offending. There is also a specification involving the forfeiture of properties involved in an offense, where forfeiture itself becomes part of the sanction after conviction.

Regarding the four *victim* measures, values of zero were assigned to all cases not meeting the labeled criterion (i.e., no victims of violence, no victim injury, victim *not* a stranger, and no victims of property crimes). This also includes all “victimless” crimes. The binary measures of *type of attorney* distinguished between public defenders, other private attorneys assigned by the court (our reference group), and hired attorneys. Also potentially relevant is whether a defendant was *uncooperative with prosecutors*, based on prosecutors’ written observations of defendants’ behaviors. Finally, of all the criminal history data at our disposal, whether a defendant had served a prison sentence was the strongest predictor of any outcome.<sup>11</sup>

Some predictors were included only in some models because they were relevant only to those decisions (*guilty plea*, *jury trial*, *failure to appear*, and *pretrial detention*). Similarly, the *indictment* measures of charges, offenses, and specifications were included in all but the sentencing models where the *conviction* measures were included instead.

### Statistical Analysis

The analysis was divided into two parts reflecting our interest in (a) main and interaction effects of a suspect’s race on treatment at each stage of case processing (bond amounts, pretrial detention, charge reductions, imprisonment, and length of imprisonment), and (b) indirect effects of a suspect’s race on sentence severity via pretrial detention, type of attorney, and histories of imprisonment (i.e., whether “cumulative disadvantages” are more pronounced for Black suspects). Because of the punitive nature of pretrial detention, the second part of

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estimates (Bushway, Johnson, and Slocum, 2007), so we explored this possibility following procedures described by Stolzenberg and Relles (1997). There is no evidence that the estimates are less efficient with the adjustment, so it was retained for the model.

10. Only the relevant offense measures are displayed in Table 1 even though many more were created and explored for inclusion. For the most serious charge measures, the jurisdiction under study follows five felony classifications ranging from F1s (most serious) to F5s (our reference category).
11. Whether a defendant had served a prison sentence was a stronger predictor than prior arrests, felony arrests, convictions, felony convictions, and the number of prior prison sentences. We explored a factor of these measures combined, based on an alpha reliability of .72, but the single measure of prior prison term was superior in prediction.

the analysis also involved estimation of indirect race effects on pretrial detention via bond amounts, type of attorney, and prior imprisonment. For both segments of the analysis, persons arrested and referred for drug offenses were weighted differently than all other cases after our decision to skip every other drug referral in the sample. Therefore, all cases were weighted inversely to their probability of inclusion in the sample, with weights normalized.

For the first part of the analysis, race-specific multivariate models (including the legal measures displayed in Table 1) were estimated to examine main and interaction effects.<sup>12</sup> All variables were grand mean centered in each model, and the main effects of race were examined by testing whether the model constants for each pair of race-specific models differed significantly for a specific outcome. Each model constant reflects the adjusted mean of the dependent variable for a particular race group (controlling for all independent variables in the model). An equality of coefficients test (Clogg, Petkova, and Haritou, 1995) revealed whether these adjusted means differed between race groups.<sup>13</sup>

The analysis of interaction effects (following our earlier discussion of possible interaction effects involving a defendant's race, sex, and age) involved estimating two additional sets of models for Black males 18 to 29 years of age versus all other suspects. Comparisons of the model constants within each respective pair of models, using the equality of coefficients  $z$  test, revealed whether case dispositions and outcomes differed significantly between these groups.

Multilevel modeling was used as a result of the nested data and the potential for correlated error among cases processed by the same prosecutor or judge. Defendants at level 1 were nested within up to 123 prosecutors at level 2 for the analyses of bond amounts, pretrial detention, and charge reductions. Defendants were nested within 34 judges for the analyses of imprisonment and sentence length.<sup>14</sup> We could not identify the municipal court judges responsible for pretrial release decisions, so we could not examine the between-judge variance in either *bond amount* or *pretrial detention*. Instead, we nested defendants within prosecutors because of the potential influence of prosecutors at these decision points. Similarly, given the role of prosecutors in plea bargaining, we also nested defendants within prosecutors for the analysis of charge reductions. Generalized least-squares regression models were estimated for *bond amount* ( $\log_e$ ), *charge reductions* (*square root*), and *prison term in months* ( $\log_e$ ). Bernoulli (binary logistic) regression models were estimated for *pretrial detention* and *prison sentence*. The software for the analysis was MPLUS 6.12 (Muthén and Muthén, 1998–2010). Level 1 PRE values cannot be computed for multilevel Bernoulli

12. Each pair of models was estimated for the applicable subsamples (3,365 bond-eligible suspects for the analysis of bond amounts; 3,459 indicted suspects for pretrial detention; 2,934 convicted defendants for charge reductions and imprisonment; 1,193 imprisoned offenders for length of imprisonment).

13. Paternoster, Brame, Mazerolle, and Piquero (1998) demonstrated the reliability of this test for identifying significant differences between maximum-likelihood regression coefficients.

14. There were 37 judges in total, but 3 were dropped from the analysis because they processed only seven cases altogether in 2009, thus, prohibiting estimation of the level 1 models for those judges.

models based on the information provided in MPLUS, so these estimates were computed using procedures described in Hox (2010: ch. 6). Estimates of variance in the Bernoulli models were derived under the assumption that the level 1 random effects conformed to a logistic distribution (Raudenbush and Bryk, 2002).<sup>15</sup>

Findings from the first stage of the analysis influenced the second stage focusing on “cumulative disadvantage.” Consistent with the absence of racial disparities in charge reductions and prison sentence length found in the first stage of the analysis (described next), we focused only on the direct and indirect race effects on pretrial detention and imprisonment. The indirect effects of both race in general (*Black*) and for young Black males in particular (*Black male 18–29*) were examined. To determine whether harsher race-based treatments at earlier decision points ultimately shape race-based treatments at later decision points, these *indirect* race effects can only be assessed with a path model that treats outcomes for the first stage of the analysis as both endogenous and lagged endogenous variables in the same model.<sup>16</sup> The results from the first stage of the analysis (capturing main and interaction race effects on each outcome separately) combined with the results from the second stage (reflecting indirect race effects across multiple stages of case processing) capture both the discrete *and* cumulative disadvantages experienced by particular race groups throughout court processing.

Toward the end of evaluating indirect race effects on pretrial detention and imprisonment, we used path modeling in MPLUS 6.12. Each set of path models included the direct and indirect race effects of interest in addition to all other statistically significant legal effects on each outcome. Including only the *significant* legal effects on each outcome served to maximize each model’s goodness of fit. Figure 1 displays the direct and indirect race effects of interest. This figure oversimplifies the full path model because of the omission of all direct effects involving legally relevant measures (available upon request).

The direct race effects depicted in Figure 1 were derived by controlling for the aforementioned legally relevant effects on each outcome. Indirect race effects represent the product of all mediating paths separating a defendant’s race from a specific outcome.<sup>17</sup> All estimates were derived with robust weighted least-squares estimation in MPLUS 6.12.

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15. The level 1 intercepts and defendant effects that varied significantly across court actors in each model were treated as random effects, with all other effects “fixed.” Treating significantly varying defendant effects as random was necessary for proper model specifications (Hox, 2010). Treating the level 1 intercepts as random allowed us to determine whether disposition and sentence severity differed significantly across prosecutors or judges.

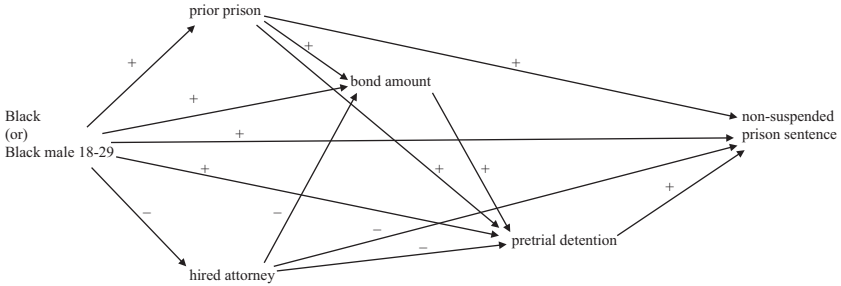
16. The cross-sectional models estimated during the first stage of the analysis cannot reveal how the influences of a defendant’s race on an earlier decision point (e.g., pretrial detention) ultimately affect race-group differences in sentence severity, separate from the direct race effects on sentencing.

17. There are several mediating effects with some of these effects appearing more than once in different “chains.” For example, the mediating effect of prior prison sentence appears in two chains linking race to pretrial detention (race to prior prison sentence to pretrial detention; race to prior prison sentence to bond amount to pretrial detention).



**FIGURE 1**

**Hypothesized Direct and Indirect Race Effects on Pretrial Detention and Imprisonment (Controlling for Legally Relevant Predictors)**



Because our interest in this segment of the analysis involved evaluating race effects across decision points rather than treating these decisions as discrete, the entire sample of indicted felony suspects was used for estimating all paths. In other words, this procedure revealed the odds of both pretrial detention and imprisonment for any indicted suspect. As such, all five felony conviction levels were included in the model of prison sentences so that the reference group would include defendants not convicted on any felony charges.

Possible mediators for the analysis of pretrial detention included bond amounts, hired attorneys, and histories of imprisonment, whereas mediators for the analysis of imprisonment included pretrial detention, hired attorneys, and histories of imprisonment.<sup>18</sup> These analyses were pooled across race groups so that “Black” and “Black males aged 18 to 29” could be included as exogenous variables in each model. The reference category for “Black” includes White Anglos, and the reference for “Black males aged 18 to 29” includes all other Blacks and all White Anglos.<sup>19</sup>

A potential limitation of our path model is that we did not use multilevel modeling because of the problems we encountered in achieving good model fit when cases were nested within prosecutors versus judges (which could be an artifact of our particular data). Future

18. The total effects of a defendant’s race on pretrial detention and imprisonment were each partitioned into direct and indirect effects via the mediators noted previously. A “total effect” usually implies a zero-order relationship between two variables, but the term is used here to reflect the sum of direct and indirect effects of a defendant’s race controlling for all of the legally relevant factors included in the models estimated for the first part of our analysis. This necessarily ignores other possible indirect effects via other measures included in the model, but that might be a subject for future research with a much larger sample.

19. Aside from the rationale given previously for excluding sentence length from this segment of the analysis, estimating models of sentence length using all indicted felony defendants might have been problematic because of an extremely skewed distribution resulting from less than half of the sample being sent to prison.

research with more defendants per prosecutor or judge will allow comparisons of parameter estimates derived with and without the multilevel component.<sup>20</sup>

## Results and Discussion

### *Main and Interaction Effects of a Defendant's Race on Pretrial Dispositions and Case Outcomes*

Addressing the first part of our analysis treating pretrial dispositions and case outcomes as discrete decision points, Table 2 displays the race-specific models of the two pretrial dispositions examined: *bond amount* ( $\log_e$ ) and *pretrial detention*.<sup>21</sup>

Regarding the models of pretrial dispositions, significant variance in bond amounts for both race groups fell across prosecutors in this court (24% for Blacks and 18% for Whites;  $p < .01$ ), whereas significant between-prosecutor variance in pretrial detention odds existed only for Black suspects ( $p < .01$ ). Applying the aforementioned equality of coefficients test ("z test" hereafter) to the difference between model intercepts within each pair of models revealed whether each outcome differed significantly between race groups (i.e., whether the main race effects were statistically significant). These tests revealed no significant differences in bond amounts between Black and White suspects ( $p > .05$ ) but significantly higher odds of pretrial detention for Blacks ( $p < .01$ ), even when controlling for bond amounts. The lack of significant race-group differences in bond amounts provides a hint that bond amounts *alone* might not mediate the significant race effect on pretrial detention. Also worth noting is that the z test revealed no significant race-group differences in the effect of bond amounts on pretrial detention. For both groups, the bond amount was the strongest (but not the sole) determinant of whether a suspect obtained pretrial release. In other words, prohibitive bond amounts were the primary reason for not obtaining pretrial release in this particular jurisdiction.

Separate sets of models were also estimated for Black males aged 18 to 29 versus all other suspects in order to assess the race  $\times$  sex  $\times$  age interaction effects discussed previously.

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20. Goodness of model fit was assessed with the root mean square error of approximation (RMSEA) and the comparative fit index (CFI). Aside from Muthén and Muthén's (2010) warning about interpreting the chi-square fit statistics for WLSM models, Byrne (2012) has provided compelling arguments for why the RMSEA and CFI are preferable to the more traditional goodness-of-fit chi square. Among the problems with the goodness-of-fit chi square is that it can be significant even when the hypothesized model is a good fit. For example, compare a baseline model with 120 degrees of freedom and a goodness-of-fit chi square equal to 1,703 to a "full" model with 98 degrees of freedom and a goodness-of-fit chi square equal to 159. Both are statistically significant, and yet the much tighter correspondence between chi square = 159 and  $df = 98$  suggests a fairly good fit to the data. Moreover, this corresponds to a RMSEA of roughly .045, which falls under the threshold of .05 and indicates a good fit. We have faced the same situation with these models.

21. The absence of certain independent variables in three of the four models is a result of overly limited dispersions of cases on these variables for particular race groups (i.e., inclusion of these measures was either not possible because they were constants for certain groups or their inclusion resulted in highly inflated parameter estimates and standard errors).

**T A B L E 2**

**Pretrial Dispositions**

Measures	Bond Amount ( $\log_e$ ) <sup>a</sup>		Pretrial Detention <sup>b</sup>	
	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )
Intercept	8.82 (0.03)	8.65 (0.04)	-1.15 (0.07)	-1.48 (0.10)
Demographics				
Male	0.53** (0.07)	0.43** (0.10)	0.41* (0.20)	0.50* (0.23)
Age 18–29	0.16* (0.08)	-0.04 (0.12)	0.21 (0.22)	0.06 (0.35)
Age 30–49	0.11 (0.08)	0.06 (0.12)	-0.00 (0.21)	0.52 (0.34)
Legal Factors				
Most serious charge indicted				
Felony 1	1.53** (0.09)	1.33** (0.17)	0.10 (0.23)	-0.36 (0.35)
Felony 2	0.78** (0.08)	0.78** (0.10)	-0.15 (0.18)	-0.17 (0.29)
Felony 3	0.58** (0.07)	0.67** (0.11)	-0.27 (0.16)	-0.19 (0.28)
Felony 4	0.22** (0.07)	0.54** (0.11)	-0.39* (0.17)	-0.46 (0.32)
Total indicted charges (square root)	0.16** (0.06)	0.40** (0.09)	0.17 (0.15)	0.20 (0.23)
Specifications in indictment				
Firearms	0.58** (0.09)	0.66* (0.31)	0.05 (0.20)	-1.09* (0.54)
Drugs	2.24* (1.12)	—	—	—
Forfeitures	-0.23** (0.07)	0.16 (0.13)	-0.47** (0.17)	-0.37 (0.36)
Repeat violent offender	0.85** (0.15)	0.50* (0.25)	0.17 (0.33)	2.24* (1.09)
Repeat sex offender	0.08 (0.23)	0.42 (0.36)	0.38 (0.38)	-0.70 (0.70)
Offenses in indictment				
Murder	2.13** (0.51)	1.85 (1.31)	—	-1.73 (1.03)
Rape	0.67* (0.29)	-0.62* (0.28)	-0.25 (0.73)	0.29 (2.26)
Robbery	0.35* (0.16)	0.26 (0.26)	-0.03 (0.34)	-0.13 (0.69)
Burglary	0.19 (0.29)	0.001 (0.58)	-0.30 (0.49)	1.42 (0.94)

(Continued)

**TABLE 2**

Measures	Continued			
	Bond Amount ( $\log_e$ ) <sup>a</sup>		Pretrial Detention <sup>b</sup>	
	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )
Theft	-0.17 (0.10)	-0.07 (0.12)	0.24 (0.22)	-0.19 (0.29)
Fraud	-0.62** (0.14)	-1.02** (0.11)	-0.06 (0.31)	0.58 (0.49)
Drug	-0.11* (0.09)	-0.30 (0.16)	-0.69** (0.23)	-0.39 (0.42)
Assault	-0.03 (0.13)	0.13 (0.25)	-0.04 (0.33)	0.07 (0.50)
Kidnapping	0.06 (0.14)	0.73** (0.28)	0.21 (0.31)	-0.50 (0.57)
Arson	0.55 (1.12)	—	—	—
Vandalism	0.25 (0.30)	1.33** (0.41)	0.34 (0.55)	-1.32 (0.85)
Breaking and entering	-0.78 (0.43)	-0.92** (0.30)	0.59 (0.83)	1.59 (1.03)
Domestic violence or nonsupport	0.36* (0.16)	0.48 (0.31)	0.33 (0.33)	0.28 (0.56)
Obstruction of justice	0.04 (0.20)	0.49 (0.35)	0.06 (0.49)	-0.57 (0.72)
Weapons under disability	0.21 (0.16)	0.43 (0.55)	-0.12 (0.38)	—
Carrying concealed weapon	0.39 (0.23)	-0.64* (0.31)	0.38 (0.66)	—
Any codefendants in case	0.09 (0.06)	-0.10 (0.07)	-0.16 (0.13)	0.25 (0.21)
Number of victims of violence	0.14** (0.05)	0.05 (0.06)	-0.14 (0.09)	-0.03 (0.23)
Victim injured	0.51** (0.08)	0.32* (0.16)	-0.73** (0.18)	-0.70* (0.33)
Victim a stranger	0.24** (0.09)	0.05 (0.16)	0.58** (0.20)	0.23 (0.44)
Number of victims of property crime	-0.06 (0.04)	-0.13* (0.06)	-0.08 (0.08)	-0.03 (0.15)
Type of attorney				
Public defender	-0.08 (0.05)	-0.31** (0.09)	0.10 (0.12)	-0.37 (0.21)
Hired	0.003 (0.07)	-0.30 (0.08)	-1.88** (0.22)	-2.01** (0.32)
Uncooperative with prosecutors	0.01 (0.16)	-0.29 (0.24)	-0.002 (0.36)	-0.92 (0.74)

(Continued)

**TABLE 2**

**Continued**

Measures	Bond Amount (log <sub>e</sub> ) <sup>a</sup>		Pretrial Detention <sup>b</sup>	
	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )
Prior prison term	0.28** (0.05)	0.37** (0.09)	0.85** (0.11)	0.68** (0.17)
Bond amount (log <sub>e</sub> )	—	—	0.90** (0.07)	1.00** (0.09)
Level 2 Variance (null model)	0.63**	0.38**	0.12**	0.02
<i>df</i> at Level 2 (null model)	123	113	123	113
Intraclass Correlation	0.24	0.18	—	—
Prop. Level 1 Variance Explained	0.39	0.34	0.44	0.45

Notes. *df* = degrees of freedom; *SE* = standard error.

<sup>a</sup>Generalized least-squares regression model for the ratio outcome.

<sup>b</sup>Logistic regression model for the binary outcome.

\*  $p < .05$ . \*\*  $p < .01$ .

The *z* tests of differences in model intercepts revealed significant group differences in *both* bond amounts and pretrial detention ( $p < .001$ ), in contrast to the nonsignificant difference in bond amounts between Black and White suspects in general. Moreover, the difference in the odds of pretrial detention was even more dramatic for young Black men compared with the difference between Blacks and Whites in general ( $z = 5.0$  for Black males aged 18 to 29 vs.  $z = 2.7$  for Blacks in general). This finding is consistent with Demuth (2003), who also found higher odds of pretrial detention for young Black men.

To provide a more substantive interpretation of these race-group differences, after controlling for the measures included in the pretrial models, Black males aged 18 to 29 were assigned bond amounts that, on average, were roughly \$3,500 higher than the amounts assigned to all other suspects. Also, the probability of being detained prior to trial was 0.68 for young Black men versus 0.22 for all other suspects. These figures were 0.24 and 0.18 for Blacks and Whites, respectively.

Also relevant to the idea of cumulative disadvantage are the significant inverse effects of hired attorney and the significant positive effects of prior prison term and bond amounts on pretrial detention (for both race groups). A history of imprisonment, a higher bond amount, and the absence of a hired attorney each served as a disadvantage at this decision point and, as such, could have ultimately contributed to additional disadvantages at the sentencing stage. Given that bond amounts were controlled in the analysis of pretrial detention, the significant inverse effect of hired attorney might not merely reflect an economic advantage for suspects with hired attorneys at pretrial release. Rather than simply reflecting economic inequities among suspects at the preliminary hearing, where those who can afford to hire attorneys can also afford to post bond, these findings suggest that hired attorneys are more

capable of securing their clients' release for noneconomic reasons. None of these three effects were significantly stronger for one race group versus the other ( $p > .05$ ), nor did any of these effects differ significantly in magnitude between Black males aged 18 to 29 and the rest of the sample.

The race-specific models of case outcomes for convicted defendants (charge reductions, nonsuspended prison sentences, and the length of nonsuspended prison sentences) are displayed in Table 3.

The cases were nested within prosecutors at level 2 for the analysis of the magnitude of reduced charges between indictment and conviction, and significant variance in charge reductions fell across prosecutors for both race groups ( $p < .01$ ). This finding could reflect differences between prosecutors in the magnitude of plea offers in this jurisdiction. Regardless, the  $z$  test of the difference in model intercepts revealed no significant differences in charge reductions, on average, between Blacks and Whites. And even though both prior prison terms and pretrial detention were significantly linked to less dramatic charge reductions for Blacks but not for Whites, the magnitude of these effects did not differ significantly between race groups. (Differences in statistical significance are likely a result of the larger sample of 2,067 convicted Blacks relative to 867 convicted Whites.) This theme also applies to the comparison of Black males aged 18 to 29 versus all other convicted defendants, with no significant differences in charge reductions between these two groups in addition to no significant group differences in the magnitude of these other effects on charge reductions. Although these findings hint at some accrued disadvantages in plea agreements stemming from the inverse effects of imprisonment histories and pretrial detention on charge reductions, these disadvantages seem to operate uniformly for the demographic groups examined.

The models of nonsuspended prison sentences in Table 3 also produced no significant race-group difference in model intercepts, indicating no significant difference in the odds of a nonsuspended prison sentence between convicted Black and White defendants in general. Pretrial detention is a strong and powerful predictor of prison sentences for both race groups; however, Black detainees were nearly five times more likely than released Black suspects to be sent to prison, whereas White detainees were nearly four times more likely than released Whites to be sent to prison (each reflecting the change in the odds ratio, computed as  $e^b$ ). Despite the raw difference in these odds between the two groups, the effect of pretrial detention on imprisonment did not differ significantly between Blacks and Whites ( $p > .05$ ). Even so, given the significant main effect of a suspect's race on pretrial detention, this raises the possibility of a greater cumulative disadvantage for Black defendants than for White defendants at the sentencing stage despite the nonsignificant *direct* effect of race on the odds of imprisonment.

Having served a prison term was also significantly related to the odds of going to prison for both race groups although the effect was significantly stronger for convicted Whites ( $p < .05$ ). A significant race effect on nonsuspended prison sentences still could emerge

**TABLE 3**

**Case Outcomes**

Measures	Charge Reductions (square root) <sup>a</sup>		Nonsuspended Prison Sentence <sup>b</sup>		Prison Sentence Length (log <sub>e</sub> ) <sup>a</sup>	
	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )
Intercept	2.35 (0.05)	2.23 (0.08)	-0.67 (0.10)	-1.14 (0.21)	1.21 (0.11)	0.89 (0.21)
Demographics						
Male	-0.12 (0.09)	0.06 (0.11)	0.46** (0.18)	0.70** (0.25)	-0.02 (0.06)	-0.05 (0.10)
Age 18–29	-0.001 (0.11)	-0.40* (0.16)	0.74** (0.20)	0.47 (0.34)	-0.05 (0.06)	-0.10 (0.11)
Age 30–49	0.12 (0.11)	-0.51* (0.14)	0.74** (0.21)	0.56 (0.33)	-0.03 (0.06)	-0.10 (0.11)
Legal Factors						
Most serious charge						
Felony 1	0.90** (0.13)	1.19** (0.24)	3.09** (0.46)	3.09** (0.57)	0.50** (0.07)	0.75** (0.14)
Felony 2	0.82** (0.11)	0.72** (0.14)	1.76** (0.26)	2.30** (0.40)	0.37** (0.06)	0.39** (0.12)
Felony 3	0.56** (0.10)	0.39* (0.17)	1.65** (0.18)	1.88** (0.28)	0.12* (0.05)	0.17 (0.10)
Felony 4	0.17 (0.10)	0.18 (0.12)	0.82** (0.15)	0.96** (0.23)	0.30** (0.05)	0.24** (0.09)
Total Charges (square root)	2.81** (0.08)	3.02** (0.21)	0.58* (0.23)	0.50* (0.21)	0.14* (0.05)	0.02 (0.10)
Specifications						
Firearms	-0.01 (0.13)	-0.19 (0.28)	1.78** (0.47)	—	0.23** (0.05)	—
Drugs	—	—	—	—	—	—
Forfeitures	0.04 (0.09)	0.24 (0.18)	0.21 (0.17)	0.08 (0.29)	0.05 (0.04)	-0.004 (0.10)
Repeat violent offender	-0.37 (0.21)	0.57 (0.40)	1.05 (1.04)	—	0.10 (0.11)	—
Repeat sex offender	1.09** (0.32)	5.31** (1.01)	—	—	—	—
Offenses						
Murder	-0.71 (0.73)	-1.33 (2.32)	-0.07 (0.84)	—	0.35** (0.10)	—
Rape	1.92** (0.40)	-1.39 (1.65)	-0.19 (1.16)	—	0.40** (0.13)	—
Robbery	0.86** (0.22)	-1.71** (0.58)	0.26 (0.30)	-0.12 (0.63)	0.001 (0.06)	-0.20 (0.18)
Burglary	0.74 (0.39)	0.98 (0.76)	0.10 (0.28)	0.06 (0.38)	0.01 (0.06)	-0.15 (0.10)

(Continued)

**TABLE 3**

**Continued**

Measures	Charge Reductions (square root) <sup>a</sup>		Nonsuspended Prison Sentence <sup>b</sup>		Prison Sentence Length (log <sub>e</sub> ) <sup>a</sup>	
	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )	Black <i>b</i> (SE <sub><i>b</i></sub> )	White <i>b</i> (SE <sub><i>b</i></sub> )
Theft	-0.15 (0.14)	-0.06 (0.18)	0.29 (0.24)	0.55 (0.33)	0.05 (0.07)	-0.03 (0.11)
Fraud	0.45* (0.18)	-0.01 (0.50)	0.17 (0.32)	-0.17 (0.49)	0.14 (0.10)	-0.18 (0.16)
Drug	-0.05 (0.11)	-0.24 (0.22)	0.61** (0.20)	0.34 (0.31)	-0.05 (0.05)	-0.23* (0.10)
Assault	-0.06 (0.18)	-0.22 (0.30)	-0.03 (0.25)	0.18 (0.37)	-0.04 (0.06)	-0.04 (0.10)
Kidnapping	0.51* (0.20)	-0.02 (0.40)	-0.06 (0.54)	0.77 (0.74)	-0.06 (0.09)	-0.19 (0.15)
Arson	-0.74 (1.43)	—	-0.98 (1.01)	—	-0.23 (0.20)	—
Vandalism	-0.55 (0.41)	-0.77 (0.43)	0.59 (0.46)	0.97 (0.51)	-0.24 (0.11)	0.05 (0.15)
Breaking and entering	0.13 (0.55)	-0.70** (0.25)	0.98** (0.35)	0.36 (0.54)	-0.06 (0.09)	-0.07 (0.18)
Domestic violence or nonsupport	-0.49* (0.21)	-0.63** (0.23)	-0.23 (0.24)	-0.47 (0.33)	-0.03 (0.07)	-0.12 (0.10)
Obstruction of justice	-0.59* (0.27)	0.24 (0.33)	-0.31 (0.24)	0.23 (0.43)	-0.02 (0.06)	-0.07 (0.11)
Weapons under disability	-0.24 (0.22)	-0.18 (0.78)	0.55 (0.38)	-1.37 (0.87)	-0.07 (0.07)	0.11 (0.28)
Carrying concealed weapon	-0.11 (0.30)	-0.18 (0.34)	0.04 (0.32)	1.66 (1.03)	-0.05 (0.09)	0.04 (0.28)
Any Codefendants in Case	0.15* (0.07)	0.28* (0.14)	0.09 (0.14)	-0.04 (0.22)	0.05 (0.04)	-0.09 (0.07)
# Victims of Violence	0.35** (0.06)	0.05 (0.12)	-0.06 (0.13)	-0.19 (0.21)	0.03 (0.03)	0.06 (0.06)
Victim Injured	-0.14 (0.11)	0.05 (0.23)	-0.04 (0.23)	0.16 (0.34)	0.02 (0.05)	-0.12 (0.09)
Victim a Stranger	-0.07 (0.13)	-0.08 (0.28)	-0.08 (0.26)	-0.40 (0.43)	-0.01 (0.05)	-0.10 (0.13)
# Victims of Property Crime	-0.06 (0.05)	-0.004 (0.10)	0.16 (0.11)	-0.03 (0.18)	-0.02 (0.02)	-0.07 (0.06)
Type of Attorney						
Public defender	0.02 (0.07)	-0.20 (0.13)	0.02 (0.13)	-0.20 (0.22)	-0.04 (0.03)	-0.03 (0.07)
Hired	-0.007 (0.09)	-0.06 (0.15)	0.10 (0.18)	0.01 (0.24)	-0.05 (0.04)	-0.10 (0.08)

(Continued)



**TABLE 3**

**Continued**

Measures	Charge Reductions (square root) <sup>a</sup>		Nonsuspended Prison Sentence <sup>b</sup>		Prison Sentence Length (log <sub>e</sub> ) <sup>a</sup>	
	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )	Black <i>b</i> ( <i>SE<sub>b</sub></i> )	White <i>b</i> ( <i>SE<sub>b</sub></i> )
Uncooperative with Prosecutors	-0.01 (0.27)	0.44 (0.69)	0.63 (0.52)	-0.38 (1.01)	-0.01 (0.11)	-0.05 (0.26)
Prior Prison Term	-0.14* (0.07)	-0.01 (0.10)	0.78** (0.12)	1.30** (0.21)	0.004 (0.03)	-0.11 (0.07)
Guilty Plea	0.72** (0.21)	1.14* (0.52)	0.62 (0.41)	0.93 (1.09)	-0.25 (0.14)	-0.23 (0.43)
Jury Trial	-0.27 (0.30)	-0.36 (0.78)	1.36* (0.63)	3.17** (1.60)	0.09 (0.15)	-0.02 (0.46)
Failure to Appear	—	—	0.76** (0.17)	0.26 (0.31)	0.07 (0.05)	0.05 (0.11)
Pretrial Detention	-0.30** (0.08)	-0.17 (0.13)	1.64** (0.14)	1.33** (0.21)	-0.04 (0.04)	0.10 (0.08)
Hazard of Non-imprisonment	—	—	—	—	-0.02* (0.01)	-0.07* (0.04)
Level 2 Variance (null model)	0.85**	2.09**	0.04*	0.08	0.01**	0.01
<i>df</i> at Level 2 (null model)	119	107	34	33	33	32
Intraclass Correlation	0.15	0.28	—	—	0.05	0.04
Prop. Level 1 Variance Explained	0.58	0.58	0.46	0.45	0.33	0.25

Notes. *df* = degrees of freedom; *SE* = standard error.

<sup>a</sup>Generalized least-squares regression model for the ratio outcome.

<sup>b</sup>Logistic regression model for the binary outcome.

\*  $p < .05$ . \*\*  $p < .01$ .

via the cumulative disadvantage of incarceration in prison, however, if Blacks are more likely to carry histories of imprisonment. On the other hand, type of attorney was not significantly linked to prison sentences in the multivariate models, suggesting a potentially weak mediating effect.

As opposed to the lack of a general race effect on the odds of imprisonment, Black males aged 18 to 29 were significantly more likely to go to prison after conviction relative to all other convicted defendants ( $p < .01$ ). The odds of going to prison were 0.40 for young Black men compared with 0.28 for the rest of the sample, even after controlling for the legally relevant factors in these models. The effects of pretrial detention, hired attorneys, and prior prison terms did not differ significantly in magnitude between young Black men and all other convicted defendants, however. The findings for pretrial detention and hired attorneys are consistent with the general race-based effects noted previously, but the absence of a significantly different effect of prior imprisonment differs from a stronger effect for Whites than for Blacks ( $p < .05$ ).

In contrast to the analyses of convicted defendants for both charge reductions and prison sentences, the models of prison sentence length in Table 3 were estimated with the subsamples of convicted defendants sent to prison. The  $z$  test of the difference in model intercepts revealed no significant difference in prison sentence length between Blacks and Whites in general. Pretrial detention, hired attorney, and prior imprisonment were not significant predictors for either race group, with no significant group differences in the magnitude of these particular estimates. This last set of findings is important because they counter the idea that these factors would generate even greater cumulative disadvantages for imprisoned offenders. Compared with the findings for imprisonment, it seems that the cumulative disadvantages of pretrial detention, type of attorney, and history of imprisonment might be applicable to the in/out decision but not to the quantity of incarceration.

The theme of no general race-group differences in the length of imprisonment and the magnitude of related interaction effects also applies to the comparison of imprisoned Black males aged 18 to 29 with all other imprisoned offenders. Compared with the four other sets of models examined, prison sentence length decisions seem to be least sensitive to differences in a defendant's race as well as to the general effects of cumulative disadvantages resulting from pretrial detention, the absence of hired attorneys, and histories of imprisonment.

The findings from the first part of our analysis revealed a general race effect on the odds of pretrial detention only and yet significant racial disparities in bond amounts, pretrial detention, and prison sentences when a defendant's race was considered in conjunction with sex and age (i.e., Black males aged 18 to 29 experienced more severe disadvantages in pretrial dispositions and sentencing than the rest of the sample). Despite these particular race-group differences, results also suggested that cumulative disadvantages might accrue equally to both Blacks and Whites in terms of how bond amounts significantly impact the odds of pretrial detention, and in terms of how pretrial detention and a history of imprisonment each influence the magnitude of charge reductions as well as the odds of imprisonment after conviction. However, this segment of our analysis could not capture possible race-group differences in cumulative disadvantages that might have existed because of *indirect* race effects on dispositions and outcomes via bond amounts, pretrial detention, the absence of hired attorneys, and histories of imprisonment. These effects were the focus of the second part of our analysis.

### *Assessing Race Differences in Cumulative Disadvantage*

The second part of our analysis focused on the indirect effects of a defendant's race on the odds of pretrial detention and on the odds of a nonsuspended prison sentence (depicted in Figure 1). The entire sample of indicted felony suspects was used for estimating all paths, as described previously, revealing the odds of both pretrial detention and imprisonment for any indicted suspect.

Bond amount reflects a cumulative disadvantage vis-à-vis pretrial detention, where higher amounts might culminate to generate more punishment (detention) for those who

**TABLE 4**

**Total, Direct, and Indirect Effects of a Defendant’s Race on Pretrial Detention and Sentence Severity**

Effects	Pretrial Detention				Nonsuspended Prison Sentence			
	Black		Black Male, Aged 18–29		Black		Black Male, Aged 18–29	
	<i>b</i>	<i>SE<sub>b</sub></i>	<i>b</i>	<i>SE<sub>b</sub></i>	<i>b</i>	<i>SE<sub>b</sub></i>	<i>b</i>	<i>SE<sub>b</sub></i>
Total Effect	0.78**	0.08	0.88**	0.08	0.48**	0.08	0.65**	0.08
Direct Effect	0.22*	0.09	0.39**	0.11	0.13	0.09	0.22*	0.11
Total Indirect	0.56**	0.08	0.49**	0.09	0.35**	0.04	0.43**	0.07
Indirect Via								
Bond amount	–0.01	0.02	0.06*	0.03	—	—	—	—
Hired attorney	0.36**	0.06	0.20**	0.07	–0.13**	0.03	–0.07*	0.03
Hired attorney and bond amount	0.02*	0.01	0.01*	0.01	—	—	—	—
Prior prison	0.16**	0.03	0.18**	0.04	0.10**	0.02	0.11*	0.03
Prior prison and bond amount	0.04**	0.01	0.04*	0.01	—	—	—	—
Pretrial detention	—	—	—	—	0.11*	0.05	0.20**	0.07
Hired attorney and pretrial detention	—	—	—	—	0.19**	0.03	0.11**	0.04
Prior prison and pretrial detention	—	—	—	—	0.08**	0.01	0.08**	0.02

Notes. *N* = 3,459. Model fit (root mean square error of approximation): RMSEA for “Black” = 0.038; RMSEA for “Black males aged 18 to 29” = 0.039.

\* *p* < .05. \*\* *p* < .01.

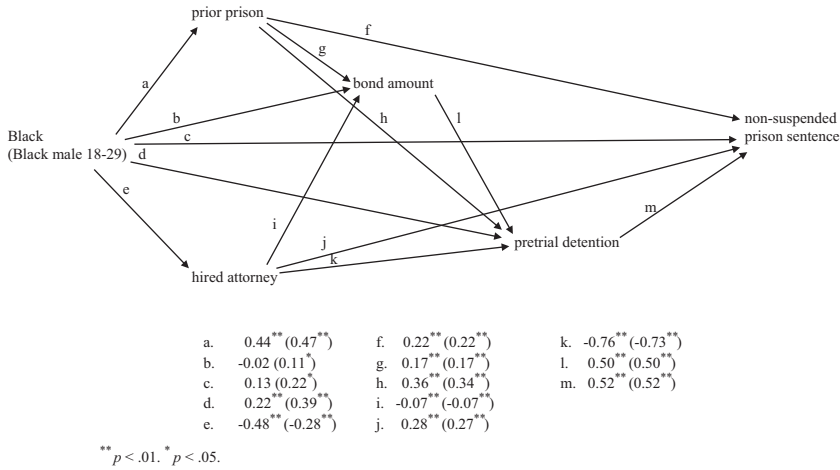
cannot post bond. It is pretrial detention, however, that is the more relevant mediator between race and sentencing, and bond amounts are relevant only as they shape the odds of pretrial detention. The latter has a more direct bearing on sentencing because of the potential of pretrial detention to interfere with defense preparations (Demuth and Steffensmeier, 2004; Goldkamp and Gottfredson, 1985) and to contribute to court actors’ stereotypes of particular defendants as more dangerous to the community (Sutton, 2013).

Table 4 displays the total, direct, and indirect effects of a defendant’s race on pretrial release and imprisonment. These effects are further broken down into the effects of race in general (Black or White) and young Black males versus all other defendants. The “total effects” are the sum of direct and indirect race effects, whereas the “direct effects” are the effects of a defendant’s race on each outcome net of significant statistical controls. The “indirect effects” are the effects of a defendant’s race on pretrial detention and sentencing mediated by the indicators of cumulative disadvantage (bond amount, pretrial detention, prior prison sentence, and type of attorney). Estimates of all hypothesized paths displayed in Figure 1 are displayed in Figure 2.<sup>22</sup>

22. The indirect effects in Table 4 are products of the coefficients along any one chain linking race to pretrial detention and sentencing. For example, paths (e) and (k) in Figure 2 represent the chain from race to

**FIGURE 2**

**Estimates of Direct and Indirect Race Effects on Pretrial Detention and Imprisonment**



The findings for race-group differences in pretrial detention and sentencing displayed in Table 4 highlight the importance of partitioning direct versus indirect race effects, where larger portions of the total effects for Blacks in general and young Black males in particular consist of indirect effects. The direct effects on pretrial detention for both subsamples and on prison for Black males aged 18–29 are significant, although analyses of these effects alone would miss significant racial disparities in each outcome stemming from race-group differences in the mediators examined. Also noteworthy is that the direct effect of race in general on the odds of a nonsuspended prison sentence is nonsignificant, so the significance of the total race effect is driven only by the indirect effects.

Despite some larger direct and indirect effects for young Black males relative to Blacks (although not always the case), these differences are not as dramatic as we expected based on discussions of how young Black men are treated more severely in U.S. courts (e.g., Spohn and Holleran, 2000; Steffensmeier et al., 1998). Our estimates do not refute this idea altogether, but they do not provide compelling evidence that this is the case in the jurisdiction examined.

All indirect race effects on pretrial detention combined were actually larger in magnitude for Blacks in general relative to young Black men, although the difference is not statistically

pretrial detention via hired attorney. The indirect effect of 0.36 in Table 4 is the product of (e)  $-0.48$  and (k)  $-0.76$ . Three-path chains often seem much weaker than two-path chains because the first indirect effect is dampened by two mediators instead of only one.

significant based on the equality of coefficients  $z$  test. There are more similarities than differences in these comparisons, including significant indirect effects on pretrial detention in the predicted directions for four of the five pathways. The primary difference is the nonsignificant indirect effect via bond amounts alone for Blacks (consistent with the null race effect on bond amount in Table 2) versus the significant indirect effect for young Black males. The most substantive effects for both groups involved the two-path chains with hired attorneys and prior imprisonment. Black suspects were less likely to hire attorneys, and suspects with hired attorneys were less likely to be placed in jail prior to trial—hence, the positive indirect race effect resulting from the product of two negative paths. In contrast, Black suspects were more likely to have histories of imprisonment, and suspects with histories of imprisonment were more likely to serve pretrial detention, resulting in another positive indirect effect. The indirect effects altogether accounted for a 75% increase in the odds of pretrial detention for Blacks relative to Whites, whereas the direct effect accounted for a 25% rise in the odds ratio. For young Black males, these figures were 63% and 48%, respectively.

The findings for indirect race effects on pretrial detention provide more specific insight into a general finding uncovered in some (but not all) empirical studies regarding higher odds of pretrial detention for minority suspects (Ayres and Waldfoegel, 1994; Chiricos and Bales, 1991; Demuth, 2003; Demuth and Steffensmeier, 2004; Katz and Spohn, 1995; Kutateladze, Andiloro, Johnson, et al., 2014; LaFree, 1985; Lizotte, 1978; Patterson and Lynch, 1991; Spohn, 2009; Sutton, 2013). That is, although these odds might be shaped directly to some extent by a suspect's race (particularly for young men), much of the disparity might hinge on minority suspects' higher odds of having served prison time and their lower odds of retaining private counsel. And although the mediating effect of bond amount might be relevant for young Black men, it is weaker than these other mediating effects. Our findings suggest that Blacks do face greater cumulative disadvantages at the pretrial stage where factors such as histories of imprisonment, an inability to hire private counsel, and higher bond amounts accrue disproportionately for Blacks to generate higher odds of pretrial detention relative to Whites, even when controlling for other legally relevant factors.

The analysis of nonsuspended prison sentences revealed race differences in the cumulative disadvantages associated with pretrial detention, which has been the dominant focus in the limited number of related studies to date. The impact of all indirect race effects involving pretrial detention combined is substantial ( $b = 0.38$ ), considering not only how pretrial detention alone mediates the race effect on sentencing ( $b = 0.11$ ;  $p < .05$ ) but also the higher odds of pretrial detention for Blacks with prior imprisonment ( $b = 0.08$ ;  $p < .01$ ) and court-appointed counsel ( $b = .19$ ;  $p < .01$ ). These mediating effects alone increased the odds of a prison sentence by 46% relative to Whites (with a corresponding figure of 48% for Black males aged 18 to 29 relative to all other defendants). The *general* indirect race effect on sentencing via pretrial detention is consistent with Kutateladze, Andiloro,

Johnson, et al. (2014), Lizotte (1978), Spohn (2009), and Sutton (2013), and our findings suggest that this general indirect effect might also be influenced in part by differences in pretrial detention odds for suspects with assigned counsel and histories of imprisonment.

Interesting to note is that the direct effect of hired counsel on the odds of a nonsuspended prison sentence was positive and significant (see Figure 2), indicating that indicted suspects who hired their own attorneys were actually *more* likely to end up in prison if convicted, controlling for all other direct and indirect effects. This finding provided some advantage to Blacks at the sentencing stage although the advantage was countered heavily by the sum of other disadvantages related to pretrial detention and prior imprisonment. The positive effect of hired attorneys on prison sentences could reflect, in part, certain disadvantages faced by private counsel in plea negotiations if they are less integrated in courtroom workgroups and lack a more regular presence in the system, as discussed by Eisenstein and Jacob (1977).

The total race effects on prison sentences in both models were significant, as were the total indirect race effects. Both models shared significant indirect effects via all of the mediators examined even though the direct race effect was significant only for young Black men. Aside from some raw differences in effects that suggest even greater disadvantages for Black males aged 18 to 29, there were no dramatic differences in the magnitude of any of these effects between the two models. The total race effect for each group ( $b = 0.48$  for Blacks;  $b = 0.65$  for Black males aged 18–29) also did not differ significantly. In conjunction with the mixed findings for different effects between these groups from the first stage of the analysis, it seems that cumulative disadvantages accrue disproportionately to Blacks in general in this particular court. Black suspects were 40% more likely than Whites to be convicted and sent to prison as a result of the cumulative effects of pretrial detention, prior prison sentences, and hired attorneys, whereas Black males aged 18 to 29 were 50% more likely than all other suspects in the sample to be convicted and sent to prison. When we separate out the inverse indirect effect of hired attorney on the odds of being sent to prison, these figures become 50% and 60% for Blacks and Black males aged 18 to 29, respectively.

The significant total indirect race effect on imprisonment for Blacks in general in conjunction with the nonsignificant direct race effect demonstrate how racial disparities in imprisonment can persist in a correctional system even when a defendant's race is excluded from consideration by judges at sentencing or by prosecutors in plea agreements. Overt biases against minorities might not be the norm that drives the overrepresentation of Blacks in prison relative to their distribution in the general U.S. population. Rather, a defendant's race is linked to other, more proximate effects on sentencing. The failure to recognize these indirect race effects could lead scholars to miss some of the underlying sources of race-group differences in incarceration rates, which was also demonstrated by Spohn (2009).

As displayed in the race-specific models of nonsuspended prison sentences (Table 3), the effects of pretrial detention and prior imprisonment served as significant disadvantages for *both* Blacks and Whites. However, the indirect effects in Table 4 demonstrate that

these particular disadvantages were greater for Blacks as reflected in the significant and positive indirect race effects on imprisonment. In short, the larger proportions of Blacks with histories of imprisonment and the larger proportions detained in jail prior to trial (see Table 1) ultimately contributed to greater disadvantages for Black defendants at sentencing.

The significant difference in bond amounts between Black males aged 18 to 29 and all other suspects in conjunction with the strong effect of bond amounts on the odds of pretrial detention suggest that the cumulative disadvantage at sentencing attributable to the pretrial detention of young Black men is partly caused by this group's inability to post bond. Sixty years ago, Foote (1954) observed that the percentage of defendants who did not make bail in Philadelphia rose consistently as bail amounts increased. This led to his recommendation that bail amounts be lowered for certain offenses. We find little evidence that Foote's recommendation was subsequently adopted by state courts, however, and so impoverished suspects could face even higher odds of prison sentences based solely on their indigent status because of the cumulative effect of pretrial detention on sentencing. This effect is not distributed equally across race groups as a result of the overrepresentation of young Black men in lower socioeconomic status neighborhoods across urban areas in general (Rose and Clear, 1998) and in this particular jurisdiction.

### **Implications**

In the jurisdiction examined, a defendant's race seemed to be linked to pretrial detention and sentencing primarily in terms of the cumulative disadvantages that accrued disproportionately for Black defendants relative to White defendants. That is, the sum of indirect race effects on each outcome was larger than the magnitude of direct effects. The empirical evidence of greater cumulative disadvantages for Black suspects via pretrial detention is consistent with Kutateladze, Andiloro, Johnson, et al. (2014), Spohn (2009), and Sutton (2013), and it highlights the importance of considering the more "subtle" processes contributing to the disproportionate overrepresentation of Blacks in U.S. prisons (Baumer, 2013; Bushway and Forst, 2013; Ulmer, 2012). Aside from further underscoring the relevance of pretrial detention for related studies, our findings also suggest that an examination of indirect race effects via criminal history and type of attorney might improve the understanding of race-group differences in cumulative disadvantages that ultimately impact sentencing decisions. The failure to recognize mediating effects could lead to the assumption that overt biases in sentencing decisions are the norm, whether these involve biases against Black defendants in general or against young Black men in particular. As noted by Stolzenberg et al. (2013), public perceptions of racially disparate treatment by the courts could undermine confidence in the courts and feed the public's cynicism toward legal authority. The cumulative disadvantages that operate disproportionately for young Black men could also further limit their chances for successful integration back into their communities after release from prison, not to mention the potentially negative impact of their incarceration on their partners and dependent children.

From a theoretical standpoint, the finding of stronger cumulative disadvantages for particular demographic groups offers another dimension to the applicability of discretion-based theories to an understanding of racial disparities in sentencing. Court actors' considerations of race as an indicator of risk for future offending at seemingly discrete decision points might actually have unique (indirect) effects on sentencing separate from any *direct* effect of race alone, as racial disparities in treatment at earlier stages of processing (bond amounts and pretrial detention) accrue to generate even greater disadvantages at sentencing. As pointed out by Kutateladze, Andiloro, Johnson, et al. (2014), an empirical focus on cumulative disadvantage also reveals how minorities can still face greater disadvantages in treatment by the courts even when no race effects emerge as significant main effects at separate decision points. For example, here we found significant race effects on the odds of pretrial detention for Blacks in general and even stronger effects for young Black men; yet the magnitude of charge reductions for either subgroup was not significantly higher than that of their comparison groups. Extrapolating from findings for charge reductions alone to other stages of case processing would underestimate racial disparities in case processing. Travis, Western, and Redburn (2014) also pointed out that, even when racial disparities are uncovered at each stage of case processing, these differences are "typically modest, but their cumulative effect is significant" (p. 94).

Toward the end of reducing racial disparities in the distribution of prison sentences, a policy implication of our findings would be to reduce the court's reliance on money bail and/or more careful consideration of bail amounts for indigent defendants. Lowering the going-rate bail amounts for some offenses might help, although reduced amounts might still be overwhelming to indigent defendants (originally observed by Foote, 1954). Another consideration involves increasing opportunities for pretrial detainees to communicate more regularly and effectively with counsel, assuming their inability to do so might contribute to a weaker defense and higher odds of conviction and imprisonment.

From an even broader perspective, when also considering the roles of prior imprisonment and hired attorneys for shaping the odds of pretrial detention, more structured guidelines for determining bond amounts and when to permit bond release and release on recognizance might assist in reducing racial disparities in pretrial detention. The Bail Reform Act of 1984 was an effort to structure bail decisions in the federal courts, but the structure is varied across state courts. For example, some states follow the federal system whereas others do not. Foote (1954) underscored the importance of being able to balance the need to ensure the suspect's appearance at trial with the desire to avoid needless punishment given that the defendant is presumed innocent until proven guilty. However, he noted that any effort to "individualize bail determination must be plagued by the treacherous *uncertainty* inherent in predicting future human behavior" (p. 1035, emphasis added). This observation foreshadowed the theoretical perspectives of Albonetti (1987) and Steffensmeier et al. (1998), who focused heavily on the idea that court actors seek clues to inform them of a defendant's risk of future criminality even if such clues fall outside the information allowed



to be considered in the decision-making process. Structuring decision making to reduce this possibility could therefore benefit minority suspects.

The implications of our findings must be tempered with our limited focus on a single jurisdiction. Given the rarity of related studies to date, it is important to consider how the unique environment of this jurisdiction might have contributed to the significant findings of stronger cumulative disadvantages for Blacks than for Whites. We were approached by the CPO because of public perceptions of racial biases in the treatment of Black suspects by the police and courts. The findings described in this article could have been predestined as a result, although some might find it surprising that there were no significant *direct* general race effects on sentencing given these public perceptions.

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## Evolution of Sentencing Research

**Cassia Spohn**

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Social scientists and legal scholars have been engaged in research examining the complex and multifaceted relationship between race and sentencing for more than eight decades. During this time period, the questions asked have become more theoretically sophisticated and the methodologies used to answer those questions more analytically rigorous. The answers to questions regarding the effect of race on sentence severity also have changed over time.

Studies conducted from the 1930s through the 1960s often concluded that racial disparities in sentencing reflected racial discrimination and that “equality before the law is a social fiction” (Sellin, 1935: 217). Reviews of these early studies (Hagan, 1974; Kleck, 1981), however, found that most of the methodologies were flawed. Many employed inadequate or no controls for crime seriousness and prior criminal record, and most used inappropriate statistical techniques to isolate the effect of race. These methodological problems persisted in research conducted during the 1970s and early 1980s, leading the National Research Council’s Panel on Sentencing Research to claim in its 1983 report that the sentencing process, although not racially neutral, was not characterized by systematic and widespread racial discrimination (Blumstein, Cohen, Martin, and Tonry, 1983). The panel also concluded that the disproportionate number of Black males locked up in U.S. prisons was primarily a result of factors other than racial discrimination in sentencing.

Reviews of research conducted from the mid-1980s through the 1990s reached a somewhat different conclusion (Chiricos and Crawford, 1995; Mitchell, 2005; Spohn, 2000). The authors of these reviews challenged the no-discrimination thesis and suggested that racial disparities in sentencing had not declined or disappeared but had become more subtle and difficult to detect. They contended that testing only for direct race effects was insufficient and asserted that disentangling the effects of race and other predictors of sentence severity required tests for indirect race effects and the use of interactive, as well as

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additive, models. These reviews highlighted the importance of attempting to identify “the structural and contextual conditions that are most likely to result in racial discrimination” (Hagan and Bumiller, 1983: 21).

The research conducted during the last two decades of the twentieth century improved on earlier work in several important ways. Nonetheless, as Baumer (2013; see also Piehl and Bushway, 2007; Ulmer, 2012) argued recently, even these more theoretically sophisticated and analytically rigorous studies left many questions unanswered. Of particular importance is that the typical race and sentencing study from this era, which relied on what Baumer (2013) referred to as “the modal approach” involving regression-based analysis of the final sentencing outcome, could not explain why racial minorities were sentenced more harshly than Whites, whether disparate treatment was found only at sentencing or accumulated as cases moved through the court process, or whether the disparities that appeared reflected decisions made by prosecutors as well as judges. These criticisms of research on racial justice are not new. Forty years ago, Hagan (1974: 379) called for studies that better captured “transit through the criminal justice system” especially as it operates “cumulatively to the disadvantage of minority group defendants.” Four decades later, Baumer (2013: 240) reiterated this concern, arguing that “it would be highly beneficial if the next generation of scholars delved deeper into the various ways that ‘race’ matters “across multiple stages of the criminal justice process.”

Wooldredge, Frank, Goulette, and Travis’s (2015, this issue) study of the impact of cumulative disadvantage on sentencing responds directly to this challenge. By using data on a sample of felony defendants prosecuted in a large Northern jurisdiction in the United States and sophisticated analytical techniques, Wooldredge et al. examine whether Blacks fare worse than Whites (and whether young Black males fare worse than other defendants) at a series of discrete decision points (i.e., bond amount, pretrial detention, charge reduction, nonsuspended prison sentence, and sentence length). They also estimate a series of structural equation models testing for the direct and indirect effects of race on pretrial detention and sentencing. Unlike prior research, which focused almost exclusively on the indirect effect of race on sentencing through pretrial detention, they estimate the indirect race effects on pretrial detention via bond amount, type of attorney, and prior imprisonment and the indirect race effects on sentencing through pretrial detention, type of attorney, and prior imprisonment. They also build on prior research by estimating separate models for young Black males.

The results of their analysis of the discrete decision points revealed that (a) race did not affect bond amounts, charge reductions, the type of sentence, or the length of the sentence but did affect pretrial detention; (b) the interaction of race, sex, and age (i.e., young Black males versus all other defendants) affected bond amounts, pretrial detention, and the type of sentence but did not affect charge reductions or sentence length; and (c) no race differences were found in the effects of bond amount on pretrial detention or the effects of pretrial detention and prior imprisonment on charge reductions and the likelihood of



imprisonment after conviction. Their structural equation models revealed that both Blacks in general and young Black males in particular had higher odds of pretrial detention but that the direct effect of race on sentence type was confined to young Black males. More importantly, this aspect of the analysis highlighted the importance of indirect race effects, which were larger than the direct race effects. For both Blacks and young Black males, race affected pretrial detention indirectly via hired attorney, hired attorney and bond amount, prior imprisonment, and prior imprisonment and bond amount; race affected the odds of a prison sentence indirectly via hired attorney, prior imprisonment, pretrial detention, hired attorney and pretrial detention, and prior imprisonment and pretrial detention. According to Wooldredge et al. (2015), “Aside from further underscoring the relevance of pretrial detention for related studies, our findings also suggest that an examination of indirect race effects via criminal history and type of attorney might improve the understanding of race-group differences in cumulative disadvantages that ultimately impact sentencing decisions.”

One of the important contributions of Wooldredge et al.’s (2015) article is that it adds to the small but growing body of research documenting cumulative disadvantage in the criminal justice system. As numerous scholars (Baumer, 2013; Kutateladze, Andiloro, Johnson, and Spohn, 2014; Ulmer, 2012) have recently pointed out, research on racial and ethnic disparity typically has been limited to a single decision-making point—usually the final sentencing decision—which captures only a static snapshot of the more dynamic process that constitutes criminal punishment. These studies, many of which find a direct race effect on sentencing, cannot identify the casual mechanisms that produce these disparities or the ways they are altered through the life course of criminal cases. Moreover, research that finds no direct race effect at sentencing can lead to erroneous conclusions that the process of determining the appropriate sentence is racially neutral, when, in fact, disparities at earlier stages of case processing lead to significant disadvantages for racial minorities at sentencing. As Wooldredge et al. demonstrate, although race in general did not have a direct effect on the likelihood of imprisonment, it did affect the odds of incarceration through its effects on pretrial detention, prior imprisonment, and type of attorney. Blacks, in other words, were more likely than Whites to be imprisoned after conviction because they were more likely to be detained pretrial, more likely to have a prior prison sentence, and less likely to be represented by a private attorney. As research has shown (Kutateladze, et al., 2014; Stolzenberg, D’Alessio, and Eitle, 2013; Sutton, 2013), certain combinations of discretionary court decisions at earlier stages of case processing can accumulate to produce marked racial disparity in punishment.

Another important contribution of this article is that it broadens the discussion of indirect race effects on sentencing. As noted, most research testing for indirect effects has focused on the effect of pretrial detention; much of this research has documented that Blacks (and Hispanics) face higher odds of pretrial detention and, as a result, receive harsher sentences. By testing for the indirect effect of bond amount on pretrial detention and for

the indirect effects of prior imprisonment and type of attorney (in addition to pretrial detention), Wooldredge et al. demonstrate that race interacts with case outcomes and case characteristics other than pretrial detention. This finding is significant as it highlights the myriad ways in which race affects criminal justice outcomes. Of particular interest is Wooldredge et al.'s finding that Blacks are more likely than Whites to have a prior prison sentence and, as a result, are more likely than Whites to be detained prior to trial and to be sentenced to prison for the current offense. Most policy makers and researchers assume that the offender's criminal history is legally relevant to the sentencing decision; they further assume that judges who premise detention and sentencing decisions—and especially the decision to incarcerate or not—on the offender's criminal history are making legitimate and racially neutral decisions. However, one can make an argument that prior criminal record is a race-linked variable. If police target certain types of crimes—for example, selling illegal drugs—or patrol certain types of neighborhoods—for example, inner-city neighborhoods with large Black populations—more aggressively (see, for example, Beckett, Nyrop, and Pfingst, 2006; Beckett, Nyrop, Pfingst, and Bowen, 2005), then Blacks could be more likely than Whites to “accumulate” a criminal history that can be used to increase the punishment for the current offense. If this is in fact what is happening, then it would be misleading to conclude that sentences based on prior imprisonment are racially neutral. Similarly, it would be misleading to conclude that the absence of a race effect once this variable is taken into account signals the absence of racial discrimination in sentencing.

Wooldredge et al.'s results (2015) confirm what research has shown about the interaction of the offender's race, sex, and age (Spohn and Holleran, 2000; Steffensmeier, Ulmer, and Kramer, 1998); the results highlight the “high cost of being black, young, and male” (Steffensmeier et al., 1998: 789). Whereas Blacks faced higher odds of pretrial detention, but not higher odds of imprisonment, than Whites, young Black males were both more likely to be detained and more likely to be imprisoned. Moreover, the differences in the odds of pretrial detention and imprisonment for young Black males and all other offenders were both statistically significant and nontrivial. The probability of being detained prior to trial was .68 for young Black men compared with .22 for all other defendants. Similarly, the likelihood of being sentenced to prison was .40 for young Black men versus .28 for all other offenders. These findings confirm that *certain types of offenders* are regarded as more problematic and, thus, as more in need of formal social control. They suggest that race, sex, and age are linked to judges' perceptions of dangerousness, culpability, and potential for reform. Spitzer (1975: 645) used the term “social dynamite” to characterize that segment of the deviant population viewed as particularly threatening and dangerous: He asserted that social dynamite “tends to be more youthful, alienated and politically volatile,” and he contended that those who fall into this category would be more likely than other offenders to be formally processed through the criminal justice system and would receive harsher treatment than other offenders. The results of Wooldredge et al.'s (2015) study demonstrate that young Black males suffer more cumulative disadvantage as their cases are processed

by the criminal justice system. Future research should broaden this approach to determine whether offenders with other constellations of characteristics—for example, Hispanics and the unemployed (Spohn and Holleran, 2000)—experience cumulative disadvantage in a similar way.

The data that are missing from Wooldredge et al.'s (2015) study—and, indeed, from most research examining criminal justice outcomes—are data on charging decisions of prosecutors, specifically the initial decision to file charges or not and the severity of charges filed (but see Kutateladze et al., 2014, and Rehavi and Starr, in press). This omission is important as a decision not to file charges obviously means that the defendant will not be prosecuted and decisions regarding the severity of the filed charges, especially in jurisdictions with presumptive guidelines or mandatory minimum sentences, affect—and in some cases determine—the sentence that will be imposed. Like prior criminal record, charge severity may be a race-linked rather than a racially neutral variable. If prosecutors routinely file more serious charges against Blacks than against Whites who engage in the same type of criminal conduct, or offer less attractive plea bargains to Blacks than to Whites, then the more serious conviction charges for Blacks will reflect these racially biased charging and plea-bargaining decisions. A Black defendant who is convicted of a more serious crime than a similarly situated White defendant, in other words, might not necessarily have engaged in more serious criminal conduct than his or her White counterpart. Although Wooldredge et al. (2015) examined race effects on charge reductions (finding no differences based on race or the interaction among race, sex, and age), they could not determine whether race affected the severity of the initial charges filed by the prosecutor. Given the important linkage between charge severity and sentence severity, future research should attempt to tease this out and to estimate how prosecutorial charging decisions contribute to cumulative disadvantage for Black defendants.

### **Policy Implications**

In 2004, the United States celebrated the 50th anniversary of *Brown v. Board of Education* (1954), the landmark Supreme Court case that ordered desegregation of public schools. Also in 2004 the Sentencing Project issued a report titled “Schools and Prisons: Fifty Years after *Brown v. Board of Education*.” The report noted that, whereas many institutions in society had become more diverse and more responsive to the needs of people of color in the wake of the *Brown* decision, the American criminal justice system had taken “a giant step back-ward” (The Sentencing Project, 2004: 5). To illustrate this, the report pointed out that in 2004 there were nine times as many Blacks in prison or jail as on the day the *Brown* decision was handed down—the number increased from 98,000 to 884,500. The report also noted that one of every three Black males and one of every eighteen Black females born at the turn of the century could expect to be imprisoned at some point in his or her lifetime. The report concluded that “such an outcome should be shocking to all Americans” (The Sentencing Project, 2004: 5).

Wooldredge et al. (2015) suggest that the reasons behind these “shocking” numbers are complex. They suggest that it is inappropriate and misleading to focus solely on the final sentencing decision, as conceptualizing how racial disparity in incarceration occurs and persists requires an understanding of the ways in which disparate treatment at discrete decision points during the life course of a criminal case accumulates. Black defendants suffer cumulative disadvantage as a result of being unable to post bond, which leads to a higher likelihood of pretrial detention, a lower likelihood of being able to hire an attorney, and thus to higher odds of imprisonment. Black defendants also face higher odds of pretrial detention and imprisonment because they are more likely than White defendants to have served time in prison previously, which may or may not reflect a more serious criminal history than that found for similarly situated White defendants.

Reducing the racial disproportionality in our nation’s prisons and eliminating racial bias in sentencing should be highly prioritized goals of policy makers and politicians. The mass imprisonment of young Black (and Hispanic) men has altered their life-course trajectories (Western, 2006), which in turn has had dire consequences for their families, children, and communities. Evidence that race infects the sentencing process undermines respect for the law and casts doubt on the ability of the criminal justice system to ensure due process for all and equal protection under the law. The way forward seems clear, although the policy reforms needed may not be politically palatable. Reducing the court’s reliance on money bail and increasing the use of release on recognizance, as Wooldredge et al. (2015) suggest, is an obvious place to start. Doing so will reduce the odds of pretrial detention for racial minorities, who are disproportionately likely to be poor, and will have spillover effects on subsequent decisions, including the decision to incarcerate, which are affected by the defendant’s pretrial status. Although not a focus of this policy essay, mandatory minimum sentencing statutes and three-strikes policies that base sentence severity on either charge severity or criminal history also should be revised or repealed, as both charge severity and criminal history are arguably race-linked rather than racially neutral variables.

Research examining the relationship between race and sentencing has evolved both theoretically and methodologically during the past five decades. Of particular importance is the fact that the questions asked have changed dramatically. Most researchers now acknowledge that it is overly simplistic to ask whether race and ethnicity matter at sentencing. The more interesting questions—and those whose answers will help us understand the mechanisms underlying the harsher punishment imposed on Blacks and Hispanics—revolve around the contexts in which or the circumstances under which race and ethnicity influence sentencing and the ways in which disparities accumulate over the life course of a criminal case. As the latest wave of race and sentencing research continues to unfold and as researchers devise new ways of estimating cumulative disadvantage, more definitive answers to questions regarding racial disparity and racial discrimination in punishment should be forthcoming.

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## Attenuating Disparities Through Four Areas of Change

Universal Release, Reimagined Policing, Eliminated Prior Records, and Funded Public Defenders

**Traci Schlesinger**

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Wooldredge, Frank, Goulette, and Travis's (2015, this issue) article is a welcome and necessary contribution to the literature on racial disparities in criminal processing that takes the mechanisms that produce disparities as seriously as the disparate outcomes themselves. Wooldredge et al. find evidence of direct anti-Black disparities in processing decisions only among some demographics and during some processing decisions—disadvantaging Black men between 18 and 29 years old in the granting of non-suspended prison sentences. In contrast, they find consistent, substantial, and significant evidence of indirect racial disparities that negatively impact all Blacks produced through three main mechanisms: bond amount, hired versus appointed lawyers, and prior imprisonment. Excitingly, innovative and fearless policy to address each of these mechanisms can markedly attenuate not only disparities in sentencing but also the scope of the carceral state.

### **Bond Amount: Universal Releases**

It is a paradox of criminal justice that bail, created and molded over the centuries in England and America primarily to facilitate the release of criminal defendants from jail as they await their trials, today often operates to deny that release (Schnacke, 2014: 1).

A preponderance of research supports Wooldredge et al.'s (2015) findings that higher bond amounts increase both racial disparities in pretrial detention (Ball and Bostaph, 2009; Bynum, 1982; Demuth, 2003; Demuth and Steffensmeier, 2004; Petee, 1994) and, through pretrial detention, the likelihood of incarceration and sentence length (Albonetti, Hauser,

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Hagan, and Nagel, 1989; Clark, Austin, and Henry, 1997; Nobiling, Spohn, and DeLone, 1998; Spohn and Cederblom, 1991; Sutton, 2013). “Cumulative disadvantage” focuses on Black–White disparities in criminal processing; this focus is perhaps the result of the demographics of the jurisdiction Wooldredge et al. are examining. Supplementing their findings, other research shows that anti-Latino disparities in bond amounts are strikingly large as well and perhaps the most critical point of disparate decision making for Latino defendants (Nagel, 1982; Schlesinger, 2005). By looking at the connection between bond amounts and later criminal processing outcomes, Wooldredge et al. find both that “prohibitive bond amounts were the primary reason for not obtaining pretrial release” and that “reduced amounts might still be overwhelming to indigent defendants.”

Sixty percent of jail inmates are awaiting adjudication; nationwide, as in this jurisdiction, their inability to pay is the main reason why they are incarcerated. The Pretrial Justice Institute estimates the cost of pretrial detention to be \$9 billion dollars a year, which could be better spent on policy initiatives that offer services such as housing, employment, and child care to defendants who are awaiting trial. Beyond the monetary cost, however, pretrial incarceration, like all experiences of incarceration, takes defendants away from jobs, families, and schools, and it places a stigma on them (Clear, 2007; Pager, 2007; Pettit and Western, 2004). Likely through the deteriorated ties with families and communities, lost jobs and housing, and the disrupted educations that it creates, pretrial incarceration also increases recidivism (Pretrial Justice Institute, 2014). Racial disparities in pretrial detention, then, will produce racial disparities in a plethora of lived outcomes from family and community ties, to employment and education outcomes, to housing.

Let us take a minute to look at the current status of pretrial detention and both the current state of and what is commonly recommended as a “best practice” for pretrial processing. According to the U.S. Department of Justice, the United States detains more people pretrial than any other nation and has a pretrial detention rate that is three times the world average (Schnacke, 2014). Moreover, nearly 20% of the 2.4 million people who are incarcerated in the United States, or 457,500 people, are being held in local jails on any given day, pretrial, without having been found guilty of any crime. These individuals make up approximately 60% of all people incarcerated in local jails (Schnacke, 2014: 22).

Although diverse practices are in play, most U.S. jurisdictions currently rely on bond schedules. These schedules charge defendants a preset amount based on the crime or crimes with which they have been charged. In February 2015, the Department of Justice’s statement of interest filed with the U.S. District Court for the Middle District of Alabama in *Varden v. The City of Clanton* (2015) had the following to say about bond schedules:

Incarcerating individuals solely because of their inability to pay for their release, whether through the payment of fines, fees, or a cash bond, violates the Equal Protection Clause of the Fourteenth Amendment. . . . It is the position of the United States that, as courts have long recognized, any bail or bond scheme



that mandates payment of pre-fixed amounts for different offenses in order to gain pre-trial release, without any regard for indigence, not only violates the Fourteenth Amendment's Equal Protection Clause, but also constitutes bad public policy. (U.S. Department of Justice 2015: 1)

As the Department of Justice, nonprofits, practitioners, and many jurisdictions have realized the problems inherent in bond schedules, the main policy push has been toward risk-assessment models, which often are hailed as “evidenced based.” Evidenced based, as a phrase, however, makes sense only when we say that the evidence proves this tool is good at producing a particular outcome; risk-assessment tools are focused at reducing recidivism and flight risk—and sometimes at reducing detention, with different tools giving differing weight to each of these goals. As a diversity of risk-assessment tools are situated in a diversity of communities, their effectiveness at achieving each of these goals varies (Annie E. Casey Foundation, 2013; Schnacke, 2014). In some cases, the problem with risk-assessment tools is that they are not good at achieving their own goals, but the more essential problem is twofold: First, they do not sufficiently focus on release, and second, they are likely to increase racial disparities (Simon, 1988). The Pretrial Justice Institute's suggested risk-assessment criteria, for example, include employment, residential stability, and mental illness (Pretrial Justice Institute, 2014). These criteria often are considered at the pretrial stage; nonetheless, they are all race salient, with Black, Latino, and First Nation people being more likely to be unemployed, have low residential stability, have had more experiences of trauma because of social vulnerability, and have more diagnoses of mental illness because of their increased contact with the welfare state (Corneau and Stergiopoulos, 2012; Dawkins, Shen, and Sanchez, 2005; Schieman, 2005; Sue et al., 2007). To the extent that all the criteria that these scales rely on correlate with Black-ness, Latino-ness, and First Nation-ness, moving toward risk-assessment scales based on these criteria will *increase* racial disparities.

The Juvenile Detention Alternatives Initiative (JDAI) has intentionally sought to avoid this outcome by having their jurisdictions implement several practices, from racial impact statements to better data collection. They also ask their jurisdictions to develop risk-assessment tools that do not rely on racialized criteria. JDAI is now being replicated in almost 200 jurisdictions in 39 states and the District of Columbia, and its reforms are associated with a 44% decrease in juvenile pretrial detention. Undoubtedly, this achievement is remarkable. Moreover, although critics might worry about concomitant increases in rearrests and failures to appear, both measures decreased in those jurisdictions that adopted the JDAI reforms—those jurisdictions that now detain 44% fewer juveniles pretrial—by 13% and 6%, respectively (Annie E. Casey Foundation, 2013). Because the juvenile field is distinct, JDAI jurisdictions are neither moving away from bond schedules nor moving toward risk-assessment scales that rely on (adult) racialized criteria such as employment and residential stability. In fact, JDAI has been intentional about having its jurisdictions develop risk-assessment tools that are designed to decrease racial disparities in detention, as stated

previously. However, although JDAI has been successful at decreasing detention, rearrests, and failures to appear, its reforms have not decreased racial disparities in pretrial detention for juveniles. In the year JDAI began implementing its reforms, 76% of detained youth were youth of color; in 2013, 78% of detained youth in JDAI's reform jurisdictions were youth of color (Annie E. Casey Foundation, 2013).<sup>1</sup> As such, even risk-assessment scales meant to decrease racial disparities seem incapable of achieving this goal.

We can do better. The Department of Justice argues that the purpose of bail, understood as the pretrial process itself, should emphasize “pretrial freedom with conditions set to provide a reasonable assurance, and not absolute assurance, of court appearance and public safety” (Schnacke, 2014: 116). As such, policies that focus on public safety or flight risk rather than on release have their priorities backward—particularly when they are steeped in myopic understandings of public safety that see only the danger a defendant poses to her community and neither the danger incarceration poses to defendants nor the danger incarceration poses to the community from which defendants are drawn. Adopting policies of universal release with the option of detention with due process in limited cases will be a genuine move toward the abolition of pretrial detention. This approach will not only be a step toward decreasing the scope of the carceral state. Because pretrial processing is the moment in criminal processing where racial disparities are the most pronounced (Albonetti et al., 1989; Ball and Bostaph, 2009; Demuth, 2003; Demuth and Steffensmeier, 2004; Katz and Spohn, 1995; Nagel, 1982; Schlesinger, 2005, 2013), it also will be a solid step toward attenuating cumulative racial disparities in sentencing and incarceration. The American Bar Association Standards states:

After a hearing and the presentment of an indictment or a showing of probable cause in the charged offense, the government proves by clear and convincing evidence that *no condition or combination of conditions of release will reasonably ensure the defendant's appearance in court or protect the safety of the community or any person*, the judicial officer should order the detention of the defendant before trial. (American Bar Association, 2015, Section 10–5.8; emphasis added)

However, flight risk is not a sufficient basis for punitive pretrial incarceration, which is an experience of trauma that simultaneously increases a defendant's odds of experiencing acute violence, particularly given that flight risk can be sufficiently addressed through a bench warrant and arresting officer. As such, jurisdictions should push even further than this and adopt a standard that states:

After a hearing and the presentment of an indictment or a showing of probable cause in the charged offense, the government proves by clear and convincing

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1. Another way to look at it is that there was a 44% overall reduction in the JDAI jurisdictions but a 46% youth of color reduction in these jurisdictions. Even from this perspective, the reduction in disparities is miniscule compared with the overall reduction.

evidence that no condition or combination of conditions of release will protect the safety of the community or any person and that evidence has been presented to substantiate that the defendant is likely to engage in serious violent behavior if released, the judicial officer should order the detention of the defendant before trial. *In all other cases, the defendant should be released until her adjudication.*

Although a call to move toward universal release will raise concerns about community safety and flight risk, policy makers should look to the JDAI data, which show that in the almost 200 jurisdictions where their policies are being implemented, substantial increases in pretrial release are associated with marked decreases in rearrests and failures to appear (Annie E. Casey Foundation, 2013). Moreover, Wooldredge et al. (2015) are wise to direct their readers' attention to the punitive nature of pretrial detention—it is after all incarceration. Policy makers must recognize the violence of incarceration itself as these institutions decrease life expectancy (Gilmore, 2007) and are sites of trauma and violence (INCITE! Women of Color Against Violence, 2006). In response, jurisdictions must prioritize the safety of defendants and the safety of the communities from which defendants are drawn (Clear, 2007; Davis, 2003; Gilmore, 2007; Pettit and Western, 2004; Western, 2007).

In light of Lopoo and Western's (2005) research that found that incarceration increases individuals' odds of engaging in domestic violence after release when compared with similarly situated individuals who did not experience incarceration, we must ask ourselves, for each person we protect through incarceration, how many do we harm? The last decade of research on the impact of incarceration shows that the public safety hazard of pretrial incarceration (Pager, 2007; Petersilia, 2003; Pettit and Western, 2004; Wakefield and Wildeman, 2011) and the racial disparities in incarceration that it works to create are greater and more certain than the public safety hazard of releasing people pretrial. This becomes particularly evident after we note, as presented by the U.S. Department of Justice's *Guide for Pretrial Practitioners*, that:

[V]ery few defendants misbehave while released pretrial . . . for example, the D.C. Pretrial Services Agency reports that in 2012, 89% of released defendants were arrest-free during their pretrial phase, and that only 1% of those arrested were for violent crimes; likewise, Kentucky reports a 92% public safety rate. (Schnacke, 2014: 102)

If keeping someone in jail is not the most cost-effective way to assure that she shows up for her adjudication and is an abrogation of justice, if defendants released pretrial are rarely rearrested for violent crimes, and if ability to pay never had anything to do with either of these factors in the first place, then why are jurisdictions still relying on bond schedules even after the U.S. Department of Justice and prominent nonprofits in the area have all shown them to be ineffective and unethical? If risk-assessment models rely on racialized criteria like unemployment, residential instability, and mental health needs and will thus

increase racial disparities in pretrial incarceration, then why are we looking at this as our solution? Each “risk-assessment” criteria points to a need for services, not for incarceration (Monahan et al., 2001).

In addition to the moral and constitutional imperative against punishing defendants who have yet to be adjudicated, working toward the abolition of pretrial detention will help us lower cumulative racial disparities in sentencing. Because pretrial detention is a particularly racialized moment in criminal processing, moving toward universal release will eliminate racial disparities in this experience of incarceration, which accounts for 20% of all people incarcerated on any given day. Moreover, because pretrial incarceration increases both people’s odds of experiencing postadjudication convictions and incarceration and their sentence lengths, this policy move will decrease both the scale of and racial disparities in postadjudication incarceration. In response to the accumulating evidence on the harm incarceration does to individuals and communities and the lack of evidence of harm released defendants pose to communities, jurisdictions should adopt policies that assume the pretrial release of all defendants.

### **Prior Records: Reimagining Policing and Eliminating Prior Records**

Processing decisions, from pretrial through sentencing, explicitly consider prior imprisonment and prior records more generally; not surprisingly, scholars have found that defendants with more serious prior records and those who have already been to prison receive more punitive outcomes both during pretrial processing (Ball and Bostaph, 2009; Demuth and Steffensmeier, 2004; Freiburger, Marcum, and Pierce, 2010; Spohn and Cederblom, 1991) and during sentencing (Albonetti, 2002; Bjerk, 2005; Daly and Tonry, 1997; Ulmer and Johnson, 2004). In fact, prosecutors’ and judges’ consideration of prior records during criminal processing produces much of the racial disparities we see in criminal legal outcomes (Albonetti et al., 1989; Schlesinger, 2013). Because Blacks have the most serious prior records and Whites the least, this produces anti-Black and anti-Latino disparities in a variety of criminal legal outcomes during the pretrial and sentencing stages (see Free, 2001, and Spohn, 2000, for incisive reviews of racial disparities pretrial and sentencing processing, respectively).<sup>2</sup> Policy changes in two broad areas are needed to address this mechanism of production of cumulative racial disparities:

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2. In many studies, scholars have not calculated the percentage of racial disparities that is created through the consideration of prior records. However, the confluence of the large racial disparities in prior records present in the demographic data, and the large and significant association between the prior record variables and the criminal legal outcomes they consider, strongly suggests that the consideration of prior records is an important mechanism through which racial disparities are generated (Albonetti, 2002; Blair, Judd, and Chapleau, 2004; Bushway and Piehl, 2007; Caravelis, Chiricos, and Bales, 2011; Daly and Tonry, 1997; Demuth and Steffensmeier, 2004; Freiburger et al., 2010; Harris, 2009; Ulmer and Johnson, 2004; Ulmer and Kramer, 1998).

- (1) Changes in police policies—including an end to hot spot policing, anti-gang initiatives, and stop-and-frisk policies and a reevaluation of what constitutes a problem in need of police attention.
- (2) An end to the consideration of prior records during criminal processing.

Racialized policing practices produce substantial and stable racial disparities in prior records. Durán (2009), Rios (2011), and Zatz and Krecker (2003) all demonstrate convincingly that anti-gang initiatives are thinly veiled methods of police profiling and targeting of young Black and Latino youth. Many anti-gang initiatives allow police to enter youth into databases as belonging in a gang as long as they exhibit a certain number of gang-related behaviors, including wearing baggy clothing or being seen with a known gang member—even a sibling or cousin. These criteria are so race salient that in 1992 in Los Angeles, nearly half of all Black men between 21 and 24 years of age were labeled as gang members; the same was true in Denver and other cities throughout the country. If a young person who is labeled this way is eventually arrested and convicted, then many anti-gang ordinances include provisions for him to then be subjected to a sentencing enhancement that adds 3, 5, or 8 years to his sentence. As the youth receiving these elongated sentences are nearly all Black or Latino, it is clear how these police tactics operate as a mechanism in the production of cumulative racial disparities (Durán, 2009; Rios, 2011; Zatz and Krecker, 2003).

Moving to another racialized police practice, Fagan and Davies (2000) and Gelman, Fagan, and Kiss (2007) used multilevel models to find that, in New York City in the years 1998 and 1999, police were more likely to stop and frisk Blacks and Latinos than they were similarly situated Whites; this was true even when controlling for precinct variation and race-specific estimates of crime participation. Following this up, in February 2007, the New York City Police Department (NYPD) released statistics that indicated that more than 500,000 pedestrians had been stopped on suspicion of a crime in New York City in 2006 through the stop, question, and frisk policy; almost 90% of the stops involved people of color. In the wake of this, the NYPD asked the RAND Center on Quality Policing to evaluate their practice for racial disparities. RAND found that officers frisked, searched, and used force against White suspects less often than they did similarly situated Black and Latino suspects even though they found contraband more often on the White suspects (Ridgeway, 2007). On August 12, 2013, a federal judge ruled that the stop-and-frisk practices of the NYPD violated the constitutional rights of New Yorkers (Goldstein, 2013). Less than two years later, on April 20, 2015, six African American men from the South and West sides of Chicago filed a federal lawsuit against the city of Chicago, police Superintendent Garry McCarthy, and 14 unnamed police officers alleging that the “suspicionless” street stops have led to constitutional abuses including unlawful searches and seizures as well as excessive force (Meisner, 2015). In the summer of 2014, the Chicago police made 250,000 stops that did not lead to an arrest; according to the American Civil Liberties Union (ACLU) of Illinois, this means that Chicagoans were four times more likely to be stopped by police than were

New Yorkers at the height of the NYPD's stop, question, and frisk policy. Moreover, Black Chicagoans were subjected to 72% of all stops even though only 32% of people living in the city were Black. Disproportionate stops occurred in both majority Black and majority White police districts. Unlike in New York City, however, where detailed data collection made it possible for an outside agency to conduct a thorough review of racial disparities in NYPD's stop, question, and frisk practices, Chicago does not collect enough data to allow for a comprehensive evaluation. According to the ACLU, "Officers do not identify stops that result in an arrest or ordinance violation, and they do not keep any data on when they frisk someone" (ACLU, 2015: 3). Even with the paucity of data available, however, it is evident that Chicago police officers are stopping an inordinate number of people and that disproportionate minority contact is occurring throughout the city. In light of the recent federal ruling on NYPD's stop, question, and frisk policy, Chicago and other cities throughout the country should end this harmful and racially targeted practice.

Although anti-gang and stop-and-frisk policies are controversial for their race-targeted practices, hot spot policing goes almost unnoticed. Used by most U.S. police departments, hot spot policing strategies focus on small geographic areas or places, usually in urban settings, where crime is concentrated (Braga, Papachristos, and Hureau, 2014). Sherman (1995) and Sherman, Gartin, and Buerger (1989) found that, in Minneapolis, 50% of calls to the police came from 3% of small geographic "places." Braga's (2005) review of hot spot policing showed that this tactic works at decreasing crime; however, Kochel (2010) showed that hot spots policing disproportionately impacts disadvantaged neighborhoods of color and decreases the legitimacy of the police among people of color. Moreover, the findings by Beckett, Nyrop, Pflugst, and Bowen (2005) and by Beckett, Nyrop, and Pflugst (2006) can be used to call into question how police departments decide what areas are hot spots by exposing the police's racially saturated ideas of what constitutes a criminal problem. Seattle police, they found, focus on Black outdoor drug markets not because of larger public health risks, public complaints, related criminal activity, or other "objective" criteria, but because of a racialized belief that crack is more of a problem than other drugs, despite the lack of evidence in Seattle to support this claim (Beckett et al., 2005, 2006). Because decreasing the impact prior records have on the production of cumulative racial disparities in sentencing begins with decreasing racial disparities in prior records, jurisdictions will need to end anti-gang initiatives, stop-and-frisk policies, hot spot policing, and policies that prioritize police focus on Black crime, such as Black drug markets. Ending these policies will decrease arrests, especially of Blacks and Latinos, and thus will decrease racial disparities in prior records.

Moreover, courts should not be considering prior records when making criminal processing decisions and instead should sentence all defendants as defendants with no prior records. Carodine (2009) argued that the use of prior records in court is a:

[D]eeply entrenched evidentiary rule that allows prosecutors to impeach the credibility of criminal defendants with their prior convictions. . . . [Moreover, p]rior convictions fit the classic definition of hearsay. The rule that provides for their admissibility exists as an exception to the rule against hearsay only because convictions are deemed inherently reliable. The presumption of reliability stems from the fact that the convictions are pronouncements from other courts. (521)

Evidence of racial disparities in both arrests and criminal processing decisions undermines the presumption that prior arrests and prior convictions are in fact race neutral and nondiscriminatory, and exposes prior records as containing within them crystalized discrimination (Beckett et al., 2005; Free, 2001; Spohn, 2000). When courts make either release decisions or sentencing decisions based, in small or large part, on prior records, they are promulgating the accumulation of discrimination. Accordingly, jurisdictions should eliminate the consideration of prior records, including prior incarcerations, during criminal processing decisions and process all defendants like those with no prior records. Moving forward, Congress and state legislatures should eliminate the legality of the consideration of prior records during all criminal processing decisions.

### **Hired Lawyers: Fund Public Defenders**

Wooldredge et al. (2015) find that one mechanism through which cumulative racial disparities in sentencing are produced is hired versus appointed lawyers. More particularly, the study finds that Black disadvantage in odds of being detained pretrial or of being given a nonsuspended prison sentence is associated with having a court-appointed lawyer as opposed to a hired lawyer. Although several studies have considered the impact of having a public versus private attorney (for example, Freiburger et al., 2010; Sutton, 2013), few studies have considered the impact of a hired versus court-appointed lawyer. The difference in this split is key. Based on Wooldredge et al.'s findings, the clear policy recommendation is to improve funding for public defenders' offices. Because not all defendants can hire their own lawyers, and in order to help lower the crushing caseloads under which public defenders currently labor, jurisdictions can better fund public defender offices. This will help to attenuate the difference in outcomes between defendants with appointed lawyers and those with hired lawyers and, thus, mitigate the racial disparities that this outcome produces. The funding for this reform can be tied to the money jurisdictions will save by limiting their pretrial detentions.

### **Conclusion**

Wooldredge et al. (2015) show us three mechanisms that produce racial disparities in sentencing outcomes: racial disparities in bond amounts, the consideration of prior records during criminal processing, and hiring a private lawyer. In doing so, Wooldredge et al. point the way toward policy changes that can alleviate both the size and disparity of the carceral

state. We are living at a time when, in the United States, 2.4 million people are in prisons or jails and 14 million people have felony convictions; two thirds of the people caught in this carceral net are people of color. Given this stark reality, it is imperative that jurisdictions take action and adopt these policy changes.

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# EDITORIAL INTRODUCTION

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## POLICE ENCOUNTERS WITH PEOPLE WITH MENTAL ILLNESS

### Police Encounters with People with Mental Illness

#### Use of Force, Injuries, and Perceptions of Dangerousness

**Robin S. Engel**

*University of Cincinnati*

One of the leading concerns among law enforcement agencies in this country is the appropriate handling of persons with mental disorders. The perceived rise in police contacts with persons with mental illness has been documented throughout the media, and the problems associated with police encounters with this population are well described. Proposed realistic solutions, however, are fleeting. Further compounding the problem is a shameful lack of resources for the appropriate care of those with mental disorders that limit police alternatives for handling these types of encounters. There is also limited research with very little quality empirical evidence regarding the frequency, quality, and outcomes of police interactions with persons with mental disorders or those individuals that police encounter who are experiencing acute mental health symptoms. As a result, the research community is poorly positioned to assist in the development of appropriate strategies to alleviate these well-known problems.

Our first step toward identifying evidence-based solutions must be to develop this body of evidence further. Specifically, we need well-designed and appropriately implemented research studies that examine multiple aspects of police encounters with persons with mental disorders. In the research study that follows, Melissa Morabito and Kelly Socia (2015, this issue) begin to fill this gap by examining police use-of-force data involving both mentally disordered and non-mentally disordered suspects. Specifically, this research article and the accompanying policy essays lay the framework for examining an infrequent, yet critical, aspect of police-citizen encounters: police use of force. Morabito and Socia also present research findings that begin the conversation about the perceived dangerousness of persons with mental disorders, and how that perception plays out during police-citizen encounters.

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Morabito and Socia examine whether suspects' mental illness increases the likelihood of injury (for officers and suspects) during police–citizen encounters involving the use of force. Using police use-of-force data collected by the Portland, Oregon Police Bureau from 2008 to 2011, they demonstrate the following:

- 1) Overall, 11.5% of reported uses of force involved mentally ill suspects.
- 2) Of the use of force encounters involving mentally ill suspects, 12% resulted in an injury to the officer and 28% resulted in an injury to the suspect, compared with 7% and 18% of encounters with non–mentally ill suspects, respectively.
- 3) Despite the higher percentage of encounters with mentally disordered suspects that resulted in injuries, after controlling for other demographic and situational factors, *mental illness alone was not a significant predictor of officer or suspect injuries.*
- 4) Encounters with suspects under the influence of drugs or alcohol—regardless of mental health status—were more likely to result in suspect injuries, and encounters with mentally disordered persons under the influence were more likely to result in officer injuries. Furthermore, use-of-force encounters with suspects who were mentally ill were more likely to involve substance use (44%) compared with encounters with non–mentally ill suspects (38%).
- 5) Although not emphasized by Morabito and Socia, use-of-force encounters with mentally ill suspects were also significantly more likely to involve suspect resistance (74%) compared with encounters with non–mentally ill suspects (47%); likewise, suspect resistance significantly predicted injuries.

Morabito and Socia (2015) conclude that police perceptions of their encounters with persons with mental illness as especially dangerous are likely unwarranted; rather, it is other predictors such as substance use (and I would add suspect resistance) that are better indicators of the likelihood of injury to officers and suspects than mental illness. Based on their interpretation of these findings, they suggest that inaccurate perceptions of dangerousness of mentally disordered persons might unnecessarily result in a stigmatization of this population that limits their access to needed services. They recommend that police agencies and policy makers give much greater consideration to collecting more comprehensive data regarding police–citizen encounters with mentally disordered persons.

Morabito and Socia (2015) also carefully document the limitations of their research, including the unknown base rate of police use of force for mentally ill compared with non–mentally ill suspects (i.e., are mentally ill suspects more likely to have force used against them to begin with?). Likewise, they acknowledge the limitations associated with using police perceptions to measure all of the variables in the analyses (including suspects' mental illness, substance use, resistance, etc.). The authors also note that because all Portland, Oregon Police Officers received Crisis Intervention Team (CIT) training, it is impossible to determine whether those officers without specialized training would react differently during

police–citizen encounters involving persons with mental disorders, or even whether they would be better able to identify persons with mental disorders. The question for the research community is whether these data limitations represent fatal flaws within this study that hamper its value. I argue that these are not fatal limitations; rather, this research provides interesting and important new insights into police encounters involving the use of force and the related issues of the perceptions of dangerousness of mentally disordered persons. The findings, however, must be interpreted in context, and therefore, the core question remains of whether police encounters with mentally ill suspects are more dangerous.

The two policy essays that follow this article—Alpert (2015, this issue) and Robertson (2015, this issue)—both consider the limitations of this study, and they question the implications for interpretations of the findings, generalizability, and future directions for research. Both essays recognize the study’s strengths, but they reiterate the need for additional research efforts. Although the policy essay authors vary somewhat on their optimism regarding the research community’s ability to conduct such research, they both recognize its importance to improving our understanding of police encounters with mentally disordered persons.

In the first essay, Geoffrey Alpert (2015) raises important issues surrounding the measurement of mental illness, and he concludes that the limitations involved in this research area have resulted in a “methodological conundrum” that cannot be solved. Alpert argues that it is important to qualify and understand Morabito and Socia’s (2015) findings better given his concerns regarding the use of measurements of mental illness, substance use, and injuries that rely on officers’ perceptions. He argues that no research has examined whether officers are consistent in their coding decisions on these critical variables, and he concludes that absent such research, “there is no convincing information that education has more of an influence than a gut reaction or conjecture.” In particular, Alpert raises concerns about police perceptions of mental illness, which he suggests might simply become a “ticked box” that influence researchers’ findings without a clear understanding of the tactical decisions that impact police–suspect encounters based on officers’ perceptions of mental illness.

Yet I believe that Alpert’s (2015) concerns are exactly why using a measure of officers’ perceptions of mental illness is likely better than any other measure, including third-party or professional clinical diagnosis. If we believe, as Alpert argues, that officers change their tactical approach during encounters based on the way they interpret citizens’ behaviors, and specifically that perceptions of mental disorder may alter officers’ approaches to these situations, then officers’ perceptions of mental illness are precisely the measurement we need to understand best the outcomes of use-of-force situations involving suspects where officers have made this value judgment. As argued by Engel and Silver (2001: 236), “a key mediating variable in the criminalization hypothesis is officers’ perceptions of mental disorder, regardless of whether mental disorder is present in a clinical sense. If the goal is to understand officers’ decision making, then officers’ perceptions of mental disorder are more relevant than are classifications based on clinical criteria.” Alpert, however, questions

whether officers are competent at identifying persons with mental illness and argues that this decision has far-reaching consequences that need to be given greater consideration.

The real value in Alpert's (2015) critique is the reminder that officers likely do change their tactical approach based on these perceptions, and persons that officers perceive as having mental disorders may be treated differently as a result. We need to improve our understanding of what these differences in tactics might look like, if they exist, and of what will improve officers' abilities to identify mental illness accurately. It is in this critical area of interpretation and decision making that future research could be the most fruitful for changes to training, policies, and procedures involving encounters with mentally disordered persons.

In the second policy essay, Allison Robertson (2015) provides readers with a review of additional research that is highly relevant to the discussion regarding persons with mental disorders and related perceptions of dangerousness. She carefully places Morabito and Socia's (2015) research findings into a larger context of research examining offending and violence patterns of persons with mental disorders. She also raises the need for specific changes to policies and additional training regarding prevention-oriented policies and de-escalation during encounters with mentally disordered persons. The crux of her response, however, is focused on the importance of understanding these new research findings within the context of CIT.

Although Robertson (2015) reiterates Morabito and Socia's (2015) acknowledgment that a comparison of CIT- and non-CIT-trained officers is beyond the scope of their research, she notes the importance of conducting similar research within agencies with both CIT- and non-CIT-trained officers to examine the differences. Indeed, conducting a randomized controlled trial would seem to be an ideal research design for this type of research question. Note, however, that the use of force is a relatively rare event, and further parceling that event into mentally disordered versus non-mentally disordered suspects, as well as CIT- versus non-CIT-trained officers, would eliminate most police departments from conducting such research given the number of encounters that would have to be observed or recorded. And so the impact of CIT training for this particular research question will likely remain unknown; however, examination of the impact of CIT training on all encounters with mentally disordered persons (regardless of the use of force) might be an important, and doable, starting point.

Of final note, although Morabito and Socia (2015) conclude that use-of-force encounters involving persons with mental illness are less dangerous to officers than those involving non-mentally disordered citizens, this actually remains a matter of interpretation. Portland Police Bureau's use-of-force encounters with mentally disordered persons were more likely to result in injuries to officers and suspects; Morabito and Socia's research suggests that the increased frequency of injuries was not a result of mental illness alone. But mentally disordered persons were more likely to show resistance and more likely to be under the influence of drugs/alcohol, both of which increase the likelihood of injuries. Therefore, it



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is not unreasonable for officers to perceive encounters with mentally ill persons to be more dangerous. In reality, these encounters are more likely to involve injuries, but the important question is *why* are these encounters more dangerous? Morabito and Socia's research suggests it is more complicated than considering just mental disorder alone because other factors that influence dangerousness may possibly interact with mental disorder. And therefore, the implications for police policies, training, and practice are likely even more complex than we may have originally thought.

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# Is Dangerousness a Myth? Injuries and Police Encounters with People with Mental Illnesses

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## **Research Summary**

*This study examined all “use-of-force” reports collected by the Portland Police Bureau in Portland, Oregon, between 2008 and 2011, to determine whether their encounters with people with mental illnesses are more likely to result in injury to officers or subjects when force is used. Although several factors significantly predicted the likelihood of injury to either subjects or officers, mental illness was not one of them.*

## **Policy Implications**

*Police consider interactions with people with mental illnesses to be extremely dangerous (Margarita, 1980). Our results question the accuracy of this belief. As such, this “dangerousness” assertion may result in unnecessary stigmatization that may prevent people with mental illnesses from accessing needed services (cf. Corrigan et al., 2005) as witnesses or victims of crime. Policies that reduce stigma may help increase police effectiveness. Furthermore, efforts should be made to increase the availability and accuracy of data on this issue.*

## **Keywords**

*mental health, police, use of force*

**P**olice interactions with people with mental illnesses have long been considered among the most dangerous calls for service to which officers must respond

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(Margarita, 1980). Because the police have low-level, yet consistent, contact with people with mental illnesses—approximately 6–7% of all public contacts (Cordner, 2006; Engel and Silver, 2001; Teplin, 1984)—interactions with this population have been represented as a major threat to police officer safety (see Ruiz and Miller, 2004; Watson, Corrigan, and Ottati, 2004). The danger to police officers represented by people with mental illness has largely been based on the perceptions of law enforcement officers themselves.

Yet, existing empirical evidence does not support this perception. Data collected as part of the Uniform Crime Reports indicate that few, if any, injuries to police officers result from encounters with people with mental illnesses (Federal Bureau of Investigation, 2009). Additionally, evidence suggests that the crimes committed by people with mental illnesses tend not to be predominantly crimes of violence (Draine et al., 2002; Fisher et al., 2006). Rather, people with mental illnesses are involved in criminal activity similar to those perpetrated by their peers of the same socioeconomic status. For example, Peterson et al. (2010) found no distinct difference between the offending patterns of those with serious mental illnesses and their peers without diagnoses. Like other offenders, people with mental illnesses engage in crimes involving property and drugs, crimes that also are unlikely to end in instrumental violence (Draine et al., 2002; Fisher et al., 2006). More generally, recent reports estimated that only approximately 4% of overall violence in the United States can be attributed to those with mental illness (Friedman, 2014), suggesting that most people with mental illnesses are not violent or dangerous, except perhaps to themselves (Swanson et al., 2014).

The type of criminal activity, limited propensity for violence, and the relatively small proportion of police contacts with this population casts doubt on the oft-repeated assertion (Watson et al., 2004) that encounters with people with mental illness are particularly dangerous for the police (Ruiz and Miller, 2004). Furthermore, a conflicting viewpoint has emerged that when police and people with mental illness interact, *people with mental illnesses* are much more likely to be the ones who are injured in the encounters (Cordner, 2006). However, given the limited use of force by the police generally (e.g., Durose and Langton, 2013; Hickman, Piquero, and Garner, 2008) and the relatively small proportion of encounters involving people with mental illnesses (Engel and Silver, 2001; Teplin, 1984), there is a potential third viewpoint: Resulting injury would be rare for *all* parties involved and would be unrelated to mental illness. Thus far, researchers have only weighed in with limited empirical findings largely resulting from a lack of available data about mental illness, injury, and the use of force overall.

To address this limited knowledge and to understand more clearly the outcomes of police interactions with people with mental illnesses, this study examines “use-of-force” reports collected by the Portland, Oregon, Police Bureau, which had universal Crisis Intervention Team (CIT) training for the period under study. First, the literature detailing encounters between the police and people with mental illnesses is explored. Next, we analyze the population of all use-of-force incidents from 2008 to 2011, as documented by

the aforementioned use-of-force reports. From these data, we examine the likelihood and correlates of injury for both police and subjects during encounters when force is used by the police. We specifically isolate and discuss the effects of mental illness on the likelihood of injury. Our findings suggest that substance abuse, not mental illness alone, is correlated with injuries for subjects or police. We conclude with a discussion on the public policy and practice implications.

### **Police and People with Mental Illnesses**

In the 1960s, the increased costs of mental health care in locked wards and advances in medical care resulted in the demise of the state hospital (Richter, 2007) and the subsequent deinstitutionalization of people with mental illnesses. After deinstitutionalization, people with mental illnesses returned to the community without adequate access to services or resources to treat their illnesses (Starr, 1982). Lacking access to adequate care or basic resources, they ended up on the streets and engaged in troublesome activities, and many of these individuals entered the criminal justice system (Teplin, 1983). The overrepresentation of people with mental illnesses in the criminal justice system became a problem that was identified easily by researchers, advocates, and practitioners (cf. Lamb and Bachrach, 2001; Lamb and Weinberger, 2013; Swank and Winer, 1976).

Researchers and advocates have argued that the “criminalization” of this population is the partial fault of the police, stemming either from their ignorance and discrimination against people with mental illnesses (Teplin, 1984) or from “mercy booking,” which is the use of arrest to provide for safety and shelter (Lamb, Weinberger, and Gross, 2004). Furthermore, from this perspective, deinstitutionalization forced the criminal justice system, rather than the mental health system, to assume the responsibility of controlling the (sometimes deviant and/or aggressive) behavior of people with mental illnesses (Abramson, 1972). In turn, this led to their disproportionate involvement with the criminal justice system. Unsurprisingly, since the late 1960s, the police have been criticized both for ignoring and for criminalizing acute symptoms that can be associated with illness and, therefore, serving as the gateway for unnecessary involvement of people with mental illnesses in the criminal justice system (Abramson, 1972).

This view was supported by research conducted by Teplin (1984). She examined the outcomes of police contacts with people with mental illnesses by using clinically trained observers in two busy precincts in Chicago. She found that police were more likely to use formal methods of social control to manage incidents with people with mental illnesses—specifically, they arrested people with mental illnesses more frequently than people without such illnesses. Teplin (1984) suggested that aggressive and “disrespectful” behavior on the part of people with mental illnesses is misunderstood by police officers and is treated criminally, rather than medically. People with mental illnesses who are symptomatic may be more likely to be engaging in seemingly deviant behavior, specifically those actions deemed disrespectful or hostile (Novak and Engel, 2005). Accordingly, police believe this deviant

behavior to be criminal and, therefore, will use a formal response—the use of force and/or arrest. The police may be more likely to use force to gain compliance in these situations; however, evidence suggests that arrest of this population is less likely than that of the general public (Novak and Engel, 2005).

Yet the exact causes for the disproportionate use of formal sanctions were questioned as researchers began to call attention to the problems with these prior studies. In particular, studies of the arrest of people with mental illnesses often excluded incident-level factors, such as the severity of the offense, the role of substance abuse or community priorities, and resulting police agency policies that typically inform police research (Engel and Silver, 2001). These factors are known to affect police response—specifically influencing the arrest decision (Morabito, 2007). Without controlling for these factors, it cannot be clear that mental illness is *itself* responsible for the criminal justice involvement of people with mental illnesses.

Furthermore, the use of clinically trained observers represents a problem in trying to isolate the police response to this population. Although police officers can generally identify mental illness with limited information (Engel and Silver, 2001), they are not clinicians and may not be able to identify the full range of symptoms that could accompany mental illness. It is possible that the symptoms that are noticeable to clinically trained observers may elude police officers, who might treat such manifestations as aggression or resistance given the context of the incident. For example, if a subject has perpetrated a violent crime, then police would be expected to treat aggressive behavior seriously. As such, it is questionable whether using this approach allows researchers to ascertain police motives for arrest. Also, it is not economically feasible to use this approach on a larger scale, given the limited proportion of contacts that involve this population (Morabito and Wilson, 2015).

Additional evidence emerged from a study conducted by Engel and Silver in 2001 that questioned Teplin's (1984) findings. They found that in observations of police response to subjects, mental illness may be a protective factor against arrest (Engel and Silver, 2001). As these encounters infrequently end in arrest (Engel and Silver, 2001; Novak and Engel, 2005), many of these situations are resolved informally. With informal resolution, the end results are rarely recorded, despite such information being important to both practitioners and researchers. Yet, informal resolution informs our understanding of these encounters. Because police officers have limited discretion when responding to crimes of violence or violent individuals (Morabito, 2007), the findings of Engel and Silver (2001) suggest that most police calls for service involving people with mental illness may not disproportionately involve dangerous behavior. Despite the growing body of evidence suggesting that mental illness does not necessarily *cause* violence, the perception remains that people with serious mental illnesses, such as schizophrenia, are more dangerous to officers than the general population (cf. Ruiz and Miller, 2004; Watson et al., 2004).

### *Officer Safety*

Few studies of officer safety have identified the factors that predict nonlethal injuries (Mesloh, Henych, and Wolf, 2008). According to Federal Bureau of Investigation data, the largest percentage of reported assaults comes from officers responding to disturbance calls (Federal Bureau of Investigation, 2009), which encompass a wide range of crimes and subjects but typically include bar fights and domestic disturbances—a relatively large category. Currently, the Law Enforcement Officers Killed and Assaulted supplement to the Uniform Crime Reports is the most comprehensive collection of data regarding officer deaths and injuries in the United States. However, the International Association of Chiefs of Police suggests that the reported number of assaults on officers likely underestimates the frequency of actual assaults and injuries experienced (International Association of Chiefs of Police and Bureau of Justice Assistance, 2014).

Furthermore, surprisingly little research has detailed the police injuries that are sustained during encounters with people with mental illnesses (Kerr, Morabito, and Watson, 2010). It is widely recognized that the injuries sustained by police officers during encounters with subjects are rare and are not particularly serious when they do occur (Kaminski and Sorensen, 1995); yet studies examining the link between mental illness and injury have been scarce. Furthermore, much of the existing criminal justice literature has focused on police *perceptions* of likelihood of injury (Ruiz and Miller, 2004), which informs us on the *expectations* of the police in these encounters but is not an accurate depiction of what actually happens in an encounter. The likelihood of injury remains an important area of research, however, because officer injury is costly to agencies and localities in the form of lost days of work by injured officers, rehabilitation expenses, and overtime payments to other officers who must cover shifts (International Association of Chiefs of Police and Bureau of Justice Assistance, 2014).

### *Injuries to People with Mental Illness*

Researchers and practitioners have long been concerned with the relationships among police use of force, mental illness, and resulting injury for people involved in encounters with the police (Council of State Governments, 2002). Yet the use of force is rare in all police contacts with the public, as most subjects readily comply with requests from officers and do not resist (e.g., Durose and Langton, 2013; Hickman et al., 2008). In practice, officer tactics tend to be concentrated at the lower end of the continuum of force, infrequently involving the use of weapons (Adams, 2004). When force *is* used, it is usually in response to one of the following behaviors identified in the literature: resisting officer requests, acting disrespectful toward officers, attacking officers, possessing a weapon, or running away from the police, but not *mental illness* (cf. Garner and Maxwell, 2002; Hickman et al., 2008; Jacobs and O'Brien, 1998; Kaminski, Digiovanni, and Downs, 2004; Terrill and Mastrofski, 2002). Given the low levels of force typically used by police officers, the subsequent injuries that

result from all encounters are minor, described mostly as abrasions and bruises (Alpert and Dunham, 2004).

This begs the following question: Are people with mental illnesses more at risk for injury resulting from the police use of force? If aggressive behavior is more likely to elicit force (cf. Terrill and Mastrofski, 2002), then it is possible that people with mental illnesses who are symptomatic could be more at risk to be involved in violent encounters with the police. The symptoms of mental illness may be misconstrued as aggression toward the police, thus, increasing the likelihood of force being used.

The literature, however, has offered a conflicting picture of the behavior of people with mental illnesses in these encounters (cf. Engel and Silver, 2001; Johnson, 2011; Kesic, Thomas, and Ogloff, 2013). Johnson (2011) suggested that people with mental illnesses who come in contact with the police tend to behave more aggressively than their peers without diagnoses, and they are frequently under the influence of illegal drugs and alcohol. In contrast, Kesic and colleagues (2013) found that the people in their sample with mental illnesses who encountered the police were less likely to be under the influence of alcohol or to engage in aggressive behavior compared with those without signs of mental illness. Furthermore, mental illness is not a static condition, which means that people with diagnoses are not symptomatic all of the time (Morabito and Wilson, 2015). Thus, it is possible that a person could be known to the police as having a mental illness but might not manifest any symptoms during an encounter.

This recognition can be bolstered by the CIT program. CIT is a police-based, pre-booking approach with officers who are trained to provide first-line response to people with mental illnesses. Officers are trained to identify symptoms of mental illness, as well as act as liaisons to the mental health system (Borum et al., 1998). The intervention is based on a model developed by the Memphis Police Department (Council of State Governments, 2002), and it has been hypothesized to improve officers' abilities to interact more safely with persons with mental illness, including reductions in the use of force and subsequent injury to both police and subjects.<sup>1</sup>

Although not specific to CIT, several studies have attempted to tease out the relationship between mental illness and the use of force. These attempts have resulted in conflicting findings about the use of force in police encounters with people with mental illnesses (cf. Alpert and Dunham, 2004; Kaminski et al., 2004; Kerr et al., 2010; Kesic et al., 2013). What is clear is that police encounters with *impaired* people may be more likely to involve violence (Kaminski et al., 2004). This finding, however, does not necessarily translate to dangerous encounters between the police and people with mental illnesses.

Unfortunately, scholars tend to lump people with mental illnesses in with people who are impaired by drugs and alcohol (Alpert, Dunham, and MacDonald, 2004). Accordingly,

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1. For a complete discussion of the elements and hypothesized outcomes of CIT, see Watson, Morabito, Draine, and Ottati (2008).



it is not clear that mental illness, as opposed to substance use, is responsible for violence in police encounters or whether it is some combination of the two (Swartz et al., 1998). When the type of impairment is differentiated, evidence is mixed as to the relationship between use of force and mental illness (cf. Kaminski et al., 2004; Terrill and Mastrofski, 2002). Johnson (2011) used data originally collected by Alpert and Dunham (2000) in Eugene and Springfield, Oregon. He found that mental instability was unrelated to police use of force (Johnson, 2011). Yet evidence suggests that people with co-occurring disorders (mental illness and substance abuse) have greater involvement with the criminal justice system than people with mental illnesses alone—both as assailants and as victims (Abram and Teplin, 1991; Swartz and Lurigio, 2007). As Abram and Teplin (1991) noted, the placements available for people with co-occurring disorders are few and far between, and they may therefore be arrested as a way to manage their illnesses. Regardless of the reason, the presence of a co-occurring disorder increases an individual's likelihood for both violent victimization (Hiday et al., 1999) and arrest (Swartz and Lurigio, 2007). Both of these scenarios will result in interaction with the police. Although the pathways to the criminal justice system are clear, we have little knowledge about how these encounters are resolved by the police or by people with these co-occurring disorders.

Even less is known about the injuries to both police and people with mental illnesses, with or without co-occurring disorders that result from their encounters. Kerr and colleagues (2010) examined the proportion of officer–subject encounters involving a person with mental illness in which an injury occurred in four districts in Chicago, Illinois. They found that in most encounters that required the use of force, physical resistance was the only significant predictor of the proportion of calls with injuries (Kerr et al., 2010). This research, however, used only a subsample of incidents involving people with mental illnesses; thus, the results did not offer insight into the likelihood of injury in encounters with the general population. In a study of the Victoria Police in Australia, Kesic et al. (2013) found that almost half of those subjects who seemed to have a mental illness were recorded as having been injured by the police. However, most of the recorded injuries were of low severity.

In short, data are scarce because the use of force is a rare occurrence in police encounters with the public (e.g., Durose and Langton, 2013; Hickman et al., 2008). Although information on police use of force is collected regularly by police agencies, the data reported are inconsistent and often not readily available (Garner et al., 1995). Furthermore, these data rarely contain measures of mental illness. The existing research that has focused on use of force in encounters with people with mental illnesses has largely excluded information about subsequent injuries. This is likely because injuries are a relatively rare occurrence in encounters with people with mental illnesses, for both subjects and the police, even when force is used (Kerr et al., 2010; Kesic et al., 2013; Ruiz and Miller, 2004).

The available research points to a void in the literature regarding the effect of mental illness on the outcomes of use-of-force incidents with the police. If mental illness is associated

with increased dangerousness, then we should expect that the incidents involving use of force between the police and people with mental illnesses should sustain more injuries than in similar encounters between the police and subjects without mental illnesses. To understand more clearly the relationship between mental illness and injuries, we examine use-of-force data collected from Portland, Oregon, to answer the following research question: Does mental illness increase the likelihood of injury during police–subject encounters involving force for either officers or subjects?

### Data and Methods

The data used in the study include all documented use-of-force cases recorded by the Portland Police Bureau from 2008 to 2011. The Portland Police Bureau serves a population of approximately 575,000 over an area of 146.6 square miles, with roughly 980 sworn members and 295 nonsworn members (Portland Police Bureau, 2011). The Portland Police Bureau implemented CIT in 2007, and by 2008, the department had moved from the specialist CIT model developed by the Memphis Police Department to universal training. At this time, CIT training was required in the training academy (12 hours in the state preservice academy and 28 hours in the advanced Portland preservice academy).<sup>2</sup>

Whenever an officer uses force, an officer is injured, or a subject is injured, Portland Police Bureau officers are required to complete a “use-of-force” form. This form documents various details about the encounter, including the precinct, event conditions, subject behavior, and officer actions. The use-of-force report was implemented in 2004 to count, report, and track the various uses of force by officers during the course of their duties for training and policy purposes. All cases between 2008 and 2011 in which such a form was filled out were included in the initial data set, accounting for 7,327 incidences. Of these, 128 cases were removed because a subject was injured but no force was used by the officer, 105 cases were removed because subject race and sex data were missing, and 963 cases were excluded because data regarding subject injury extent or timing were missing. This resulted in a final data set of 6,131 incidences.<sup>3</sup>

The Portland Police Bureau defined force as a physical or mechanical intervention used by a police officer to defend, control, overpower, restrain, or overcome the resistance of an individual. This included the use of any of the following force options: control holds causing injury, takedowns, hobbling, use of hands or feet, baton, pepper spray, Taser, or bean bag rounds. Force also includes the pointing of a firearm, even if not discharged. Escort holds and handcuffing are not considered to be a use of force unless physical or mechanical intervention is applied against resistance (Portland Police Bureau, 2009).

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2. The Portland Police Bureau made additional changes to their CIT training and service provision in 2012. These changes are not detailed because they occurred after the study period.

3. Any encounters that resulted in injury where force was not used have been excluded from the sample.

TABLE 1

## Officer and Subject Injury Distribution

Injury Type	Cases Involving Injury			
	To Officer		To Subject	
	<i>N</i>	%	<i>N</i>	%
None	5,676	92.58	4,968	81.03
Any	455	7.42	1,163	18.97
Bruises	136	2.22	294	4.80
Abrasions	260	4.24	943	15.38
Lacerations	49	0.80	405	6.61
Broken bones	6	0.10	22	0.36
Other injuries	125	2.04	249	4.06
<b>Total</b>	<b>6,131</b>	<b>—</b>	<b>6,131</b>	<b>—</b>

*Notes.* Because of the potential of multiple injury types per encounter, individual injury outcomes do not add to 100%. In instances where a subject was injured *prior* to the encounter with police ( $N = 331$ ), but sustained no injuries either during the encounter or while in custody, the subject's injuries were considered to be "None."

### Dependent Variables

We analyze two dependent variables separately: (a) any officer injury and (b) any subject injury. Any officer injury is measured as a dichotomy, with 0 indicating no injury to the officer during the encounter with the subject, and 1 indicating any injury during the encounter. Note that "any injury" could include any of the following: bruises, abrasions, lacerations, broken bones, or other injuries (e.g., sprained ankle or concussion). Any subject injury is measured dichotomously, with 0 indicating no injury to the subject during the encounter with the officer, and 1 indicating any injury during the encounter. Injury definitions were identical to those used for officer injury. Note that only injuries occurring *during* the police encounter were considered. As such, subjects whose only injuries were recorded as occurring *prior* to the encounter with police were categorized as having no injuries for the purposes of the analyses. Subjects who were injured both prior to the encounter *and* during the encounter were classified as having injuries for the purposes of the analyses.

The distribution of specific officer and subject injuries is provided in Table 1. The distribution of these injuries indicates that most incidents involve no injury to either the officer or the subject. Subjects are, however, more likely to sustain any injury than officers. Specifically, in 7.4% of incidents, some type of officer injury was reported, whereas in 19.0% of incidents, there was a reported injury to the subject. Furthermore, consistent with the use-of-force literature, when injuries *do* occur to either officer or subject, they rarely involve the most serious injuries (i.e., broken bones) and instead involve one or more less serious injuries (i.e., bruises, abrasions, and lacerations) (Adams, 2004). As shown in Table 1, police and subjects receive major injuries in just 0.10% and 0.36% of use-of-force incidents, respectively.

### *Independent Variables*

The independent variables are taken directly from the data contained in the use-of-force reports. These variables include those that could influence injury (either to officers or to subjects) and that relate to perceived subject conditions, subject characteristics, and the use of force. The composition of each variable is explained in more detail later in this article. Note that all variables regarding perceived subject conditions were based on officer *perceptions* at the time of the encounter and may not necessarily match historical records, clinical diagnoses, or laboratory tests.

First, we include information about the subject's race in the model. Based on available information collected in the force report, White is coded dichotomously as White (1) or non-White (0). Subject sex also is based on information collected in the force report. Male is coded dichotomously as male (1) or female (0). Approximately 2% of cases were missing both race and sex for the subject, and these cases are excluded from the analyses.<sup>4</sup>

Next, we examine the behavioral health challenges (mental illness and substance abuse) of the subject encountered by the police. We measure the presence of mental illness by using a set of dichotomous variables indicating the absence or presence of mental illness or substance use. As such, mental illness only is a dichotomous variable that captures the officer's perception that the subject had a mental illness but was not under the influence of alcohol, drugs, or both. This definition was established by Portland Police Bureau Directive 850.20 (Portland Police Bureau, 2014):

A person may be affected by mental illness if he or she displays an inability to think rationally (e.g., delusions or hallucinations); exercise adequate control over behavior or impulses (e.g., aggressive, suicidal, homicidal, sexual); and/or take reasonable care of his or her welfare with regard to basic provisions for clothing, food, shelter, or safety.

Next, we measure substance abuse only. This is a dichotomous measure of the officer's perception that the subject was currently under the influence of alcohol, drugs, or both but *did not* have a mental illness. According to the Portland Police Bureau's *Manual of Policy and Procedure* (Portland Police Bureau, 2009), officers can recognize a person under the influence as "[d]isplaying bizarre behavior (violence, extreme strength, immunity from pain etc.) associated with drug induced psychosis/excited delirium." Finally, mental illness *and* substance use is measured dichotomously as the officer perceiving that the subject had *both* a mental illness *and* was under the influence of alcohol, drugs, or both. As such, this measures the interaction of perceived mental illness *and* substance use. Officers are instructed to use

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4. The results did not change when subjects missing a racial classification were coded as White (the modal category), as non-White, or replaced with the mean of the variable. The results also did not change when subjects missing a sex classification were coded as male (the modal category), as female, or replaced with the mean of the variable.

perceptual cues to identify the co-occurrence of substance abuse and mental illness among subjects they encounter. Because indicators for mental illness only and for substance use only are included in the models, the comparison is with cases that had no mental illness and no perceived substance use.

The second group of variables includes incident characteristics. Subject weapon is measured dichotomously as the subject having been reported to be (or perceived as being) armed with a weapon. Prior subjects assault is measured dichotomously as the subject is known to have assaulted a subjects immediately prior to or during the encounter. Officer assault is measured dichotomously as the subject having assaulted an officer during the encounter (as noted by the officer). An analysis of the bivariate correlation between officer assault and officer injury indicated that an officer being assaulted was not simply a proxy for any officer injury ( $r = .33$ ). Although this may seem counterintuitive, it can be explained both by instances in which officers were *technically* assaulted but did not experience any injuries (e.g., a subject pushing an officer) or in which officers were injured without direct subject action, such as falling during a foot chase or bruises from tackling a subject.

The next two variables are indicative of how the subject interacts with the police during the encounter. We include subject resistance as a measure of officer perception that the subject was engaged in, or indicated the intent to engage in, physical or aggressive physical resistance. This variable is measured dichotomously, and “no resistance” is the reference category. Foot chase is a dichotomous measure of whether the officer had to engage in a foot chase of the subject.

A use-of-force report must be filled out if a firearm was pointed, regardless of whether other force was used. To capture the data regarding whether force was used, pointing a firearm without physical force is measured dichotomously as the officer pointing a firearm at the subject during the encounter but *not* using any form of “physical” force. Given the “hands-off” nature of pointing a firearm (assuming no shots are fired) and the likely dichotomous nature of the injuries resulting from firearm pointing (i.e., none if shots were not fired and potentially lethal if shots were fired), that particular force option is considered separately from the other, more physical, force options. Physical force *and* pointing a firearm also was measured dichotomously as the officer using physical force *and* pointing a firearm at the subject during the encounter. Because an indicator for cases involving *only* pointing a firearm *without* physical force is included in the models, the comparison is with incidents that *only* involved the use of physical force *without* pointing a firearm.

Given the two dichotomous outcome variables (any officer injury, any subject injury), two separate logistic regression models are used to predict the likelihood of injury either to the officer or to the subject.<sup>5</sup>

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5. The model results include a column for odds ratios, which represent the change in likelihood that the dependent variable is present (1), given a one-unit change in the independent variable. When one is

**T A B L E 2**

**Descriptive Statistics**

Variable	All Cases		Non-Mental Illness Cases		Mental Illness Cases	
	Mean	SD	Mean	SD	Mean	SD
Any Officer Injury	0.07	0.50	0.07	0.25	0.12	0.32
Any Subject Injury	0.19	0.39	0.18	0.38	0.28	0.45
White Subject	0.55	0.31	0.52	0.50	0.75	0.43
Male Subject	0.89	0.32	0.90	0.31	0.85	0.36
Mental Illness and Substance Use						
No mental illness <i>and</i> no substance use	0.55	0.50	0.62	0.49	—	—
Mental illness only	0.06	0.25	—	—	0.56	0.50
Substance use only	0.34	0.47	0.38	0.49	—	—
Mental illness <i>and</i> substance use	0.05	0.22	—	—	0.44	0.50
Subject Weapon	0.33	0.32	0.32	0.47	0.38	0.48
Prior Subjects Assault	0.11	0.21	0.11	0.32	0.11	0.31
Officer Assault	0.05	0.50	0.04	0.20	0.08	0.28
Subject Resistance	0.50	0.34	0.47	0.50	0.74	0.44
Foot Chase	0.13	0.50	0.14	0.34	0.06	0.24
Physical Force and Pointing Firearm						
Physical force only	0.50	0.50	0.46	0.50	0.75	0.43
Pointing firearm only	0.46	0.50	0.49	0.50	0.19	0.39
Physical force <i>and</i> pointing firearm	0.04	0.21	0.04	0.20	0.06	0.24
<i>N</i> (All Variables)	6,131		5,425		706	

*Notes.* Given their dichotomous nature, the minimum value for all variables in both models is 0, and the maximum value for all variables in both models is 1. Standard Deviation abbreviated as SD. Table excludes cases with missing data on a subject's sex, race, or injuries, or an officer's injuries. Differences between Non-Mental Illness and Mental Illness cases are *all* significant ( $p < .05$ ) with the exception of subjects assault ( $p = .70$ ).

**Results**

Descriptive statistics of each variable are provided in Table 2. Of interest is that a subject was perceived as having a mental illness without substance use approximately 6% of the time and as having a mental illness in conjunction with substance use approximately 5% of the time (Table 2). Subject injury occurred approximately 19% of the time, whereas officer injury occurred approximately 7% of the time (Table 2).

Variable differences between cases involving the perception of a mental illness and those without also are included in Table 2. Note that *t* tests indicate significant ( $p < .05$ ) differences between the two groups for all variables except subjects assault. For example, people with mental illnesses involved in force encounters are more likely to be White,

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subtracted from the odds ratio coefficient and the result is multiplied by 100, it represents the percentage change in the likelihood of the dependent variable being present (1), given a one-unit change in the independent variable.

TABLE 3

## Officer and Subject Injury Models

Variable	Any Officer Injury			Any Subject Injury		
	B	Standard Error	Odds Ratio	B	Standard Error	Odds Ratio
White subject	−0.02	.11	0.98	0.29***	.08	1.33
Male subject	0.06	.18	1.06	0.49***	.13	1.63
Mental illness only <sup>a</sup>	−0.18	.21	0.83	0.08	.15	1.09
Substance use only <sup>a</sup>	−0.26*	.12	0.77	0.45***	.09	1.57
Mental illness <i>and</i> substance use <sup>a</sup>	0.12*	.20	1.13	0.50***	.15	1.65
Subject weapon	−0.14	.15	1.13	0.34***	.10	1.40
Prior subjects assault	−0.47**	.17	0.62	−0.09	.11	.91
Officer assault	1.99***	.14	7.35	0.39**	.14	1.48
Subject resistance	0.74***	.19	2.09	0.61***	.11	1.84
Foot chase	0.51***	.13	1.66	0.47***	.10	1.59
Pointing firearm only <sup>b</sup>	−2.52***	.33	0.08	−2.78***	.18	0.06
Physical force <i>and</i> pointing firearm <sup>b</sup>	−0.61*	.27	0.54	−0.40*	.16	0.67
Any subject injury	0.89***	.11	2.43	—	—	—
Any officer injury	—	—	—	0.88***	.11	2.42
Constant	−3.08***	.26	0.05	−2.36***	.18	0.09
N		6,131			6,131	
Degrees of freedom		13			13	
LR chi <sup>2</sup> (13)		830.23***			1,494.02***	
Pseudo R <sup>2</sup>		.26			.25	

<sup>a</sup>Comparison is no perceived mental illness *and* no perceived substance use.

<sup>b</sup>Comparison is the use of physical force only (*without* pointing firearm).

\* $p < .05$ . \*\* $p < .01$ . \*\*\* $p < .001$ .

female, armed, and resist arrest than their peers without mental illnesses. Although the use-of-force cases involving people with mental illnesses are significantly more likely to result in injury for both the officer and the subject, without controlling for other variables, the exact relationship between mental illness and injury is unclear.

To address our research question, we employ two multivariate logistic regression models predicting (a) any officer injury and (b) any subject injury. As shown in Table 3, these models include the same predictor variables, with the exception of the inclusion of subject injury in the model predicting officer injury and officer injury included as a predictor of subject injury. An analysis of uncentered variance inflation factors suggested that multicollinearity is not a concern for either model. The results of each model are explained in more detail later in this article.<sup>6</sup> The likelihood ratio tests for both the officer injury and the subject injury models were significant ( $p < .001$ ). Note that in the officer injury model, subject

6. Model results report nonrobust standard errors, but the results did not change when robust standard errors were used (results not shown).

injury was included as an independent variable, and in the subject injury model, officer injury also was included.

### *Officer Injury*

The officer injury models predict the likelihood that an officer received any injury during the encounter with the subject. As shown in Table 3, neither the race nor the sex of the subject significantly influenced the likelihood of officer injury.<sup>7</sup> Contrary to the bivariate findings, perceived mental illness alone (without perceived substance use) did not significantly influence the likelihood of officer injury compared with subjects without both perceived mental illness and substance use. This suggests that subjects with perceived mental illnesses do not represent an increased danger to officers in terms of injury. However, perceived substance use alone (without any indication of mental illness) was associated with a 23% decrease in the likelihood of officer injury, whereas the combination of perceived mental illness *and* substance use was associated with a 13% increase in the likelihood of officer injury; both substance use alone and in combination with mental illness were significant ( $p < .05$ ).

Encounters involving an armed subject did not significantly influence the likelihood of officer injury. When the subject had assaulted a subjects prior to or during the encounter, there was a 38% reduction in the likelihood of officer injury. As expected, one of the strongest predictors of officer injury is whether the subject had assaulted an officer; in this case, there was a 635% increase in the likelihood of officer injury. Other physical actions by the subject were associated with an increased likelihood of officer injury, such as having resisted (109% increase) or being involved in a foot chase (66% increase).

The results also suggest that compared with *only* the use of physical force, when a firearm was pointed at a subject *without* the use of physical force, the likelihood of officer injury decreased by 92%. This finding is not surprising, and it indicates that force options of a more physical nature are likely to result in injured officers, whereas pointing a firearm (a nonphysical force option, assuming no shots are fired) is less likely. Interestingly, compared with only the use of physical force, the combination of physical force *and* pointing a firearm was associated with a 46% decrease in the likelihood of officer injury. An injury to the subject resulted in a 143% increase in the likelihood of officer injury.<sup>8</sup>

### *Subject Injury*

The subject injury models predict the likelihood that the subject received any injury during the encounter with the officer(s) or while in custody after the encounter. As shown in Table 3, both the race and sex of the subject significantly influenced the likelihood of subject injury

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7. As noted, an analysis of the models with missing sex and race cases recoded as either the mode category (White and male), the non-mode category (non-White and female), or the mean did not change overall interpretations (results not shown).

8. The overall results did not change when subject injury was excluded from the model (results not shown).



( $p < .001$ ). Specifically, White subjects were 33% more likely to be injured than non-White subjects, and male subjects were 63% more likely to be injured than female subjects.

Similar to the officer injury models, perceived mental illness alone (without perceived substance use) did not significantly influence the likelihood of subject injury. Again, this finding is contrary to the initial bivariate findings, and it suggests that subjects perceived by the police as having a mental illness are no more likely to be injured during a forceful encounter than subjects without (perceived) mental illness when controlling for the influence of substance use. Interestingly, compared with no indication of either mental illness or substance use, perceived substance use alone (without any indication of mental illness) was associated with a 57% increase in the likelihood of subject injury. Similarly, the combination of perceived mental illness *and* substance use was associated with a 65% increase in the likelihood of subject injury. In combination, these results suggest that subject injury is less the result of perceived mental illness and more the result of substance use.

When a subject was armed, there was a 40% increase in the likelihood of subject injury. Whether a subject had assaulted a subjects did not significantly influence the likelihood of subject injury. As subject injury was only included if it occurred during the encounter or while in custody, any injuries to the subject that occurred at the hands of other subject *prior to the encounter* (e.g., a bar fight between two patrons or a domestic assault) would not be counted as an injury for the purposes of the current study. Unsurprisingly, when a subject had assaulted an officer, there was a 48% increase in the likelihood of the subject being injured. Similar to the officer injury model, other physical actions by the subject were significantly associated with an increased likelihood of subject injury, such as having resisted (84% increase) or being involved in a foot chase (59% increase).

The results involving physical force and firearm pointing are similar to the officer injury model. Specifically, the results suggest that compared with *only* the use of physical force, when a firearm was pointed at a subject *without* the use of physical force, the likelihood of subject injury decreased by 94%. Compared with only the use of physical force, the combination of physical force *and* pointing a firearm was associated with a 33% decrease in the likelihood of subject injury. Similar to the officer injury model, an injury to the officer resulted in a 142% increase in the likelihood of subject injury.<sup>9</sup>

### *Results Summarized*

Overall, the results of the study suggest that certain situational and individual variables play an important role in whether a subject or an officer is injured during an encounter involving force. Specifically, the race and sex of a subject influenced the likelihood of subject injury but not officer injury. The variable of interest to the current study, perceived mental illness in a subject, did not significantly increase the likelihood of injury to either party when it was not paired with perceived substance use. However, substance use alone resulted

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9. The overall results did not change when officer injury was excluded from the model (results not shown).

in an increased likelihood of injury to the subject and a decreased likelihood of injury to the officer. Interestingly, encounters with subjects perceived as having *both* a mental illness and substance use increased the likelihood of injury to both the officer and the subject. Encounters with an armed subject increased the likelihood of subject injury but not of officer injury. Subject assault significantly decreased the likelihood of injury to the officer, but not to the subject, whereas officer assault increased the likelihood of injury to both parties. Resisting and chasing increased the likelihood of injury to both parties. Compared with only the use of physical force without pointing a firearm, both pointing a firearm alone and in combination with physical force decreased the likelihood of injury to both parties. Finally, injury to one party increased the likelihood of injury to the other party.

### Discussion and Policy Implications

Our findings are largely consistent with the large body of literature examining the use of force, in that injury to a subject is most likely when subjects are armed, resist police commands, assault an officer, or are involved in a foot chase (Durose and Langton, 2013; Garner and Maxwell, 2002; Hickman, Piquero, and Garner, 2008; Kaminski, Digiovanni, and Downs, 2004; Terrill and Mastrofski, 2002). Officers also are more likely to be injured in many of these same circumstances. Although this information is not new, it is confirmed by the existing police literature (cf. Garner and Maxwell, 2002; Hickman et al., 2008; Kaminski et al., 2004; Terrill and Mastrofski, 2002) and gives us confidence that our findings are representative of encounters in which police use force.

When considering the impact of subjects' mental illness on the likelihood of officer and subject injury, we found that mental illness alone is not predictive of injury for either police officers or subjects. However, the findings also show that when mental illness is co-occurring with substance use, the likelihood of injuries for both officers and subjects increases significantly. This increased likelihood in injuries is found also for subjects without a mental illness but who are under the influence of drugs or alcohol. Together, these findings suggest that it is *not* mental illness that has an impact on these encounters, but it is substance use for both subjects with mental illness and those without that affects whether officers or subjects are injured.

A few potential possibilities (or some combination of) could explain why mental illness *alone* is unrelated to injury. First, it may be that these incidents are simply not as dangerous as officers believe. Next, the effects of co-occurring substance use could be what drive dangerousness, not the effects of mental illness. An alternative explanation is that CIT training may have prepared officers to manage these encounters well, and subsequently, there are few resultant injuries to either party. This would mean that officers can recognize symptoms that are a manifestation of mental illness, and not treat subjects as though they are resistant. Unfortunately, the data did not allow us to examine the specific role CIT played in these results, and the CIT literature to date has not been conclusive (Compton et al., 2008). Yet either way, these data indicate that manifestations of mental illness are

not being addressed with police responses that result in injury, either to the police or to the subject.

As expected, subjects are more likely to be injured when they are under the influence of alcohol or drugs, either alone or in combination with perceived mental illness, possibly because this can result in behavioral unpredictability. However, this finding does not translate into an increased likelihood of injury to officers (unless co-occurring with mental illness), which could be because officers must use “more force” to apprehend these subjects because of the effects of drugs and alcohol on behavior. It should be noted that drug use is a broad category—for example, heroin or marijuana might affect behavior in a different way than crack or alcohol. Future research should try to parse out the effects of different types of drug use and their relationship to injury (of either party) in these encounters. It is unclear, however, whether police could make these distinctions without additional information.

Also, it is possible that officers are not perceiving subjects’ conditions correctly; however, evidence suggests that although officers cannot make specific diagnoses, they are good at identifying mental illness when subjects are symptomatic and making determinations based on limited information (Bittner, 1967; Engel and Silver, 2001; Fry, O’Riordan, and Geanellos, 2002). Specific symptoms could elude police, particularly in the context of serious crime, but overall police seem to be competent at identifying mental illness.

If, in fact, incidents involving people with mental illness are not significantly more likely to end in injury, then the “dangerousness” assertion may be overstated and result in unnecessary stigmatization of this population. Stigmatizing beliefs include misconceptions about mental illness—beliefs that often are reflected in movies and television (cf. Coverdale, Nairn, and Classen, 2002). This stigma can lead to discrimination, avoidance, and exclusion, and it could lead to the general segregation of people with mental illnesses (Corrigan and Watson, 2002). Stigmatization also can prevent people with mental illness from accessing the criminal justice services that they need (cf. Corrigan et al., 2005) as either witnesses or victims of crime.

In the long term, stigma not only prevents people with mental illnesses from accessing services but also will subsequently represent a threat to police effectiveness. Because people with mental illnesses are more likely to be victims of crime than perpetrators (Barbato, 2015), it is imperative that they report crimes and cooperate with the police for successful investigation. To further this cooperation, policies should be implemented that help to reduce the stigma attached to mental illness among police officers, particularly surrounding the myth of dangerousness. Training for police should be expanded to include tools to identify mental illness across a larger range of calls for service. For instance, people with mental illnesses who also are victims of sexual assault or other types of personal violence may already be known to police because of their mental illnesses. Therefore, both police and subjects with mental illnesses may have preconceived and often negative notions about how these encounters will play out (Watson et al., 2008). Because of the preexisting relationship, people with mental illnesses may not respond to traditional police practices. Additional

training on mental illness can inform police response, thereby providing better service to the community while enhancing overall police effectiveness.

Although we believe that these findings add to the growing body of literature exploring police encounters with people with mental illnesses, some limitations must be discussed. As with all official data, we do not know whether officers are filling out forms properly. Although officers are required to fill out a form every time force is used or someone is injured, the veracity of the information is not verified. Another limitation is that the data lack information about the officer characteristics such as race, education, and years in service. For example, some research has suggested that officers with more experience are less likely to use formal tools such as arrest or force (Green, 1997). Similarly, community context is missing from our data. We do not know whether community or neighborhood characteristics, including the availability of behavioral health resources, are predictive of injury because we do not have access to the exact location of the incidents in the sample.

Based on the available data, we cannot tease out the role of CITs in whether subjects or officers sustain or prevent injuries. Without comparison data, it is not possible to determine the relative effect of CIT training. If CIT is responsible for mediating the relationship between mental illness and injury, then the findings from this study would limit generalizability to only jurisdictions where the police have had this training.

Another problem is that because we do not know the backgrounds of the individuals involved in the incidents included in our sample, we do not know about prior encounters between police and the subjects. Evidence suggests that some officers become familiar with people with mental illnesses in their districts (Morabito, 2007). They become aware of which people are dangerous and how to approach them (Morabito, 2007). This familiarity could lead to a reduced likelihood of injury compared with interactions with previously unknown subjects and should be considered in future research.

Finally, and perhaps most importantly, this study is a limited snapshot of police encounters with the public. Our sample includes only incidences in which force was used or a firearm was pointed; it is not the complete sample of all police contacts with the public. We do not have a baseline of incidents to compare with use-of-force incidents. Relatedly, because the issue of officer and subject injury has important implications for both mental health and police policy and practice, data on the prevalence of these events would be extremely useful. Unfortunately the denominator for such rates—the number of police encounters with persons subjected of having serious mental illness—is lacking in most jurisdictions, including in the current study. It would be a significant contribution to the discussion of this issue if such data could be obtained.

Despite these limitations, these data allow for the examination of research questions previously unanswered regarding police encounters with people with mental illnesses. Our findings increase our understanding of the police management of these incidents by calling into question the impact of mental illness on the likelihood of injury to both police and subjects. That is, our research suggests that encounters with people with mental illnesses

are *not* more likely to end in injury as is commonly believed. Rather, these encounters are unlikely to result in reported injury, and mental illness *itself* does not significantly increase the likelihood of officer injury *unless* combined with the use of substances. Similarly, people with mental illnesses are not at greater risk for injury during encounters with the police unless also under the influence of a substance.

This finding suggests that symptoms of mental illness may not explain the likelihood of injury in encounters where force is used. Perhaps people with mental illnesses are not as unpredictable as originally believed, and rather, the influence of substance use (regardless of mental health) results in increased dangerousness. Alternatively, perhaps training such as CIT has given police officers the tools to manage the unpredictability. Although the reasons why this relationship exists cannot yet be explained, this finding sets the stage for additional research to explore how these encounters are negotiated by both police and people with mental illnesses.

However, it is impossible to gain a more complete understanding of the problem without data that allow us to measure its extent. It is difficult to research this issue because agencies typically do not systematically collect data about contacts with people with mental illnesses. Furthermore, the information included in the Law Enforcement Officers Killed and Assaulted supplement is incomplete at best. Individual departments can make these data available; yet even with access, use-of-force reports are not uniform, which makes it difficult to compare outcomes across departments.

As such, there is a distinct need for comprehensive data about criminal justice contacts with this population that spans across police agencies and jurisdictions. More comprehensive data would allow for testing of the effectiveness of mental health partnerships including CIT—an intervention whose effectiveness we still know little about. Although a great deal of research has examined these interventions (cf. Hails and Borum, 2003), few studies have looked across more than one or two departments (cf. Steadman et al., 2000; Teller et al., 2006). Although this research has been valuable, it has not presented a comprehensive picture of police/mental health partnerships or encounters between the police and people with mental illnesses.

Accordingly, we call on practitioners and policy makers to begin a wider scale collection of information about this issue in an effort to learn more about the safety of both police officers and people with mental illnesses—particularly those with co-occurring disorders. This research suggests that substance abuse is responsible for injuries both to police and subjects in use-of-force encounters. Not enough, however, is known about how substance abuse interacts with mental illness in these same encounters.

In the absence of this information, the myth of dangerousness creates unnecessary and harmful stigmatizing attitudes toward people with mental illnesses. Given that approximately 6–7% of police contacts involve people with mental illnesses (Cordner, 2006) and the disproportionate victimization of this population (Barbato, 2015), stigma could have far-reaching consequences on victimization experiences, reporting likelihood, and criminal

investigations. Yet, researchers cannot properly ascertain the likelihood or severity of injuries sustained by this population without more complete data, and it is beyond the capability or responsibility of any one local department to compile this information. This collection calls for a much broader and comprehensive effort so we know not only about the mental health status of individuals involved in force incidents but also about those contacts with police that do not end in force, arrest, or other negative outcomes. As policy makers begin to think about changes to the collection of local police data, measures of mental illness should be part of the discussion.

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# Police Use of Force and the Suspect with Mental Illness

## A Methodological Conundrum

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Routine police work brings officers in contact with all sorts of citizens, including those who exhibit bizarre behavior. Estimates of the nature and extent of those encounters are conflicting (see Cordner, 2006; Monahan, 1992; Morabito and Socia, 2015, this issue), but regardless of the correct numbers, it is generally accepted that a variety of problems involving those with mental illness are resolved by the police. We have learned that many citizens with mental illness also have alcohol and drug problems. For better or worse, the police are the frontline response to many citizens with mental illnesses and other conditions.

Morabito and Socia (2015) report that citizens with mental illness are perceived as a threat by the police, but they argue that no empirical evidence exists to support this claim. They conclude, “More generally, recent reports estimated that only approximately 4% of overall violence in the United States can be attributed to those with mental illness (Friedman, 2014), suggesting that the vast majority of people with mental illnesses are not violent or dangerous, except perhaps to themselves (Swanson et al., 2014).” To reach this conclusion, they cite an editorial in the *New York Times* (Friedman, 2014, para. 8) that stated, “If we can’t reliably identify people who are at risk of committing violent acts, then how can we possibly prevent guns from falling into the hands of those who are likely to kill?” They also cite Swanson, McGinty, Fazel, and Mays (2014: para. 11), who summarized research findings from The National Institute of Mental Health Epidemiologic Catchment Area study and reported, “The facts showed that people with serious mental illnesses are, indeed,

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somewhat more likely to commit violent acts than people who are not mentally ill, but the large majority are *not* violent toward others” [emphasis in the original]. In the Swanson et al. (2014) study, mental illness was identified through a structured diagnostic interview using *Diagnostic and Statistical Manual-III* criteria (American Psychiatric Association, 1980). Finally, Morabito and Socia (2015) review use-of-force reports, isolate injuries to those with mental illness, and conclude, “The variable of interest to the current study, perceived mental illness in a suspect, did not significantly increase the likelihood of injury to either party when it was not paired with perceived substance use.”

Although there is no reason to question or refute Morabito and Socia’s (2015) findings, it is important to qualify and understand them. As they report, we really do not have good information about the interactions between the police and the segment of the population with mental illness. Additionally, research findings on their violent tendencies and injuries from interactions they have with police are inconclusive. Our knowledge is so limited because although mental illness can be identified through clinical interviews, it is extremely difficult, except in the extreme cases, to identify someone with mental illness by observing his or her actions or by having limited interactions with the person. Someone acting and talking strangely can be doing so for a variety of reasons, and distinguishing mental illness from other forms of aberrant behavior is challenging even for trained professionals. The purpose of this policy essay is to raise methodological questions about research on police interacting with the mentally ill. The premise is that a police officer who checks the box on a form to indicate that the suspect is mentally ill or writes observations on a narrative is providing an educated guess. We will explore how much a decision is based on education and how much it is based on a guess.

Morabito and Socia (2015) evaluate the more than 6,000 use-of-force incidents reported by the police in Portland, Oregon, between 2008 and 2011. They focus on the effects of mental illness on the use of force and likelihood of injuries to officers and suspects. Their approach is a popular one that explores the impact of a specified condition or action on officer and suspect injuries. For example, several studies have explored the different rates of injuries to officers and suspects when a conducted electronic weapon (CEW) is deployed (see Terrill and Paoline, 2012). The methods are relatively simple as force events are analyzed by injury and controlled by the use of a CEW. Use-of-force events are usually reported on a distinct form or a general incident report, whereas the reporting officer or supervisor notes observed injuries and complaints of injuries on the form. Normally, these terms are defined by policy or training and can be checked by reviewing statements, photographs, and medical records. The use of a CEW is both reported by the officers and checked mechanically when data from the CEW are downloaded. Although some measurement error can occur, the terms are operationalized, and the data can be checked and audited. Even though the methods are straightforward and the use of force and deployment of a CEW are easy to determine, the existence of an injury requires a specific definition. For

example, what constitutes an injury? Although this seems to be a relatively clear choice, the distinction between an injury and noninjury can become subjective or clouded. Most policies and instructions to complete a form will include a definition or instruction on what information is requested. There are questions about the temporal order of an injury. If a suspect twists his wrist fighting an officer prior to the deployment of a CEW, then does that count as a CEW-related injury? If an officer is cut by a suspect and subsequently deploys a CEW, then should that injury be counted as an injury in an encounter involving a CEW? It is likely that these situations are covered in a policy or when officers are instructed how to complete a form. However, these decisions and whether the CEW puncture is considered an injury can make an important difference in the analysis of harm and general assessment of the CEW (see Kaminski, Engel, Rojek, Smith, and Alpert, 2015). As in any assessment, the numerator or injury is often compared with the denominator or number of deployments. It is important to define both the numerator and the denominator. In the case of encounters with the mentally ill, the terms *encounter*, *mentally ill*, and *injury* must be operationalized and coded properly.

Morabito and Socia (2015) do an excellent job of reviewing the literature on police encounters with the mentally ill and related injuries. As part of their literature review, they note with concern that scholars often combine as a group suspects with mental illness with those who are impaired by alcohol and drugs. They report that a consequence of that pooling is the inability to distinguish whether mental illness, substance abuse, or a combination contributes to violence in police encounters. In an attempt to overcome the deficiencies of prior research, Morabito and Socia used the Portland officers' perception of whether a suspect had mental illness but was not under the influence of a substance or both. As they report, the definition of mental illness was established by Portland Police Policy (Portland Police Bureau, 2014, para. 6):

A person may be affected by mental illness if he or she displays an inability to think rationally (e.g., delusions or hallucinations); exercise adequate control over behavior or impulses (e.g., aggressive, suicidal, homicidal, sexual); and/or take reasonable care of his or her welfare with regard to basic provisions for clothing, food, shelter, or safety.

These directions are helpful in educating the officer as to the specific criteria used to determine whether a person is suffering from mental illness. This information, operationalized to be interpretation of verbal or behavioral cues that suggest an "inability to think rationally," will trigger the determination that the subject is suffering from mental illness, and such encounters will be so coded. Unfortunately, no research has been reported on whether officers are consistent in their decisions based on cues or other prompts to determine whether subjects are mentally ill or under the influence of one substance or another. It is logical that specific instruction, training, and discussion on how to make that

determination is likely to improve decision making, but there is no convincing information that education has more of an influence than a gut reaction or conjecture.

### **Mad, Bad, or Sad<sup>1</sup>**

Research findings on police encounters with subjects who suffer from mental illness rely ultimately on the officer's ability to distinguish the sad and the bad from the mad (see Phelan, Link, Stueve, and Pescosolido, 2000). Morabito and Socia (2015) note that, "Specific symptoms could elude police, particularly in the context of serious crime, but overall police seem to be competent at identifying mental illness." Unfortunately, Morabito and Socia do not provide any data to substantiate their claim (although they provide general citations). Perhaps this is a guide to an important research question: Can police officers identify the mentally ill? The ability to distinguish the mad, bad, and sad involves an inherently subjective process that requires an officer to differentiate among three or more reasons to explain the same behavior. As noted previously, this decision requires officers to interpret information from a call for service, an observation, and/or brief encounter to form an opinion as to whether the person is suffering from a mental illness, is under the influence of alcohol or drugs, both, or none of the above. The impact of that decision has long-reaching consequences. If officers identify someone as under the influence, then the officer might take certain actions to deal with the subject. If the subject is identified as one who is merely a person having a very bad day and acting out, then the officer's actions could take a different form. Finally, if the officer believes the subject is suffering from a mental illness, then the officer can take other actions. These responses range from aggressive enforcement techniques to deescalation and calling for trained specialists as backup. Regardless of the available resources and potential responses, the important consideration is the identification of the reason for the subject's behavior.

In the late 1990s, research on the use of force was conducted in both Eugene and Springfield, Oregon, and during the development of the research instrument, officers were questioned about their abilities to distinguish between the mentally ill and those under the influence of drugs and alcohol. It was the consensus among the officers that the way the information on the use-of-force form was collected was unreliable, as it was more of a subjective impression or estimate than the proficient identification of a reason for a subject's action. It was their suggestion to combine the behaviors into one classification. Data from that study were subsequently coded into a variable that included perceived mental illness and being under the influence of alcohol and drugs (Alpert and Dunham, 2000). More recently, Adkins, Burkhardt, and Lanfear (2015) studied interactions between the police in both Corvallis and Benton County, Oregon, and mentally ill subjects. As conscientious researchers relying on perceptions, they were concerned about the capabilities of the officers

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1. This term was told to the author by Sr. Sgt. Damien Hayden, Queensland Police Service.

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and offered in their technical report the following clarification (Adkins, Burkhardt, Lanfear, Stevens, and Amorim, n.d.: 1, footnote 1):

It is important to note that throughout this report the term people with mental illnesses people [sic] perceived by law enforcement as exhibiting symptoms of a mental illness. The term should not be taken as an indication of a formal clinical diagnosis. Law enforcement agents have distinctive criteria and thresholds which they utilize to determine if a person has a mental illness. The specific criteria and thresholds may vary by agency, officer and even by contact, based on existing knowledge, or lack thereof, of the individual being contacted.

### **Consequences of the Error**

Police managers and mental health providers have stressed the importance of identifying subjects with mental illness so they can be treated fairly and safely, as well as to provide resources for them. A great deal of attention has been given to the ways in which the police handle the mentally ill and how those resources can improve service delivery (Murphy, 1986). What has not been managed well is the ways in which police officers identify these subjects and differentiate them from others who are either intoxicated or acting out in a violent way. Officers are trained extensively in the analysis of physical threat, and they might use that type of identification to inform them of how to manage those with mental illness.

When police officers confront suspects whom they think are threats, they may act differently by being defensive, protective, or even aggressive. The way an officer responds to the citizen will likely frame the interaction. When an officer confronts a person who is acting strangely, the officer may proceed in a variety of ways. On the one hand, if the officer is calm and composed and does not show signs of fear or defensiveness, then the subject may respond calmly and the interaction could result in a positive manner. On the other hand, if the officer is excited and seems agitated, the subject may respond in a like manner, causing the officer to react negatively, and the interaction can result in coercion and possibly the use of force. Therefore, it is important for police officers to learn how to identify those with mental illness, summon available resources to manage the person, and learn to calm or defuse the situation (Jennings and Hudak, 2015; Murphy, 1986). This initial decision by the officer is likely made after a brief observation and/or personal contact with the individual. Although there is an important emphasis on tactical disengagement with many subjects, this strategy may be most important with those suffering from mental illness. In the real world, the interaction will play itself out and the officer will write his or her report where there is a box or narrative to indicate whether the suspect was suffering from a mental illness, intoxication, or both. As researchers, we rely heavily on whether that box was ticked but not on the decisions or even actions taken by the officer. We use those ticked boxes as “variables of interest” and base our statistical analyses of the aggregate data based on those ticks. As the use of force is a rare event, and the use of force on a suspect

suffering from mental illness is an even more rare event, false positives or false negatives can impact our findings, conclusions, and policy recommendations. Perhaps the false data will have an impact on the ways officers react to suspects. We must be careful to understand how our data are produced and report their limitations. More than six decades ago, Paul Tappan warned us to pay particular attention to the quality of the data we collect and not just to build “statistical cathedrals on muddy foundations and shifting sands” (1949: 55).

Whether the dangerousness of people with mental illness is a myth remains unanswered. Morabito and Socia (2015) raise a lot of good points and direct future research in the proper direction. The most important question is whether police officers are competent at identifying persons with mental illness and if particular types of training will improve that skill.

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## Building on the Evidence

### Guiding Policy and Research on Police Encounters with Persons with Mental Illnesses

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Police–citizen encounters that end badly—with someone getting injured or killed—have lately captured media attention in the United States. These events have focused public attention on the troubled relationship between law enforcement and urban communities plagued by violent crime, entrenched poverty, and a legacy of racial discrimination. Within that larger social context are complex situations that police officers often face when called to intervene with people who appear to be mentally ill and who might pose a risk of harm to themselves or the public. In these encounters, police act not only as public safety officers but also as informal social workers, emergency health-care workers, and providers of access to treatment services (Wood, Swanson, Burris, and Gilbert, 2011). The question of when to use force in such cases, as well as how best to avoid or minimize its use without compromising officer safety, is an ongoing challenge for law enforcement training, practice, policy, and community relations.

The U.S. Supreme Court’s recent decision in *City and County of San Francisco v. Sheehan* (2015) highlighted complex legal issues related to these matters as well. The court held that officers who forcibly entered the room of a woman with a mental disability and shot her are entitled to qualified immunity from a lawsuit seeking redress for the woman’s injuries. The court left undetermined the broader question of whether police officers who arrest or detain and transport a person with a mental illness are subject to ADA Title II requirements to provide reasonable accommodation of persons with disabilities. In this policy essay, I discuss several underlying issues that are raised by Morabito and Socia’s (2015, this issue) study on the question of potentially increased risk of injury when police officers encounter

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persons in the community who seem to suffer from acute psychiatric symptoms or substance intoxication.

Within the broad range of police encounters, special attention is needed to understand and inform encounters with persons with mental illnesses—a challenging interface during which police and persons in mental-health crisis may both feel vulnerable, raising the risk that the exchange could involve use of force or injury. Officers' presumptions about the dangerousness of persons in mental-health crisis are likely to have a strong influence on their response, including the extent to which they use force during those encounters. There have been longstanding concerns that persons with mental illnesses face prejudicial treatment by police largely for being misunderstood and stigmatized, and that they are disproportionately vulnerable to police use of force and injury for those reasons. Little definitive evidence exists, however, to support or discount this hypothesis.

### **New Evidence on Injury During Police Encounters with People with Mental Illnesses**

Morabito and Socia's (2015) study on this issue offers important new evidence about the role of mental illnesses in predicting subject or officer injury during police encounters that involve use of force, with an expectation that any real element of heightened dangerousness among persons with mental illnesses would translate to an increased likelihood of injury during those encounters in which force was used. They examined predictors of injury during these police encounters in Portland, Oregon—where all officers have Crisis Intervention Team (CIT) training—and aimed to determine whether subjects who were perceived by officers to be mentally ill were at an increased risk of injury for themselves or the responding officers. Notably, Morabito and Socia found no evidence that mental illnesses alone increased risk for injury in these cases but found that several other situational- and individual-level factors did, including assaultive behavior toward officer, resisting arrest, and being armed, as well as substance use, both alone and in combination with mental illness.

Morabito and Socia's (2015) work makes an important contribution to what is known about real versus perceived dangers during police encounters with persons with mental illnesses. The absence of increased risk for injury among suspects with mental illnesses suggests that these individuals are not subject to disproportionately prejudicial and discriminatory treatment by police officers but rather that the situational circumstances and whether the suspect is intoxicated largely drive the intensity of police officers' response. Furthermore, the absence of increased risk for injury among the responding officers in Morabito and Socia's study suggests that any assumption by officers that mentally ill individuals are more dangerous to engage than others is unfounded. To the extent that these study findings can be generalized to other settings and police officers, it can help shape evidence-based approaches to policing practice.

Whereas Morabito and Socia (2015) set out to help answer the question of whether persons with mental illnesses are indeed more dangerous in their encounters with police

than individuals without mental illnesses, their findings more narrowly reflect policing practices and outcomes among CIT-trained officers. All officers in their Portland, Oregon, study population were CIT trained at the time of the study, and so the results may not generalize to officers with no CIT training, who comprise most of the U.S. police force. The findings do, however, provide highly relevant data on a primary outcome of interest in the scope of CIT police work—the likelihood of injury to suspects with mental illnesses and to responding officers during encounters involving use of force. The absence of an elevated risk for injury associated with mental illnesses alone in this study cannot be attributed to a “CIT effect” given there was not a comparison group of officers without CIT training; nonetheless, it provides important preliminary evidence for more definitive research on the topic.

Another dimension of this study that would benefit from further investigation involves the severity of the subjects’ offenses surrounding these incidents. Controlling for offense-level characteristics could help illuminate the causal pathway to injury when encounters with police involve the use of force. Offense severity could confound the relationship between mental illnesses and injuries sustained during the encounter if (a) the subgroup with mental illnesses primarily interfaced with police for minor offenses (e.g., trespassing, loitering, and disturbing the peace) and was thereby at lower risk for more extreme use of force that leads to injury, and (b) the subgroup without mental illnesses primarily interfaced with police for more serious or violent offenses that might be more likely to lead to injury during police use of force. With that, accounting for severity of offense in the study design might reveal that persons with mental illnesses are indeed at higher risk for injury during police encounters involving the use of force if compared with persons without mental illnesses who committed a similar offense. Achieving a better understanding of the basis for injury during police encounters would add important clarity to this line of inquiry about police practice, officers’ presumptions about the dangerousness of mentally ill persons, and the extent to which those presumptions drive their course of action.

### **Mental Illnesses and Dangerousness: What We Know**

A robust literature has explored the question of dangerousness among persons with mental illnesses, aiming to determine whether and how much more dangerous they might be than their counterparts in the community with no mental illnesses. National surveys have documented a widespread public belief that persons with serious mental illnesses such as schizophrenia are likely to be dangerous (Pescosolido, Monahan, Link, Stueve, and Kikuzawa, 1999); epidemiologic evidence paints a more complex picture. The NIMH Epidemiologic Catchment Area (ECA) Study found that 10% to 13% of adults with serious mental illnesses had committed an assault versus 3% of other adults in the community without mental illnesses (Swanson, 1994); more recent findings from the National Epidemiologic Survey on Alcohol and Related Conditions (NESARC) were consistent (Van Dorn, Volavka, and Johnson, 2012).

The landmark MacArthur Violence Risk Assessment Study (MVRAS) (Monahan et al., 2001) found that 28% of a sample of discharged acute psychiatric inpatients—a relatively high-risk subset of all adults with mental illnesses—committed a serious violent act (i.e., causing injury to another person or using a weapon to harm or threaten another person). Interestingly, the results indicated that only those with co-occurring substance use disorders had an elevated risk, and those with mental illnesses alone were no more violent in the community than other residents of their same neighborhoods. The study sample, however, was situated in a high-risk neighborhood, so the risk for the subset with mental illnesses alone might have been elevated if compared with a more typical sample of the general population.

Estimates from the ECA study indicate that just 4% of violent acts can be attributed to mental illnesses, which means 96% of violence is committed for other reasons. In fact, persons with mental illnesses are far more likely to be victims of violence rather than perpetrators—with risks for victimization that are far higher than others in the general population (Teplin, McClelland, Abram, and Weiner, 2005).

### **Mental Illnesses and Dangerousness: A Broader Framework**

Several studies following the MVRAS have uncovered important risk factors for violence among persons with mental illnesses that lie beyond their psychopathology and that are shared with offenders who are not mentally ill. Substance abuse is a leading predictor of criminal offending and violence in this population—as demonstrated also by Morabito and Socia (2015)—and has been well documented in several other community-based studies of adults with mental illnesses (Elbogen and Johnson, 2009; Robertson, Swanson, Frisman, Lin, and Swartz, 2014; Swanson et al., 2006, 2008; Van Dorn et al., 2012).

Other recent work has provided a theoretical framework and supporting empirical evidence to demonstrate the strong influence of factors beyond psychopathology among mentally ill offenders, including criminogenic and social-environmental risk factors. Along with substance abuse, Swanson and colleagues (2002, 2008) found that violent behavior in adults with mental illnesses was linked to a history of violent victimization and trauma, current exposure to violence in the community, and a history of longstanding antisocial behavior problems, typically beginning in childhood. Meanwhile, Gray et al. (2004) estimated that the actual risk for offending was largely attributable to criminogenic variables and found that the addition of clinical variables did not add explanatory power.

Skeem, Manchak, and Peterson (2011) devised an expanded framework for understanding offending in adults with mental illnesses, examining a broader range of direct and indirect influences on offending. Estimates in related work have indicated that mentally ill offenders are especially likely to have general risk factors for offending (Skeem, Winter, Kennealy, Loudon, and Tatar, 2014)—including substance use—and that a mental illness itself is responsible for as little as 6% to 10% of offending (Junginger, Claypoole, Laygo, and

Crisanti, 2006; Peterson, Skeem, Hart, Vidal, and Keith, 2010; Peterson, Skeem, Kennealy, and Bray, 2014).

A broad body of research has indicated that despite common perceptions among the general public, and possibly law enforcement, mental illness per se influences risks for offending and violence only modestly.

### **Next Steps for Research**

Morabito and Socia (2015) have contributed an important new piece of evidence about outcomes of police encounters for persons with mental illnesses, indicating that they are no more likely than persons without mental illnesses to sustain injuries themselves or cause injuries to officers, which may be consistent with the literature that has demonstrated that mental illness alone does not substantially increase risk for violence. It could be that there truly is no elevated risk for injury among mentally ill individuals who interface with police and that the highly publicized incidents of officer-involved shootings of mentally ill persons are atypical. Morabito and Socia's results also could indicate an unmeasured beneficial effect of CIT. Therefore, an important next step in this line of research would be a study that first compares rates of police use of force and then compares the rates of injury among subjects of police use of force in two groups: persons with mental illnesses and persons without mental illnesses, controlling for relevant officer-, subject-, and offense-level characteristics.

It would be important to conduct this research in a jurisdiction that has both CIT and non-CIT officers, which would help determine whether the absence of elevated risk for injury for persons with mental illnesses in Morabito and Socia's (2015) study was attributable to a beneficial CIT effect, given that all officers in their study jurisdiction were CIT trained. Identifying benefits of CIT in this context would both add to the literature on CIT effectiveness and help inform jurisdictions that do not yet have CIT programs during their considerations around its adoption and implementation.

A real contribution of a study like the one just proposed could be identifying differential rates of police use of force among suspects with and without mental illnesses, along with differential rates of resulting injury in a representative sample of police officers, only some of whom have CIT training. It could be that, as Morabito and Socia (2015) found in their study, there are no differential rates of injury by observed mental status of the suspect, but that persons with mental illnesses are more likely to have force used against them. If mentally ill persons are indeed more vulnerable to police use of force, then it would be important to understand the extent to which the use of force itself has negative effects and not just the injuries that can result from it. Especially for individuals with mental illness, being subjected to police use of force could have both internalizing and externalizing harms—posttraumatic stress, a chilling effect if the experience were to engender distrust in police and other members of institutional authority, and reinforced stigma against persons with mental illnesses if community members observe these incidents and conclude that

mentally ill individuals are dangerous and warrant that level of engagement by police as a necessary public safety measure.

### **Next Steps for Policy**

During this time of national concern about excessive use of force by police, a range of stakeholders—including communities, federal and state governments, and law enforcement—seek improvements in community partnership, accountability, transparency, and skillful restraint to begin reversing the problems of excessive police use of force, including as they affect persons with mental illnesses. Prevention-oriented policies and programs that offer tools for improving de-escalation of potentially dangerous encounters with persons in a mental-health crisis could help minimize risk for injury during police encounters for all parties involved and reduce the likelihood that force will be needed at all. As an example, a recent trend in state law making broadens police authority to remove guns from persons who are considered to be in danger of harming themselves or others but who have not committed a criminal act and are not prohibited from gun ownership. Connecticut, Indiana, and California are three states with different versions of a law in place to remove guns from potentially dangerous persons. A benefit of this policy approach to managing potential dangerousness among persons in crisis is in removing the focus from mental illnesses per se and placing it instead on demonstrated behaviors by any member of the community that suggest heightened risk. This conceptual and operational shift in focus from mental illnesses to dangerousness is important for reducing stigma around mental illnesses, both in the community and among law enforcement officers.

There has also been increased awareness in law enforcement communities of the need for more in-depth officer training in de-escalation. The *New York Times* reported on a recent survey conducted by the Police Executive Research Forum (PERF) that highlighted the tradition of a disproportionate focus on training in use of force rather than de-escalation techniques and estimated the following median number of hours spent in various training areas: firearms = 58 hours, defensive tactics = 49 hours, de-escalation = 8 hours, and crisis intervention = 8 hours (Apuzzo, 2015). PERF asserted that some large police departments support the idea of a new approach to training that focuses more heavily on teaching officers the necessary skills to defuse tense situations and avoid violent confrontations; but other departments are resistant. Persons with mental illnesses would undoubtedly benefit during their encounters with police who had more comprehensive training in de-escalation and potentially reduce their chances not only of injury during those encounters but also of being subject to police use of force at all.

Certain jurisdictions have had strong success in implementing prevention-oriented models that include specialized officer response to persons with mental illnesses like CIT. Other successful additions to the community infrastructure provide officers with an alternative to taking a person in crisis to the emergency department or jail. San Antonio, Texas, paired CIT training with a newly constructed Restoration Center, a facility with a 16-bed



psychiatric unit, a medical clinic, and a “sobering room” where police can drop off people who are intoxicated rather than taking them to jail. The city reports related savings for the police department of \$600,000 a year in overtime pay alone; undoubtedly, many encounters that would have involved use of force and possible injury were averted.

Morabito and Socia (2015) also call for policy improvements in consistency, quality, and availability of data on encounters during which police use force and the surrounding circumstances, including measures that thoroughly describe the circumstances of a person’s mental-health crisis. While a range of community and government stakeholders are engaged, now is an especially important time to develop and institute progressive policies in an effort to increase accountability for what takes place during police encounters. New policies that require detailed reporting of police use-of-force incidents would also facilitate further rigorous research on the effectiveness of CIT, preemptive gun seizure laws, and other approaches to mitigating dangerousness in encounters with persons in mental-health crisis. To ensure that new reporting policies are sustainable, however, jurisdictions would need to give careful consideration to issues around their implementation. Challenges to be addressed include the acceptability of such policies among police department leadership and their officers, as well as the feasibility of additional reporting and paperwork for officers and data management for administration.

Morabito and Socia (2015) offer salient new evidence about risk for injury during police encounters involving persons with mental illnesses in which force is used. Their study lays a foundation for further research that should extend to include non-CIT police officers to learn more about both CIT-specific outcomes and any unobserved elevated risk for injury during police encounters with persons with mental illnesses. Policy response to these concerns can begin now, starting with improved reporting on police encounters with persons with mental illnesses and a rigorous focus on building officers’ skills in de-escalation.

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# EDITORIAL INTRODUCTION

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## OUTCOME EVALUATION PROGRAM FOR FEMALE OFFENDERS

### Implementation and Outcomes in Cognitive-Behavioral Therapy Among Female Prisoners

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A body of evidence has emerged over the past several decades about “what works” in rehabilitating criminal offenders (Andrews and Bonta, 2003; MacKenzie, 2006; MacKenzie and Zajac, 2013). Much of this evidential basis is summed up in the risk–need–responsivity (RNR) framework (Andrews and Bonta, 2010), which by now should be sufficiently well known that I will not bother with a summary of it in this brief introductory piece. The “what works” and RNR literatures inform a set “principles of effective offender intervention” that can be used, with appropriate caveats and limitations, as a guide to the design of correctional interventions (Andrews and Bonta, 2003; Van Voorhis, Braswell, and Lester, 2004). These principles direct that any specific type of treatment intervention delivered to criminal justice clients should be supported by evidence of effectiveness in reducing recidivism or having impacts on other criteria of interest (e.g., reducing drug relapse or improving compliance with supervision). One widely used treatment approach that has accumulated a strong basis of support in the research is cognitive-behavioral therapy (CBT). In brief, CBT addresses antisocial cognitions and dysfunctional thinking patterns that support criminal behavior with highly structured behavioral therapies targeting problem solving, decision making, coping mechanisms, peer associates, and other factors, with a strong emphasis not only on *learning* new skills but also on *practicing and rehearsing* those skills to instill more prosocial behavioral routines, for example, to help the client deal with high-risk situations (Van Voorhis et al., 2004). A number of studies and meta-analyses have established considerable support for this approach (Landenberger and Lipsey, 2005; Lipsey, Landenberger, and Wilson, 2007; Pearson, Lipton, Cleland, and Yee, 2002; Wilson, Bouffard, and MacKenzie, 2005).

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Although we know that some things can “work” with offenders, at least in principle, and that one of those things is CBT, much remains to be learned about how to make evidence-based programs work consistently in the daily correctional practice setting, such as prison-based treatment groups and probation or parole contexts. Some evidence shows that the impact of CBT programs attenuates when not implemented with careful fidelity to the model (Lipsey, Chapman, and Landenberger, 2001; Van Voorhis et al., 2002; Wilson et al., 2005). More broadly, the emerging field of implementation science indicates that program fidelity does interact importantly with outcomes; well-implemented programs have produced treatment effects as much as three times larger than programs with low implementation fidelity (Andrews and Dowden, 2005; Durlak and DuPre, 2008; Fixsen, Naoom, Blasé, Friedman, and Wallace, 2005). This *implementation fidelity principle* tells us that we must pay at least as much attention to *how* we are doing something as to *what* we are doing, but implementation remains a black box in many cases. Program studies often neglect process and fidelity evaluation, as well as evaluability assessment; the standards for exactly how good program implementation needs to be are still largely unsettled (Durlak and DuPre, 2008; Elliott, Zajac, and Meyer, 2013).

Duwe and Clark (2015, this issue) contribute to advancing our understanding of the nexus between program fidelity and program outcomes. Their research examines the differential outcomes of a high-implementation fidelity period and a low-implementation fidelity period of the Moving On gender-responsive CBT program for incarcerated women in Minnesota. Their evaluation found that treatment effects were significantly better when Moving On was implemented with fidelity; indeed, in the low-fidelity period, no treatment effects at all were detected. Importantly, Duwe and Clark (2015) provide a basic scale of implementation fidelity, affording a more fine-grained picture of exactly how good (or poor) implementation was at the two periods. As shown, fidelity in the “high” period was markedly better than in the “low” period. This finding provides discrimination between the two periods and contributes much needed insight into how “good” program implementation should be. Thus, this study contributes to our understanding of the results produced by CBT programs in general, and Moving On in particular, and it highlights the importance of attention to fidelity.

Duwe and Clark (2015) discuss the policy implications of their findings, but this bears reinforcing here. It does little good to implement any program poorly (although probably it does not matter much for truly ineffective programs and may even be fortuitously beneficial for them!), but for programs that seem to show some promise of producing useful treatment effects, poor implementation is a waste of public resources and a disservice to clients who need the help that such programs may deliver. Negative results from poorly implemented programs (especially when the poor implementation quality is not known) also contribute to cynicism about whether anything works in rehabilitating offenders. Next, studies such as that by Duwe and Clark (2015) have reinforced the importance of documenting and reporting implementation fidelity, as well as broader implementation experiences, in all

program evaluations and of considering this information in efforts to cumulate results of disparate evaluations (i.e., in meta-analyses and program reviews). Finally, this line of inquiry demonstrates to program managers and clinicians the importance of continuous quality control in the administration of justice programs, or according to the old adage, if something is worth doing, then it is worth doing right.

In their policy essays, Salisbury (2015, this issue) and Miller and Miller (2015, this issue) reinforce the importance of conducting process and implementation fidelity assessments as an absolutely essential complement to outcome evaluation. This can be especially important, as Salisbury (2015) notes, to avoid Type 2 errors (e.g., concluding that Moving On had no effect, when in fact it might have if proper implementation had been achieved). Thus, they commend Duwe and Clark (2015) for the contribution made by their study. But both policy essays offer suggestions for improvements in future research of this kind. Most notably, process evaluation involves more than simply running down a checklist of whether a specific set of program elements is present or absent. Drawing from Durlak and DuPre (2008), a fuller understanding of implementation is achieved by examining the rich context of political, ecological, and organizational cultural factors that condition implementation. Moreover, as specifically critiqued by Salisbury (2015), a process evaluation of *gender-specific programs* should consider the use of process evaluation tools that have been specifically developed for this purpose (although she does offer the limitation that these tools have not yet been widely used or tested). I concur that we need to learn much more not only about implementation but also about how to study and measure it. As someone who is currently leading the process evaluation phase of the National Institute of Justice Honest Opportunity Probation with Enforcement Demonstration Field Experiment (HOPE DFE), I can attest that fully comprehensive, real-time process and implementation fidelity evaluations are extremely time consuming and challenging even when supported by grants. My colleagues and I are fortunate to have the time and resources to document closely the range of experiences associated with the implementation of HOPE at four sites nationally, but sometimes evaluators must work with what they have (even retrospectively) often with limited support. Duwe and Clark (2015) seem to have made good use of the program information that was available to them in describing how implementation and outcomes unfolded together in one program.

In closing, science is a slow process of knowledge building; no single study answers the question in full, but indeed every bit of research takes us a step closer to a better understanding of a problem. Although much more remains to be done in the study of implementation, Duwe and Clark (2015) have contributed a piece toward our understanding of this field.

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# Importance of Program Integrity

## Outcome Evaluation of a Gender-Responsive, Cognitive-Behavioral Program for Female Offenders

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### Research Summary

*We used a quasi-experimental design to evaluate the effectiveness of Moving On, a gender-responsive, cognitive-behavioral program designed for female offenders. Between 2001 and 2013, there were two distinct periods in which Moving On was administered with, and without, fidelity among female Minnesota prisoners. To determine whether program integrity matters, we examined the performance of Moving On across these two periods. By using multiple comparison groups, we found that Moving On significantly reduced two of the four measures of recidivism when it was implemented with fidelity. The program did not have a significant impact on any of the four recidivism measures, however, when it operated without fidelity.*

### Policy Implication

*The growth of the “what works” literature and the emphasis on evidence-based practices have helped foster the notion that correctional systems can improve public safety by reducing recidivism. Given that Moving On’s success hinged on whether it was delivered with integrity, our results show that correctional practitioners can take an effective intervention and make it ineffective. Providing offenders with evidence-based interventions that lack therapeutic integrity not only promotes a false sense of effectiveness, but also it squanders the limited supply of programming resources available to correctional agencies. The findings suggest that ensuring program integrity is critical*

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*to the efficient use of successful interventions that deliver on the promise of reduced recidivism.*

### **Keywords**

*program integrity, recidivism, prison, Moving On, cognitive-behavioral program, gender-responsive program*

Cognitive-behavioral treatment (CBT) is one of the most effective correctional tools for reducing recidivism (Allen, MacKenzie, and Hickman, 2001; Lipsey, Chapman, and Landenberger, 2001; Lipsey, Landenberger, and Wilson, 2007; Pearson, Lipton, Cleland, and Yee, 2002; Wilson, Bouffard, and MacKenzie, 2005). CBT includes all programs that address the link between dysfunctional thought processes and harmful behaviors through timely reinforcements and punishments, as well as through role-playing and skill-building exercises. These programs aim to improve decision-making and problem-solving skills, as well as to teach individuals how to manage various forms of outside stimuli. CBT can reduce recidivism by targeting an array of risk factors, including general antisocial cognition and chemical dependency.

Although many studies have documented CBT's effectiveness for reducing recidivism, multiple meta-analyses have revealed that the magnitude of this effect can vary widely (e.g., Pearson et al., 2002; Wilson et al., 2005). Researchers have suggested that this variability in effectiveness could partly be a result of the implementation fidelity of CBT programs (Lipsey and Cullen, 2007; Lowenkamp, Latessa, and Smith, 2006; Palmer, 1995). That is, the CBT programs designed in accordance with established principles of effective correctional interventions that maintain integrity upon implementation should be more effective than the same or similar programs that deviate too far from their original designs and compromise evidence-based program elements (Andrews and Dowden, 2005; Gendreau, Goggin, and Smith, 1999; Lowenkamp et al., 2006). Despite wide acceptance that program integrity is an important piece of effective correctional programs, few studies have examined the link between program integrity and recidivism. The current study addresses this deficit in the literature with a quasi-experimental design that compares the recidivism outcomes of CBT program participants when a program was and was not implemented as designed.

Given that males account for a large majority of all correctional populations, most research on CBT's effectiveness has focused on programs that commonly or exclusively treat males. In addition to examining the link between program integrity and recidivism, the current study makes another contribution to the literature by focusing on a CBT program designed exclusively for women offenders: *Moving On: A Program for At-Risk Women* (Van Dieten, 2010). To date, only one outcome evaluation of Moving On has been published, and it has some methodological shortcomings that the current study overcomes.

## Effective Interventions and Program Fidelity

The criminal justice system has amassed a library of research on how to deal effectively with crime and the individuals that commit crimes. Criminal justice practitioners can now reference a large body of empirical evidence on best practices in every field from policing, to the courts, to corrections. In corrections, practitioners have increasingly adopted the *principles of effective interventions* outlined by Andrews, Bonta, and Hoge (1990) to design programs and guide facility operations (see also Gendreau, 1996; Gendreau and Andrews, 1990).

In addition to the acceptance of CBT as one of the preferred methods of offender intervention, the *principles* outlined by Andrews et al. (1990) also hold that interventions should be matched to an offender's risk of reoffending, criminogenic needs, and responsivity issues (see Gendreau, French, and Gionet, 2004). This risk–need–responsivity (RNR) model calls for offender risk to be measured by using actuarial risk-assessment tools that have been validated and normed (Andrews and Bonta, 2010). The most intensive programs—generally measured by total length and number of hours—should be reserved for individuals rated as high risk (Sperber, Latessa, and Makarios, 2013). Criminogenic needs are individual characteristics that increase the risk of offending behaviors (Latessa and Lowenkamp, 2005). Static needs (e.g., prior criminal record and age) cannot be changed through interventions, whereas dynamic needs (e.g., antisocial attitudes and chemical dependency) can and should be targeted for the best recidivism outcomes. The RNR model also dictates that individual characteristics that could affect responsiveness to treatment should be considered when assigning offenders to programs (Andrews and Bonta, 2010; Dowden and Andrews, 1999). Gender is a responsivity issue. Although some correctional programs are gender neutral, in that they can be effective for both males and females, some programs target the unique risk factors that affect females more than males, or vice versa.

Well-designed programs that adhere to the RNR model and include many of the other evidence-based intervention strategies outlined by Andrews et al. (1990) can be ineffective if they are not implemented as designed (Matthews, Hubbard, and Latessa, 2001; Van Voorhis and Brown, 1996). By altering an intervention's original design, program administrators risk losing too many of the program components that contribute to its potential effectiveness (Fixsen, Naoom, Blasé, Friedman, and Wallace, 2005). Budgetary limitations, staff turnover, time constraints, and many other potential disruptions can erode program integrity (Durlak and DuPre, 2008). Evaluability assessments can be used to measure the degree to which programs maintain integrity upon implementation (Prosavac and Carey, 1992; Trevisan and Huang, 2003).

The Correctional Program Assessment Inventory (CPAI) and the Evidence-Based Correctional Program Checklist (CPC) are two standardized evaluability assessments created specifically to assess the design and implementation of correctional programs (Gendreau and Andrews, 1994; Latessa, 2012). Effective correctional programs can vary in terms of focus and substance, but several program elements contribute to the likelihood that a

program will significantly reduce recidivism, including qualified program leadership and staff, evidence-based treatment approaches, and use of risk and need assessments. The CPAI and CPC measure the extent to which these and other elements are present in a program. These tools were developed and validated based on assessments from hundreds of correctional programs. However, few subsequent studies have examined the relationship between program integrity and recidivism outcomes.

Nesovic (2003) used a condensed version of the CPAI to rate adult and juvenile correctional programs based on 173 recidivism outcome evaluations with 266 effect sizes. Nesovic (2003) did not directly assess programs firsthand by using the CPAI. Rather, the author based the assessment on written information about each of the evaluated programs. The average Pearson's  $r$  correlation between CPAI scores and phi coefficients derived from the evaluations was 0.46 ( $p < .05$ ). The positive correlation coefficient indicates that higher CPAI scores are associated with larger recidivism reduction effects.

With a more complete, yet still condensed, version of the CPAI, Lowenkamp et al. (2006) examined the relationship between program integrity and effectiveness with data from community-based residential programs ("halfway houses") in Ohio. The researchers matched more than 3,000 parolees released to halfway houses with a similar set of parolees not released to halfway houses, and they rated the halfway house programs by using a slightly abbreviated form of the CPAI. The total CPAI scores were positively and significantly associated with new offense reincarcerations, supervision revocations, and both of these recidivism measures combined. This positive relationship means that higher program integrity was associated with larger reductions in recidivism for halfway house residents relative to the comparison group.

The current study compares recidivism outcomes from an evidence-based CBT program with a standardized curriculum from when the curriculum was and was not fully implemented. The CPAI and CPC were not used to assess this program at the time of full and partial implementation, but there was documentation about which design elements were lost when the program was altered. This evaluation measures the extent to which the loss of those evidence-based components affected the program's ability to reduce recidivism.

### **Moving On**

Moving On is one of a growing number of standardized CBT programs used to treat correctional populations. Unlike most correctional-based CBT programs, Moving On was designed to treat female, not male, offenders (Gehring, Van Voorhis, and Bell, 2010). As female prison populations have continued to grow (Carson, 2014), so too has the recognition that female offenders are both similar to and different from male offenders (Brennan, Breitenbach, Dieterich, Salisbury, and Van Voorhis, 2012; Holtfreter and Wattanaporn, 2013; Makarios, Steiner, and Travis, 2010; Van Voorhis, Wright, Salisbury, and Bauman, 2010; Wright, Van Voorhis, Salisbury, and Bauman, 2012). Male and female offenders share some of the same risk factors and reentry hardships, including past criminal records,

education deficits, and unstable employment histories (e.g., Greiner, Law, and Brown, 2014; Makarios et al., 2010; Smith, Cullen, and Latessa, 2009). However, males and females tend to be incarcerated for different types of offenses (Carson, 2014), and evidence shows that female offenders are more likely to have histories of multiple types of victimization and co-occurring mental health disorders and substance abuse issues (Belknap, 2007; Scroggins and Malley, 2010; Van Voorhis et al., 2010; Wright et al., 2012).

Moving On is a gender-responsive CBT program that focuses on improving communication skills, building healthy relationships, and expressing emotions in a healthy and constructive manner (Gehring et al., 2010; Van Dieten, 2010). The program is delivered in 26 sessions via group and one-on-one discussions, self-assessments, writing exercises, and role-playing and modeling activities. The women are encouraged to set goals for the future and assess their own personal strengths and weaknesses. Each session is designed to last 1.5 to 2 hours (Gehring et al., 2010).

Moving On was initially offered to female offenders in the Minnesota Correctional Facility (MCF)-Shakopee during the fall of 2001 by trained facilitators. Up through 2010, the program was generally offered to offenders on a quarterly basis. Participation in the program was voluntary, and offenders often entered the program during the last half of their confinement period. The program lasted a total of 12 weeks, participants were in class 4 hours per week for a total of 48 hours, and class sizes were relatively small (between 5 and 10 participants).

In 2011, however, a decision was made to begin offering Moving On to offenders shortly after their admission to the MCF-Shakopee. In response to concerns that scheduling offenders for Moving On often seemed to conflict with prison work assignments or participation in other institutional programs, Moving On began to be offered to offenders at the time of intake, or what is referred to as R&O (reception and orientation) at the MCF-Shakopee. Modifying the point at which offenders entered Moving On brought about several substantive changes to the way the programming was delivered. Because R&O generally lasts 3 weeks, the length of Moving On was trimmed from 12 weeks to 3 weeks. Offenders participated 2 hours each day, 5 days per week, for a total of 30 hours.

Although some curriculum was cut in reducing overall classroom time from 48 hours to 30 hours, the main program changes involved the elimination of role-playing, skill-building, and homework exercises. These exercises were removed not only because of the condensed amount of time over which the programming was offered but also because class sizes had greatly expanded to approximately 40–50 offenders per class. The loss of these components also led to the loss of timely reinforcements for each participant's contributions to the group (i.e., recognition and small material rewards), as well as consequences (i.e., redirection and failure to complete the program). The growth in class sizes was attributable, in no small part, to the fact that participation was no longer voluntary; rather, all offenders admitted to the MCF-Shakopee were required to participate in the "watered-down" version of Moving On.

In the fall of 2013, a decision was made to return Moving On to the way it had operated prior to 2011. Currently, the full program (i.e., 48 hours of classroom time over a 12-week period) is being offered on a quarterly basis, participation is voluntary, and class sizes are relatively small (fewer than 10 participants). The one notable difference compared with how it operated prior to 2011 is that risk assessments are now being used to target which offenders should participate in Moving On. In April 2013, the Minnesota Department of Corrections (MnDOC) implemented the Minnesota Screening Tool Assessing Recidivism Risk (MnSTARR), a risk-assessment instrument that has been validated on Minnesota prisoners (Duwe, 2014a). Consistent with the risk principle, offenders with a higher recidivism risk, per the MnSTARR, are being prioritized for participation in Moving On.

To date, only one outcome evaluation of Moving On has been published. By using a sample of female probationers in Iowa, Gehring et al. (2010) compared 190 Moving On participants with 190 similar female probationers who did not participate in any CBT during their probation periods. The treatment and comparison groups were matched on a limited number of characteristics, including judicial district, race, age, risk-assessment scores, and probationary period start times. By comparing rates of four recidivism outcomes after 12 to 30 months of follow-up time, Gehring et al. (2010) found that Moving On participants had significantly lower rates of rearrest and new convictions than the comparison group of probationers. Moving On participants and comparison group members did not have significantly different rates of incarceration, but Moving On participants did have significantly higher rates of technical violations. When limiting the sample to Moving On program completers ( $N = 111$ ) and the same number of matched probationers, Gehring et al. (2010) found that completers had significantly lower rates of rearrest, new convictions, and incarcerations than the comparison group. The difference in rates of technical violations was not significant between the groups.

The results of Gehring et al.'s (2010) analysis are encouraging for Moving On's effectiveness, but this study suffered from two key methodological shortcomings. First, the authors should have used more probationer characteristics to match treatment and control group members, and they could have conducted a more rigorous matching process to ensure balanced treatment and control groups. Second, the authors did not conduct any multivariate analyses to control for the effect of other potential variables on the recidivism outcomes.

Given that Gehring et al.'s (2010) study is the only evaluation of Moving On's effect on recidivism outcomes, limited evidence shows that Moving On works for women correctional populations. However, Moving On's original design and implementation at MCF-Shakopee included multiple elements that contribute to program effectiveness. Table 1 lists 10 evidence-based program characteristics and implementation strategies and tells whether these elements were present during the two phases of implementation that are compared in the ensuing analyses. The early phase of implementation (covering years



T A B L E 1

**Description of Program Components and Strategies Present and Absent During Implementation Phases at MCF-Shakopee**

	Implementation Phase	
	High Fidelity (2001–2010)	Low Fidelity (2011–2013)
<b>Moving On Components and Implementation Strategies</b>		
<b>1. Facilitator Qualifications</b> Facilitators have at least a bachelor's degree or higher in a helping profession, at least 2 years of prior experience working with offender populations, and completed the 5-day Moving On facilitator training.	Yes	Yes
<b>2. Risk, Need, and Responsivity Assessed</b> Offender risk, need, and responsivity assessed by using validated and normed actuarial assessments; participants matched to programming based on assessment results.	No	No
<b>3. Target Higher Risk Offenders</b> At least two thirds of the participants are rated as medium or high risk to reoffend.	No	No
<b>4. Criminogenic Targets</b> Program targets several criminogenic needs with treatment (e.g., antisocial cognition, unhealthy peer and family relationships, and harmful emotional expressions).	Yes	Yes
<b>5. Treatment Approach</b> Program uses an evidence-based treatment approach, including radical behavioral therapy or cognitive-behavioral therapy.	Yes	No
<b>6. Treatment Length</b> The average program length is between 3 and 9 months, and not longer than 1 year.	Yes	No
<b>7. Group Size</b> Group sizes range from 5 to 10 participants; no more than 10 participants per facilitator.	Yes	No
<b>8. Use of Reinforcers and Punishers</b> A range of social or tangible rewards are used to acknowledge progress and accomplishments in the program; sanctions are imposed for antisocial or disruptive behaviors observed during the program.	Yes	No
<b>9. Completion Criteria and Completion Rate</b> Completion of the program requires progress and acquisition of new skills; completion is not based solely on attendance; and a majority of the participants can complete the program, but not every participant necessarily completes the program.	Yes	No
<b>10. Skill Modeling and Training with Graduated Practice</b> Facilitators model skills to use in response to adverse stimuli; participants practice skills by using simulations and role-playing exercises; new skills are practiced in gradually more difficult situations.	Yes	No
<b>Total</b> Percentage of evidence-based components and implementation strategies that were present during the phase of implementation.	80%	20%

2001 to 2010) is referred to as the “high-fidelity” phase, and the latter phase (covering years 2011 to 2013) is referred to as the “low-fidelity” phase. The elements listed in this table are loosely based on the CPC, as well as on general knowledge based on the “what works” literature (Gendreau and Andrews, 1994; Gendreau et al., 1999; Latessa, 2012). Although only 10 items are listed in Table 1, they relate to at least 25 scoring items on the CPC and are the ones most applicable to group treatment programs.

Risk, need, and responsivity assessment instruments were not widely or consistently used within MnDOC during most of the time period covered in this study, so participants were not matched to Moving On based on the results of such assessments (reference item 2 in Table 1). Because risk scores are not available for many of the women included in this study, it is not known whether most of the program participants were medium or high risk to reoffend (reference item 3 in Table 1).

With the exception of qualified facilitators and the targeting of multiple criminogenic needs, Moving On’s second phase of implementation at MCF-Shakopee (the low-fidelity phase) lost many of the evidence-based elements present during the first phase of implementation (the high-fidelity phase), including ideal program length and group size, as well as the use of skill modeling and training with increasing difficulty (reference items 5 through 10 in Table 1). Overall, the high-fidelity phase of implementation included 80% of these items, whereas the low-fidelity phase included only 20% of these items. In addition to providing an evaluation of Moving On’s effectiveness at reducing recidivism by overcoming the methodological shortcomings of the previous study, the current study assesses what effect, if any, the loss of program integrity has on recidivism outcomes.

## Data and Methodology

The population for this study consisted of 4,101 female offenders released from prison in Minnesota between 2003 and 2013. Of these offenders, 216 participated in Moving On prior to 2011 when it was run with integrity. Another 864 offenders participated in the program during the 2011–2013 period when it did not operate with fidelity. The remaining 3,021 inmates did not participate in either version of Moving On.

To determine whether participation in Moving On and, more generally, program integrity had an impact on recidivism outcomes, we used a retrospective quasi-experimental design with three separate sets of comparisons. Our first comparison assessed the effects of participating in Moving On prior to 2011 on recidivism. Therefore, our treatment group for this comparison included the 216 offenders released during the 2003–2013 period who participated in Moving On before 2011. The pool for our comparison group, meanwhile, contained 2,972 female offenders released between 2003 and 2013 who did not participate in Moving On.

Our second comparison examined the impact of the Moving On program offered during the 2011–2013 period on recidivism. The treatment group consisted of the 864 offenders who participated in this version of Moving On and were released prior to 2014.

Nearly all of the female offenders who were admitted to prison between 2011 and 2013 participated in Moving On. In fact, given that only 49 did not participate, mainly because they had brief lengths of stay in prison, it was not possible to construct a contemporaneous comparison group of nonparticipants. As a result, we relied on a historical comparison group pool that contained the same 2,972 nonparticipants used for the first comparison.

For the third comparison, we assessed the effects of participating in Moving On both before 2011 and during the 2011–2013 period on recidivism. More specifically, we compared the 216 pre-2011 Moving On participants with the 864 offenders who participated in the program between 2011 and 2013. In our analyses, the pre-2011 participants comprised the treatment group, whereas the 2011–2013 participants comprised the comparison group.

In an effort to control for observable selection bias, we used propensity score matching (PSM), which we will discuss in more detail, so as to create equivalent comparison groups for all three comparisons. The use of multiple comparisons enables us to draw inferences about the effects of both Moving On and program integrity on recidivism. For example, if Moving On works but program integrity is irrelevant, then we should expect to observe better outcomes from participants in the first two comparisons but no difference between groups for the third comparison. If integrity matters, however, then we should expect to observe better recidivism outcomes from the pre-2011 participants in the first and third comparisons. But if Moving On is ineffective and program integrity does not matter, then we should not expect to observe improved recidivism outcomes in any of the three comparisons.

### *Dependent Variable*

Because there is no best measure of recidivism, we used multiple measures in this study. We operationalized recidivism as a (a) rearrest, (b) reconviction, (c) reincarceration for a new offense, or (d) revocation for a technical violation. Among the first three measures, which strictly quantify new criminal offenses, rearrest provides the most sensitive measure of reoffending because not all rearrests result in a reconviction. New offense reincarceration, on the other hand, offers the most conservative reoffending measure given that offenders who are rearrested and reconvicted for a new offense could receive a probation sentence, for example, rather than a prison sentence. Compared with the three reoffense measures, technical violation revocations (the fourth measure) represent a broader measure of rule-breaking behavior. Offenders can have their postrelease supervision (i.e., parole) revoked for violating the conditions of their supervised release. Because these violations can include activity that might not be criminal in nature (e.g., use of alcohol, failing a community-based treatment program, failure to maintain agent contact, and failure to follow curfew), technical violation revocations do not necessarily measure reoffending.

Recidivism data were collected on offenders through June 30, 2014. Because the offenders in this study were released between January 2003 and December 2013, the follow-up time ranged from 6 months to more than 11 years. Data on arrests and convictions were

obtained from the Minnesota Bureau of Criminal Apprehension, whereas reincarceration and revocation data were derived from the Correctional Operations Management System (COMS)—the MnDOC's database. Because these data measure only arrests, convictions, or incarcerations that took place in Minnesota, the findings presented later likely underestimate the true recidivism rates for the offenders included in this study. We anticipate, however, that the amount of non-Minnesota recidivism will be similar across all treatment and comparison groups.

To measure accurately the total amount of time offenders were actually at risk to reoffend (i.e., "street time"), we accounted for supervised release revocations in the recidivism analyses. For the three recidivism variables that strictly measure new criminal offenses (rearrest, reconviction, and new offense reincarceration), it was necessary to deduct the amount of time they spent in prison for technical violation revocations from their total follow-up period. Failure to deduct time spent in prison as a supervised release violator would artificially increase the length of the at-risk periods for these offenders. Therefore, to achieve a more accurate measure of "street time," the time an offender spent in prison as a supervised release violator was subtracted from her follow-up period, but only if it preceded a reoffense or if the offender did not recidivate prior to July 1, 2014. Similarly, to measure "street time" accurately for the technical violation revocation measure, we accounted for the time an offender spent in prison for a new felony offense, which was deducted from the follow-up period as long as it preceded a revocation or if the offender had not been revoked by the end of June 2014.

### *Independent Variables*

Participation in Moving On is the key variable of interest in this evaluation. Offenders who participated in Moving On were assigned a value of "1," whereas the offenders in the comparison group were given a value of "0." In the comparison between pre-2011 and 2011–2013 Moving On participants, the former were given a value of "1," whereas the latter received a value of "0." The independent, or control, variables included in the statistical models were those that were not only available in COMS but also might have an impact on recidivism and Moving On program selection (see Table 2).

We included several measures commonly associated with recidivism risk, such as the offender's race, age, number of prior supervision failures, number of prior convictions, number of felony convictions, and institutional misconduct. Previous research on Minnesota prisoners has shown that suicidal history increases an offender's risk for recidivism (Duwe, 2014a). We also accounted for admission type (new commit), offense type, commitment county (metro), and length of stay because prior studies have indicated these variables are significant predictors of recidivism for Minnesota prisoners (Duwe, 2010; Duwe and Clark, 2013).

In addition to including factors that increase the likelihood of recidivism, we accounted for factors that have been shown to decrease recidivism risk, such as prison visits (Duwe and

T A B L E 2

Logistic Regression Models for Moving On Program Selection

Predictors	Predictor Description	1		2		3	
		OR	SE	OR	SE	OR	SE
Age at Intake (years)	Offender age in years at time of admission to prison	0.987	0.008	0.997	0.005	0.986	0.010
Sentence Length (months)	Sentence length in months	1.000	0.001	0.982**	0.003	1.041**	0.005
Minority	Minority = 1; White = 0	1.130	0.159	0.974	0.091	1.155	0.203
Metro Commit	Twin Cities metropolitan area = 1; greater Minnesota = 0	1.092	0.151	0.888	0.088	1.150	0.193
Supervision Failures	Number of prior revocations while under correctional supervision	0.688*	0.153	0.451**	0.145	1.328	0.228
Total Convictions	Total number of convictions, including index conviction(s)	0.994	0.012	1.052**	0.006	0.974	0.015
Felony Convictions	Total number of felonies, including index conviction(s)	1.074*	0.029	0.819**	0.028	1.208**	0.048
Offense Type	Other offense serves as the reference						
Violent	Violent offense = 1; nonviolent offense = 0	3.092**	0.306	1.084	0.188	1.830	0.371
Drugs	Drug offense = 1; nondrug offense = 0	1.184	0.300	1.125	0.163	0.713	0.355
Property	Property offense = 1; nonproperty offense = 0	1.084	0.317	1.054	0.168	0.889	0.371
Felony DWI	Felony DWI offense = 1; nonfelony DWI offense = 0	2.111	0.401	1.149	0.249	1.129	0.479
Admission Type	New Court Commitment serves as the reference						
Probation violator	Probation Violator = 1; new commit and release violators = 0	0.681	0.217	3.915**	0.176	0.313**	0.307
Release violator	Release Violator = 1; new commit and probation violators = 0	0.174**	0.521	0.083**	0.749	1.947	1.047
Secondary Degree	Secondary degree at intake = 1; less than secondary degree = 0	1.398*	0.160	1.374**	0.088	1.012	0.205
Postsecondary Degree	Postsecondary at intake = 1; less than postsecondary degree = 0	2.222	0.654	0.801	0.711	1.549	1.207
Constant		0.081**	0.395	0.378**	0.239	0.130**	0.518
N		3,188		3,866		1,080	
Log-likelihood		1457.105		3527.189		787.710	
Area Under Curve (AUC)		0.708		0.750		0.835	
Nagelkerke R <sup>2</sup>		0.097		0.209		0.376	

Notes. Comparison 1: Pre-2011 Moving On versus contemporaneous comparison group. Comparison 2: 2011–2013 Moving On versus historical comparison group. Comparison 3: Pre-2011 Moving On versus 2011–2013 Moving On. OR = odds ratio; SE = standard error.

\**p* < .05. \*\**p* < .01.

Clark, 2013), participation in the Challenge Incarceration Program (CIP, a correctional boot camp program [Duwe and Kerschner, 2008]), and involvement in programming relating to chemical dependency treatment (Duwe, 2010), education (Duwe and Clark, 2014), employment (Duwe, 2012), and work release (Duwe, 2014b). Combined, the covariates we used tap into several risk factors such as antisocial history (prior supervision failures, criminal history, and prison misconduct), social support (prison visits), antisocial cognition (chemical dependency treatment and CIP are delivered within a cognitive-behavioral framework), education and employment (educational programming, employment programming, and participation in work release), and substance abuse (chemical dependency treatment).

### *Propensity Score Matching*

PSM is a method that estimates the conditional probability of selection to a particular treatment or group given a vector of observed covariates (Rosenbaum and Rubin, 1985). The predicted probability of selection, or propensity score, is typically generated by estimating a logistic regression model in which selection (0 = no selection; 1 = selection) is the dependent variable and the predictor variables consist of those that theoretically have an impact on the selection process. Once estimated, the propensity scores are used to match individuals who participated in an intervention with those who did not. In matching the offenders who entered Moving On with those who did not on the conditional probability of selection into the program, the main advantage with using PSM is that it can simultaneously “balance” multiple covariates on the basis of a single composite score. In doing so, PSM helps create a counterfactual estimate of what would have likely happened to the offenders in the Moving On group had they not participated in the program.

Despite its growing popularity as a matching technique, PSM has several limitations that are worth noting. First, and most important, because propensity scores are based on observed covariates, PSM cannot control for “hidden bias” from unmeasured variables that are associated with both the assignment to treatment and the outcome variable. Second, for PSM to be effective, there must be substantial overlap among propensity scores between the treatment and comparison groups (Shadish, Cook, and Campbell, 2002). If the overlap is insufficient, then the matching process will yield incomplete or inexact matches. Finally, PSM is generally more effective with larger samples (Rubin, 1997).

In addition to using a large sample ( $N = 4,101$ ), we tried to address the “hidden bias” limitation, to the extent possible, by including a relatively lengthy list of theoretically relevant covariates in our statistical models. Moreover, the matching for the first two comparisons was largely successful, which reflects the fact that the overlap in propensity scores was sufficient. Achieving complete and exact matches for the third comparison was more difficult, however, because of the greater separation in propensity scores between the two groups of Moving On participants. As discussed next in more detail, we used multiple matching methods along with covariate and propensity score adjusted Cox regression models.

### *Matching for Moving On Selection*

For each of the three sets of comparisons, we calculated propensity scores by estimating a logistic regression model in which the dependent variable was participation in Moving On. The variables included in a propensity score estimation model should consist of those related to the outcome—even if it is a weak association—that affect treatment selection and are not caused by the treatment (Shadish et al., 2002). As we described previously, the point at which offenders entered Moving On during their confinement varied between the pre-2011 and 2011–2013 periods. More specifically, because Moving On participants from 2011–2013 entered the program toward the beginning of their incarceration, most of the covariates pertaining to participation in programming (e.g., chemical dependency treatment, EMPLOY, etc.) and postrelease supervision (e.g., intensive supervised release and discharge) do not temporally precede their involvement in Moving On. Although Moving On participation is not one of the criteria MnDOC staff consider in making programming and supervision level decisions for female offenders (e.g., whether an offender is placed on intensive supervised release at the time of release is not caused by participation in Moving On), it is possible that Moving On could have affected measures such as institutional misconduct (i.e., discipline convictions).

We therefore estimated a propensity score estimation model that contained only the covariates that would be known at the time of intake and, thus, would precede potential selection into Moving On across both time periods. Yet, to address the possibility that these covariates might not include all of the variables that affected selection, particularly for the pre-2011 period, we also estimated a propensity score estimation model that included all of the covariates we examined. As we note later on, both approaches yielded similar results regarding Moving On's impact on recidivism. Consequently, we focus on the results pertaining to the propensity score models that included only the covariates known at the time of intake.

Table 2 describes the covariates used in the propensity score estimation models, and it presents the results from these analyses. The results show several factors that predicted selection for each of the three comparisons we examined. For the first comparison, the results reveal that the odds of participating in pre-2011 Moving On were significantly greater for offenders incarcerated for a violent offense and inmates with more felony convictions. The odds were significantly less, however, for offenders with supervision failures and those admitted to prison as a release violator. For the second comparison, the likelihood of participating in Moving On from 2011 to 2013 was significantly greater for offenders with more total convictions, probation violators, and offenders who entered prison with a secondary degree (i.e., high-school degree or GED). The odds of participation were significantly lower, however, for offenders who had shorter sentences, more supervision failures, and a greater number of felonies, and for those who were admitted to prison as release violators. For the third comparison, the chances of participating in pre-2011

Moving On were significantly greater for offenders with longer sentences and a larger number of felony convictions. Conversely, the odds were significantly less for probation violators.

After obtaining propensity scores for the three sets of comparisons, a “greedy” matching procedure that used a without-replacement method was used to match the offenders from the treatment and comparison groups. For the first two comparisons, Moving On participants were individually matched to a comparison group of nonparticipants who had the closest propensity score (i.e., “nearest neighbor”) within a relatively narrow caliper (i.e., range of propensity scores) of 0.05. We obtained a match rate of 99.5% for the treatment group offenders in these two comparisons. For example, of the 216 pre-2011 Moving On participants, we found a comparison group match for all but one of the offenders. For the second comparison, we found matches for 860 of the 864 Moving On participants from 2011 to 2013.

With the third comparison, however, it was more difficult to produce a high rate of exact matches because of the lack of strong overlap in propensity scores between pre-2011 and 2011–2013 Moving On participants. Indeed, we obtained matches for only 80% of the pre-2011 participants when using a .05 caliper. To avoid bias resulting from incomplete matching, we used nearest-neighbor matching in which we matched all 216 of the pre-2011 participants with 216 participants from the 2011–2013 period.

In Table 3, we present statistics that measure the degree to which PSM was effective in reducing observable selection bias for the three comparisons. We use a measure (“Bias”) developed by Rosenbaum and Rubin (1985) that quantifies the amount of bias between the treatment and comparison samples (i.e., standardized mean difference

$$\text{Bias} = \frac{100(\bar{X}_t - \bar{X}_c)}{\sqrt{\frac{(S_t^2 + S_c^2)}{2}}}$$

between samples), where  $\bar{X}_t$  and  $S_t^2$  represent the sample mean and variance for the treated offenders and  $\bar{X}_c$  and  $S_c^2$  represent the sample mean and variance for the untreated offenders. If the bias value exceeds 20, then the covariate is considered to be unbalanced (Rosenbaum and Rubin, 1985).

Prior to matching, there were five imbalanced covariates for the first comparison, four for the second comparison, and five for the third comparison. After matching, the results presented in Table 3 show that all 15 covariates (plus the propensity score) had bias values below 20 for the first two comparisons. But for the third comparison, we find that sentence length (plus the propensity score) had a bias value greater than 20.

### *Analysis*

Given that recidivism is typically operationalized as a binary outcome, multiple logistic regression is a popular technique for recidivism analyses. One key assumption that logistic



TABLE 3

## Covariate Balance for Moving On Selection

Variable	1			2			3		
	MO 1	Comparison	Bias	MO 2	Comparison	Bias	MO 1	MO 2	Bias
Propensity Score	0.11	0.11	0.09	0.33	0.32	1.40	0.44	0.32	<b>35.92</b>
Age at Intake (years)	32.85	33.15	2.70	32.68	32.72	0.35	32.81	32.47	3.03
Sentence Length (Months)	47.50	50.17	2.24	20.47	20.41	0.28	47.40	35.69	<b>30.23</b>
Minority	0.42	0.45	4.94	0.36	0.37	1.70	0.42	0.41	1.65
Metro Commit	0.48	0.46	3.26	0.39	0.46	11.75	0.48	0.47	1.63
Supervision Failures	0.53	0.54	1.14	0.83	0.85	3.23	0.53	0.50	3.45
Total Convictions	8.52	8.92	3.68	8.97	9.08	1.15	8.64	8.83	1.85
Felony Convictions	3.37	3.49	2.79	2.27	2.30	1.37	3.52	2.89	13.67
Violent Offense	0.31	0.30	1.76	0.13	0.12	2.47	0.31	0.24	12.61
Drug Offense	0.32	0.31	1.75	0.43	0.44	1.65	0.32	0.36	6.92
Property Offense	0.23	0.24	1.92	0.32	0.32	0.00	0.24	0.22	3.88
DWI Offense	0.07	0.10	9.24	0.05	0.04	3.88	0.06	0.10	12.34
Probation Violator	0.34	0.34	0.00	0.78	0.78	0.00	0.34	0.38	6.82
Release Violator	0.02	0.03	5.23	0.00	0.00	0.00	0.02	0.01	6.03
Secondary Degree at Intake	0.68	0.71	5.28	0.63	0.66	5.10	0.68	0.66	3.47
Postsecondary Degree at Intake	0.01	0.00	7.78	0.00	0.00	0.00	0.01	0.00	7.78

Notes. Comparison 1:  $N = 430$  (Moving On 1 = 215; comparison group = 215). Comparison 2:  $N = 1,760$  (Moving On 2 = 860; comparison group = 860). Comparison 3:  $N = 432$  (Moving On 1 = 216; Moving On 2 = 216).

regression makes in analyzing recidivism is that offenders have follow-up periods that are equal in length. When they vary in length, however, the shortest observed follow-up period must be used to meet this assumption. For example, because the follow-up periods in this study ranged from 6 months to 11 years, we would need to limit the follow-up period to 6 months for all offenders in order to use logistic regression for our recidivism analyses. In addition to resulting in a significant loss of outcome data, the use of such a brief follow-up period for recidivism would weaken our ability to draw valid conclusions about the effectiveness of Moving On or the importance of program integrity.

Because survival analysis models are designed to handle censored observations, they can accommodate follow-up periods that vary in length. Therefore, we used Cox regression, a multivariate survival analysis technique, for our recidivism analyses. Cox regression relies on time-dependent data, which are important in determining not only whether offenders recidivate but also when they recidivate. More specifically, it uses both “time” and “status” variables in estimating the impact of the independent variables on recidivism. For the analyses presented in this study, the “time” variable measures the amount of time (in days) from the date of release until the date of first rearrest, reconviction, new offense reincarceration, technical violation revocation, or June 30, 2014, for those who did not recidivate. The “status” variable, meanwhile, measures whether an offender

**T A B L E 4**

**Recidivism Rates for Moving On Participants and Comparison Group Offenders**

Comparison	Rearrest, %	Reconviction, %	New Offense Reincarceration, %	Technical Violation Revocation, %
Comparison 1 ( <i>N</i> = 430)				
Pre-2011 Moving On	49.3	34.9	16.3	23.7
Comparison group	62.8	48.4	20.5	27.4
Comparison 2 ( <i>N</i> = 1,720)				
Moving On, 2011–2013	42.9	25.1	6.9	20.9
Comparison group	71.6	55.6	21.7	25.3
Time-adjusted rate	39.2	24.1	4.8	19.9
Comparison 3 ( <i>N</i> = 432)				
Pre-2011 Moving On	49.1	34.7	16.2	23.6
Time-adjusted rate	19.4	7.9	2.3	16.2
Moving On, 2011–2013	31.0	16.7	4.2	19.9

recidivated (rearrest, reconviction, new offense reincarceration, and technical violation revocation) during the period in which she was at risk to recidivate.

**Results**

In Table 4, we present the recidivism rates for the offenders in the three comparisons we analyzed. In the first comparison, which contains 215 pre-2011 Moving On participants and a contemporaneous comparison group of 215 nonparticipants, we observe that offenders who participated in the program had lower rates for all four recidivism measures, especially rearrest and reconviction. For example, through the end of June 2014, 49% of Moving On participants had been rearrested versus 63% of those in the comparison group. Likewise, 48% of comparison group offenders had been reconvicted compared with 35% of Moving On participants.

Because we used historical comparison groups for the second and third comparisons, simply comparing recidivism rates through June 2014 can be misleading because of the varying lengths of the follow-up periods (i.e., the longer follow-up period, the higher the recidivism rate) between groups. For example, in the third comparison, the average follow-up period length for pre-2011 Moving On participants was 2,445 days (80 months) versus an average of 528 days (17 months) for the 2011–2013 participants. As a result, we also calculated time-adjusted rates for the two groups that had longer follow-up periods (nonparticipants in comparison 2 and pre-2011 Moving On participants in comparison 3). Given that the matching process was performed on an individual basis, we shortened the follow-up periods for the offenders in these two groups so that it was

commensurate with the length of the follow-up period for their 2011–2013 Moving On counterparts.

To illustrate, for the second comparison, let us assume that a nonparticipant in the comparison group had a follow-up period of 1,825 days (approximately 5 years), whereas her matched counterpart in the 2011–2013 Moving On group had a follow-up period of 730 days (approximately 2 years). For the nonparticipant in the comparison group, we calculated her recidivism rates based on a 730-day follow-up period. We performed this calculation for all 860 nonparticipants in the second comparison and all 216 pre-2011 Moving On participants in the third comparison.

For both the second and third comparisons, we find that the groups with the longer follow-up periods (nonparticipants in the second comparison and pre-2011 Moving On participants in the third comparison) had much higher recidivism rates. When we examine the time-adjusted rates, however, we observe little difference in recidivism for the second comparison between the 2011–2013 Moving On participants and the matched comparison group of nonparticipants. Moving On participants had rates that were slightly higher than the time-adjusted rates for their comparison group counterparts for all four recidivism measures. For the third comparison, we observe that the time-adjusted rates for the pre-2011 Moving On participants are lower than their 2011–2013 Moving On counterparts for all four recidivism measures.

### *Effects of Moving On and Program Integrity on the Hazard of Recidivism*

To determine the effects of Moving On and program integrity on recidivism, we estimated Cox regression models for each recidivism measure across all three comparisons, resulting in 12 models total.<sup>1</sup> Each model contains covariates known to be associated with recidivism that were excluded from the propensity score estimation models because they follow entry into Moving On, at least for the 2011–2013 participants. As indicated previously, although we obtained complete matches for our third comparison (pre-2011 participants versus 2011–2013 participants), the matches were inexact because of a lack of covariate balance. Therefore, in the third comparison, we estimated models that included the propensity score, which can be conceptualized as a single covariate that approximates adjusting for all of the covariates in the propensity score estimation model because it encapsulates the distribution of these covariates (Austin, 2014). For the second and third comparisons, we estimated additional Cox regression models that used the time-adjusted follow-up periods discussed earlier. We do not present the results from these additional models, however, because they were largely the same as those produced from the models that used the full follow-up period. Still, these findings can be obtained from the authors upon request.

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1. For all of the models we estimated, we assessed the proportional hazards assumption by including a time-dependent covariate for Moving On participation.

As shown in Table 5, the results indicate that, by controlling for the effects of the other covariates, participating in Moving On prior to 2011 significantly reduced two of the four recidivism measures in the first comparison models, lowering the risk of reoffending by 31% for rearrest and 33% for reconviction. The hazard ratios were in the negative direction for new offense reincarceration and technical violation revocations, but neither one was statistically significant at the .05 level in any of the three models.<sup>2</sup>

For the second comparison (2011–2013 Moving On participants vs. a historical comparison group of nonparticipants), the results from all three models indicated that Moving On participation did not have a significant effect on any of the four recidivism measures. Although the hazard ratio was in the negative direction for technical violation revocations, it was in the positive direction for the other three measures.<sup>3</sup>

For the third comparison, which compares pre-2011 and 2011–2013 Moving On participants, the results are largely similar to those observed for the first comparison. Compared with 2011–2013 Moving On participants, the risk of rearrest and reconviction was significantly lower for pre-2011 Moving On participants in all three models. More precisely, the hazard of reoffense was 44% lower for rearrest and 47% lower for reconviction.<sup>4</sup> As with the first comparison, significant effects were not observed for either new offense reincarceration or technical violation revocations.<sup>5</sup> Although the hazard ratio was in the

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2. As noted, we carried out an additional PSM analyses in which we used all of the covariates (except for age at intake, sentence length, and educational degrees at intake) in the propensity score estimation model. For the first comparison, matches were obtained for 215 Moving On participants, and none of the covariates had bias values above 20. The results from a bivariate Cox regression model showed that Moving On significantly reduced the risk of rearrest (31% reduction) and reconviction (38% reduction). Significant effects were not found for either new offense reincarceration or technical violation revocations.
  3. In the additional PSM analyses for the second comparison, we obtained matches for 861 Moving On participants and all of the covariates were balanced. The results were similar, as the 2011–2013 version of Moving On did not have a significant effect on any of the recidivism measures. The only difference is that the direction of the hazard ratio was negative for reconviction and positive for technical violation revocations.
  4. In the additional PSM analyses for the third comparison, we also used nearest-neighbor matching because of the incomplete matches that resulted from matching with a .05 caliper. Because four of the covariates (probation violator, discipline, length of stay, and visited) had bias values greater than 20, we estimated models with and without the propensity score. Neither reincarceration measure was statistically significant in either model. Participation in pre-2011 Moving On significantly reduced the risk of rearrest, lowering it from 38% to 42% in the two models. Similarly, pre-2011 Moving On participation significantly decreased the hazard of reconviction, reducing it from 49% to 58% in the two models.
  5. We also estimated models for this comparison in which we excluded the propensity score. The results were virtually the same, with significant effects for rearrest (44% reduction in the hazard) and reconviction (47% decrease in the hazard) and nonsignificant findings for both reincarceration measures.

T A B L E 5

**Cox Regression Models: Impact of Moving On Program Participation on the Hazard of First Recidivism Event**

Hazard Ratios by Comparisons and Type of Recidivism

Predictors	Rearrest			Reconviction			New Offense Recarceration			Technical Violation Revocation		
	1	2	3	1	2	3	1	2	3	1	2	3
Moving On (pre-2011)	0.695**		0.562**	0.671*		0.527**	0.828		0.666	0.945		1.299
Moving On (2011–2013)		1.125			1.026			1.232			0.874	
Propensity Score			1.126			1.283			2.291			0.869
Age at Release (years)	0.970**	0.980**	0.987	0.971**	0.980**	0.993	0.971*	0.961**	0.991	0.980	0.980**	1.001
Length of Stay (Months)	0.992	0.966**	0.987	0.992	0.970**	0.989	0.995	0.984	0.990	0.992	0.987	0.986
Prison Discipline Convictions	1.007	1.018**	1.014**	1.012*	1.012**	1.017**	1.009	1.014*	1.004	1.009*	1.021**	1.013*
Suicidal Tendencies	1.480**	1.119	1.085	1.219	1.029	1.109	1.467	1.461**	1.304	2.440**	1.762**	1.390
Visited in Prison	0.676*	0.882	0.644*	0.535**	0.980	0.498**	0.587*	0.792	0.570	0.905	0.566**	0.421**
Earned Secondary Degree in Prison	1.116	0.998	1.016	0.931	0.939	0.668	0.975	0.895	0.651	0.683	0.635	0.882
Earned Post-Secondary Degree in Prison	0.855	0.659	0.899	0.765	0.667	0.486	1.206	0.697	0.580	0.566	0.865	0.937
Entered Chemical Dependency TX	0.839	1.187	0.724	0.858	1.197	0.591	0.616	1.045	0.630	1.158	0.573**	1.085
Entered EMPLOY Program	0.931	0.849	1.041	0.835	0.783	1.213	0.608	0.625	0.586	0.418	0.745	0.487
Entered Work Release Program	0.964	0.628**	0.631*	1.003	0.501**	0.525*	0.831	0.492**	0.619	1.718*	1.250	1.146
Entered CIP	0.981	0.513**	0.625	0.834	0.322**	0.432	0.616	0.535	0.236	1.775	2.197**	1.473
Placed on ISR	1.081	0.638	0.798	0.634	0.768	0.464	0.740	0.808	0.508	2.858**	1.076	1.997*
Discharge	2.320*	0.931	1.839	2.823*	1.205	3.261	3.173*	2.225	4.294			
Supervised Release Revocations	1.149	1.002	1.664**	1.335*	1.027	1.561*	1.253	1.063	1.346			
New Offense Recarcerations										0.683	0.852	0.623
N	430	1,720	432	430	1,720	432	430	1,720	432	422	1,709	429

Notes. Comparison #1: Pre-2011 Moving On versus contemporaneous comparison group. Comparison #2: 2011–2013 Moving On versus historical comparison group. Comparison #3: Pre-2011 Moving On versus 2011–2013 Moving On.  
\**p* < .05. \*\**p* < .01.

negative direction for new offense reincarceration, it was in the positive direction for technical violation revocations.<sup>6</sup>

The results also showed the hazard ratio was significantly greater for offenders with more institutional discipline convictions (9 of the 12 models), younger offenders (7), offenders with suicidal tendencies (4), and inmates with shorter lengths of stay in prison (2). Offenders placed on intensive supervised release (ISR) had a significantly greater hazard of revocation for the first and third comparisons, whereas those with supervised release revocations had a greater risk of subsequent reoffending in three models. Similarly, offenders who were discharged (i.e., released to no correctional supervision because they completed their sentence) had an increased risk of reoffending in three models. Participation in work release and CIP increased the hazard of revocation in one model, and it decreased the risk of recidivism in several models. Finally, offenders who received prison visits had a reduced hazard of recidivism in seven models, and the risk of revocation was lower for CD treatment participants in one model.

## Discussion

The results suggest that Moving On was generally effective in reducing recidivism prior to 2011. Although significant effects were not observed for either reincarceration measure, pre-2011 participation in Moving On lowered the risk of rearrest and reconviction. The findings further showed that between 2011 and 2013, Moving On did not have a significant effect on any of the four measures of recidivism. The results from the first two comparisons were confirmed by the third comparison, which indicated that recidivism outcomes—particularly for rearrest and reconviction—were significantly better for pre-2011 participants in comparison with those who participated in Moving On during the 2011–2013 period.

Overall, the findings suggest that Moving On can be an effective correctional program for female offenders. But the results also imply that its effectiveness hinges on whether it is implemented with fidelity, which provides support for the notion that program integrity matters when it comes to reducing recidivism. Indeed, when the operation of Moving On was largely consistent with how it was designed, the program significantly lowered the risk of rearrest and reconviction. But when parts of the curriculum were cut, the

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6. To avoid biased estimates, unreliable confidence interval coverage, and convergence problems in logistic regression models, Penduzzi, Cocato, Kemper, Holford, and Feinstein (1996) recommended a rule of thumb of 10 events per variable (EPV) based on the simulation results from their study. In a more recent simulation study by Vittinghoff and McCulloch (2007), the authors reported that the EPV standard could likely be cut in half to five predictors per event. Given the modest sample size for our third comparison ( $N = 432$ ), combined with the relatively low overall rate for new offense reincarceration for this comparison (10%), the EPV was less than five for this model. We estimated models in which the EPV was higher than either threshold (5 or 10), but the results were not substantively different than those reported in Table 5.

length of the program was shortened and class sizes were far bigger than recommended during the 2011–2013 period; thus, participation in Moving On ceased to reduce reoffending. As the quality of the intervention was diluted, so was its beneficial impact on recidivism.

## **Conclusion**

Several limitations with this study are worth noting. First, we focused on the effectiveness of a specific correctional program among a sample of female offenders who were incarcerated in Minnesota's prison system. As a result, the findings might not be generalizable to other correctional programs, other offender populations (e.g., probationers or male offenders), or offenders from other jurisdictions.

Second, historical comparisons are generally weaker than contemporaneous ones, and we relied—out of necessity—on historical comparison groups for two of the three comparisons we analyzed. Despite our use of multiple comparison groups, it is possible that the results observed in this study could be influenced by factors unique to the offenders in the 2011–2013 Moving On group that we could not control.

Third, although we documented the differences in program integrity between the two time periods we examined, we could not determine whether some or all of these differences were responsible for the recidivism outcomes we observed. On the one hand, it is possible that the large class sizes for Moving On during the 2011–2013 period, rather than the abbreviated curriculum, weakened its impact on recidivism. On the other hand, the virtual absence of role-playing exercises could have been the culprit for the worse recidivism outcomes. Or the timing as to when the programming was provided to offenders could have made a difference because it was offered much earlier during an offender's incarceration period (at intake) for the 2011–2013 participants.

Fourth, and perhaps most important, we could not control for either offender motivation or whether they volunteered for Moving On. Recall that participation in the program was voluntary prior to 2011, whereas it was mandatory between 2011 and 2013. To be sure, it is possible that the reason for the better recidivism outcomes before 2011 is a result of volunteerism rather than of program integrity. Existing research provides mixed evidence, however, on the impact that volunteerism has on treatment effectiveness. Findings from the substance abuse and sex offender treatment literature suggest that mandatory interventions can be just as effective as voluntary programming (Anglin, Brecht, and Maddahian, 1989; Grady, Edwards, Pettus-Davis, and Abramson, 2012; Knight, Hiller, Broome, and Simpson, 2000; McSweeney, Stevens, Hunt, and Turnbull, 2007; Mitchell, Wilson, and MacKenzie, 2007). In contrast, the results from the meta-analysis by Parhar, Wormith, Derksen, and Beaugard (2008) indicate that voluntary correctional programs produce better recidivism outcomes than those that are mandatory or coercive. As Parhar et al. (2008) acknowledged,

however, their study did not control for factors such as program integrity, treatment intensity, or offender recidivism risk.

### *Implications for Correctional Policy and Practice*

Given these limitations, we cannot definitively conclude that the better recidivism outcomes for the pre-2011 participants were entirely a result of greater program integrity. At the same time, however, this study is one of the first to examine closely the relationship between program fidelity and reoffending. Although future research is needed to arrive at stronger conclusions about the importance of program integrity, we believe the findings still carry several important implications for correctional research, policy, and practice.

First, the results provide additional evidence that cognitive-behavioral programming can be effective in reducing recidivism for offenders. More narrowly, given the consistency between our findings and those from the only other evaluation of Moving On (Gehring et al., 2010), the evidence suggests that the gender-responsive program can successfully lower recidivism for female offenders.

Second, this study offers additional, albeit qualified, support for the idea that program integrity matters. It has long been true that many correctional programs fail to work because they are not rooted in sound criminological theory and, thus, exemplify “correctional quackery” (Latessa, Cullen, and Gendreau, 2002). It is also true, however, that a common reason for the failure of programs, including those with a solid theoretical foundation, is a lack of therapeutic integrity (Cullen and Gendreau, 2000). Scholars have argued that some of the variation in effectiveness observed among meta-analyses of correctional programs likely stems from a lack of program integrity (Cullen, 2002; Gendreau, 1996).

Although our research is a microcosm of this broader point about the association between program integrity and effectiveness, it highlights the importance of accounting for program integrity when interpreting the results from individual program evaluations. For example, had we focused only on the 2011–2013 period and assumed the program operated with integrity, we would have been left with the erroneous conclusion that Moving On does not work. Although “black box” evaluations serve their purpose by helping identify what works within corrections, it is also important to look inside the box to understand more clearly why programs fail or succeed.

Third, this evaluation provides evidence that correctional practitioners can take an effective intervention and make it ineffective. The change made to Moving On in 2011 helped ease concerns over scheduling offenders for other institutional programming, but it also led to the implementation of an unsuccessful program that was inconsistent with its original design. The reasons why a program lacks integrity, however, might not always be unintentional. Anecdotally, we are aware of instances in which practitioners have purposefully altered or “enhanced” evidence-based programs (i.e., programs that had achieved positive outcomes in prior research). Moreover, faced with tight budgets, correctional agencies are frequently under pressure to do more with less, which might include offering the “light,”



shortened version of a program. Yet, cutting corners to reduce costs in the short term might ultimately be cost-inefficient over the long run by producing worse recidivism outcomes. We are not suggesting, however, that local program innovation does not have a place in corrections. Rather, efforts to improve program performance should be conducted within the context of controlled experiments.

Regardless of why a program lacks integrity, we believe this study should be viewed as a cautionary tale for correctional practitioners who modify an intervention without regard to program integrity considerations. Making changes that compromise program integrity can have an adverse impact on recidivism outcomes, as our research suggests. But there are also other, more subtle consequences. As the rehabilitative ideal has made a comeback over the last several decades (Cullen, 2005), correctional agencies have generally embraced the idea of using evidence-based practices, that is, interventions that have been shown to be effective. Indeed, evidence on “what works” with offenders has led to the development of the principles of effective correctional intervention and, more narrowly, the RNR model, which is arguably the prevailing paradigm used within North American correctional systems today. Under the RNR model, one main goal is to direct offenders to effective programming based on assessments of their recidivism risk and criminogenic needs (Andrews and Bonta, 2010). By providing offenders with evidence-based programming that addresses their criminogenic needs, correctional agencies can presumably help increase public safety through a reduction in recidivism.

Although correctional agencies might believe they are lowering recidivism through the use of effective interventions, this reduction is likely to be elusive if the programs are not delivered with integrity. As a result, using evidence-based interventions without verifying whether they have been implemented with fidelity could promote a false sense of effectiveness. But perhaps more important, offering offenders programming that is unlikely to reduce recidivism because it lacks therapeutic integrity is wasteful of correctional resources, which are almost always scarce. Therefore, in the interests of operating more cost-efficient interventions that yield public safety benefits, ensuring the integrity of programming should be a key consideration for correctional agencies.

In late 2013, the MnDOC returned Moving On to the way it operated prior to 2011 but with one notable exception. This time, offenders are being selected for the program based on their likelihood of reoffending, which is consistent with the risk principle. The current version of Moving On within the MnDOC will thus provide another opportunity not only to evaluate program integrity but also to assess whether adherence to the RNR model and, more narrowly, the risk principle matters for recidivism outcomes. Given the relatively scant research on program integrity to date, much more remains to be learned about its relationship with recidivism outcomes. In particular, rigorous evaluations are needed to clarify the degree to which program fidelity affects recidivism outcomes and identify whether there are any conditions under which a lack of integrity could be more or less harmful.

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# Program Integrity and the Principles of Gender-Responsive Interventions

## Assessing the Context for Sustainable Change

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Duwe and Clark (2015, this issue) provide a sophisticated and novel approach to understanding the importance of program integrity in evaluation research. They investigated two time periods in which women inmates from the Minnesota Correctional Facility—Shakopee participated in *Moving On* (Van Dieten, 2010), a cognitive-behavioral and gender-responsive curriculum specifically designed for women offenders. During the first time period between 2001 and 2010, Duwe and Clark determined that *Moving On* was delivered with 80% (high) fidelity, whereas during the second time period (2011–2013), the program was delivered with only 20% (low) fidelity. By comparing recidivism outcomes across multiple comparison groups and under both fidelity conditions, Duwe and Clark demonstrated that *Moving On* produced meaningful reductions in two of the four measures of recidivism when implemented with integrity, but it failed to do so when implemented without integrity. As such, Duwe and Clark's study is a stark reminder that programs may fail to produce reductions in recidivism not because they are ineffective or based on poor theoretical assumptions, but because they are not implemented as program developers intended.

Their evaluation, in my opinion, is all the more important because it focuses on an idea in corrections that has not yet been fully embraced—that a gender-responsive approach for women offenders is necessary to maximize positive outcomes and reductions in recidivism. Despite the increasing evidence that women have unique pathways to crime (Brennan, Breitenbach, Dieterich, Salisbury, and Van Voorhis, 2012; DeHart, Lynch, Belknap,

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Dais-Brailsford, and Green, 2014; Salisbury and Van Voorhis, 2009) and distinguishing criminogenic needs (Holtfreter and Cupp, 2007; Kelly and Bogue, 2014; Van Voorhis, Wright, Salisbury, and Bauman, 2010), a false belief continues to circulate in the field that gender-responsive strategies is a “boutique” topic of questionable or limited worth. On several recent occasions, I have witnessed executive leadership teams from correctional agencies struggle to come to the understanding that pursuing a gender-responsive approach to programming, assessment, and supervision is not a deviation from evidence-based practices but instead is a move *toward* it. Professional colleagues have voiced similar sentiments to me.

Although Duwe and Clark’s (2015) evaluation is only the second to assess the effectiveness of *Moving On* (Gehring, Van Voorhis, and Bell, 2010, produced the first evaluation), the theoretical principles on which the curriculum is based have generated strong empirical support and include principles that go beyond cognitive-behavioral theory and the work of Don Andrews et al. Relational (Baker Miller, 1986; McClean Taylor, Gilligan, and Sullivan, 1995), trauma-centered (Covington, 2008), and strength-based principles (Van Wormer, 1999) are core components of *Moving On* and continue to be critical in driving women offenders toward (or away from) antisocial behavior (Blanchette and Brown, 2006; Brennan et al., 2012; Owen, 1998; Salisbury and Van Voorhis, 2009; Van Voorhis et al., 2010).

Moreover, although Duwe and Clark (2015) are to be strongly commended for highlighting the principle of program integrity, they nevertheless missed the mark in assessing the fidelity of the very features of *Moving On* that make it so unique—its gender-responsive principles.<sup>1</sup> The *Moving On* curriculum was intentionally designed for women offenders and their specific treatment needs using theoretical modalities that attend to their social, cultural, and psychological realities. Testing these differentiating theoretical principles is critical to learning whether we can reduce female offending more effectively.

By primarily using the Evidence-Based Correctional Program Checklist (CPC; Latessa, 2012) and “what works” research literature as a method for operationalizing program integrity, the core, gender-responsive components of *Moving On* were not measured (i.e., see Table 1 in Duwe and Clark, 2015). The CPC is a useful program assessment instrument to assess correctional programs for their adherence (fidelity) to the principles of effective intervention outlined in the “what works” correctional literature—but it is not useful for assessing gender-responsive principles. The CPC and the “what works” literature is primarily based on studies and meta-analyses conducted with male offenders (Van Voorhis, 2012), and it assesses only some of the assumptions that *Moving On* truly encompasses. To the scholars and practitioners who wish to start with women in mind when it comes to policies that affect them, it is hard not to feel like this methodological decision represents either, at

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1. Durlak and DuPre (2008), as well as Dane and Schneider (1998), referred to this as *program differentiation*, which is a core component of implementation reflecting the extent to which a program’s theory and practices can be distinguished from other programs. See Table 1 for the other microlevel components of implementation.



best, an ignorance or, at worst, a dismissal of gender-responsive principles that have formally been in the correctional consciousness for at least 12 years (Bloom, Owen, and Covington, 2003), if not longer.

Nevertheless, it is not my intention in this policy essay to suggest that Duwe and Clark (2015) willfully ignored key components of the program, nor to diminish the implications from their evaluation, because they are indeed important implications. In fact, Duwe and Clark raised the bar in setting a standard for far less responsible (and less ethical) researchers who might have evaluated the *Moving On* program without measuring integrity and concluded, “It doesn’t work, and gender-responsive programs are therefore unnecessary.” The study raises critical points about the science of implementation that need to be analyzed more closely if we are to truly know why some innovations fail, where others succeed. For the remainder of this policy essay, I focus on additional components for scholars to consider when either implementing evidence-based practices or evaluating their overall effectiveness, as well as guidance for measuring fidelity to gender-informed principles.

### **Program Integrity Happens Within a Context**

In a highly interpretable fashion, Duwe and Clarke (2015, this issue) cogently emphasize that the assessment of integrity is an absolute necessity in program evaluation. Nevertheless, researchers must also keep in mind that integrity is but one component of many to the successful implementation of any evidence-based innovation. In thinking about sustaining evidence-based programs, such as *Moving On*, we need to consider not only treatment fidelity, or the extent to which an innovation adheres to the original program curriculum, but also several other elements under the umbrella of *implementation*.

For instance, Durlak and DuPre (2008) analyzed more than 500 studies in prevention and health promotion among children and found that at least 23 contextual factors influence implementation. Their results underscored that effective implementation requires a multilevel, ecological perspective that includes community-level factors (e.g., political context and external funding), provider and staff characteristics (e.g., perceived need for innovation and skill proficiency), organizational capacity (e.g., organizational culture, climate, communication, and leadership), and training and technical support. Durlak and DuPre (2008) also outlined several additional microlevel components of implementation, beyond program integrity, which are shown in Table 1.

Several other researchers investigating the science of implementation have also made similar empirical conclusions and incorporate comparable factors into their models (Fixsen, Blase, Naoom, and Duda, 2015; Greenhalgh, Robert, Macfarlane, Bate, and Kyriakidou, 2005; Stith et al., 2006). For instance, the National Implementation Research Network founded by Karen Blase and Dean Fixsen identify three categories of implementation drivers: (a) competency drivers, (b) organization drivers, and (c) leadership drivers (Fixsen et al., 2015).

TABLE 1

**Implementation Component Descriptions (Microlevel)**

Component	Description
Integrity, fidelity, adherence, or compliance	Extent to which an innovation adheres to the original program
Dosage, quantity, or strength	Amount of the original program that has been delivered
Quality	How well the core components are delivered
Participant responsiveness	Degree to which the participants “buy into” the program
Program differentiation or uniqueness	Extent to which a program’s theory and practices can be distinguished from other programs
Monitoring of control and comparison conditions	The nature and dosage of services received by members of control and comparison groups
Program reach and scope	The rate of involvement and representativeness of participants
Program adaptation or modification	Changes made to the original program during implementation

*Note.* Adapted from Durlak and DuPre (2008) and from Dane and Schneider (1998).

Competency drivers include factors that the criminal justice field has traditionally focused its attention toward when implementing new innovations: recruitment and selection of staff, training, coaching, and technical assistance. However, as Fixsen et al.’s (2015) model elegantly shows in Figure 1, achieving sustainable innovations with quality and integrity requires much more than training appropriate staff to achieve mastery of new material—organizational and leadership drivers must also be considered for long-term, faithful adoption because they affect the environment, or context, in which the evidence-based innovation is implemented. For those of us who have ever conducted training within an organizational culture that could only be described as “soul crushing,” we get this, and we leave knowing our training is not going to last long.

Because we have a strong foundation of evidence-based practices (EBPs) in criminal justice, I feel we have an obligation to assist agencies in understanding how to implement EBPs effectively. Dr. Jody Sundt and I recently incorporated these concepts on a Bureau of Justice Assistance Smart Supervision grant by conducting ImpleMap interviews with community corrections departments across the state of Oregon (Sundt, Salisbury, and Boppre, 2015). By mapping the implementation landscape and drivers for sustainable change across nine counties, we identified both the implementation strengths and opportunities for improvement throughout the state. As a result, the Oregon Department of Corrections and the various county-level community correctional systems can more effectively tailor their implementation strategies surrounding EBPs.

These implementation drivers should be explored to determine their relationship to effective implementation of EBPs and treatment integrity. Furthermore, these drivers are likely to be important when evaluating any innovation, including gender-responsive programs. Nevertheless, as discussed, additional core principles should be considered when evaluating gender-responsive programs.

FIGURE 1

## Fixsen et al.'s (2015) Implementation Drivers



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### Assessing the Principles of Gender-Responsive Interventions

Implementing effective gender-responsive services means “creating an *environment* through site selection, staff selection, program development, content and material that reflects an understanding of the realities of the lives of women in criminal justice settings and addresses their specific challenges and strengths” (Covington and Bloom, 2006: 19; emphasis added). Additionally, the core principles of gender-responsive interventions originally outlined by Bloom et al. (2003) are as follows: (a) acknowledge that gender matters—women’s experiences in this world are fundamentally different than men’s experiences; (b) recognize that environment matters—create a setting based on safety, respect, and dignity; (c) support women’s relational needs—promote healthy connections to children, family, partners, and the community; (d) provide relevant services and supervision—evidence-based programs and supervision should target women’s risk factors in a culturally sensitive manner (e.g., mental health, trauma, addiction, unhealthy relationships, and parenting); (e) address economic and social status—improve women’s economic and social conditions by developing their capacity to be self-sufficient; and (f) build community—establish a system of community supervision and reentry with wraparound, collaborative services.

I am aware of at least four program or policy assessment instruments that have been developed based on these established principles of gender-informed interventions. As such, each can be used as a guide for operationalizing fidelity to key theoretical principles of gender-responsive innovations. Each tool is relatively new and thus has not yet been validated empirically. The first is the Gender Responsive Policy and Practice Assessment (GRPPA) developed by Bloom, Covington, Messina, Selvaggi, and Owen (2014) and published by the National Institute of Corrections. The GRPPA is “a process designed to guide assessment of research-based, gender-responsive policies and practices in jails, prisons, and community corrections programs for women” (Bloom et al., 2014: 2). Its intended use is as an internal planning tool, or self-assessment, by organizations wanting to determine their fidelity to gender-informed principles and practices.

Completion of the GRPPA protocol allows agencies to be more fully prepared for a Gender Informed Practices Assessment (GIPA; National Institute of Corrections, n.d.), which is an externally facilitated process conducted by consultant teams. The GIPA is the product of a cooperative agreement between the National Institute of Corrections and the Center for Effective Public Policy, and it takes several days for consultants to complete. It is primarily to be used with institutional correctional agencies in an effort to provide feedback in areas of strength and improvement that surround gender-responsive practices.

Similar to the GRPPA, the Center for Gender and Justice also published a Gender-Responsive Program Assessment (Covington and Bloom, 2008) designed to be an internal self-assessment for agencies working with women and girls. It consists of seven domains:

- (1) Theoretical foundation and mission statement
- (2) Site and facility
- (3) Administration and staffing
- (4) Program environment and culture
- (5) Treatment planning
- (6) Program development
- (7) Program assessment

Furthermore, Patricia Van Voorhis from the University of Cincinnati is currently developing the Gender Responsive Correctional Program Assessment Inventory (GRCPAI), which is being piloted among several correctional agencies (P. Van Voorhis, personal communication, May 5, 2015). In contrast to the CPC, this gender-informed tool adds the assessment of program dimensions that are pertinent to women offenders, including gender-responsive criminogenic needs (Van Voorhis et al., 2010); gender-informed case management (e.g., *Women Offender Case Management Model*; Millson, Robinson, and Van Dieten, 2010); multidimensional substance abuse programming (used to address the confluence of substance abuse, mental health, and trauma); and wraparound services targeting education for sustainable careers, poverty, trauma, healthy relationships, safety,

and parenting. In contrast to the GIPA, the GRCPAI is designed for community rather than for institutional settings. Additionally, the assessment also targets program qualities that are pertinent to the treatment of both males and females, including leadership, assessment, case management, staff qualifications and training, cognitive-behavioral programming options, and program resources.

In sum, there is no shortage of assessments to use as a guide for measuring fidelity to gender-responsive principles. However, as relatively new instruments, they are still in need of being validated in large-scale studies. As more opportunities become available for evaluating gender-responsive curricula, I sincerely hope these assessments will be sought out to gain a more complete picture of program integrity and implementation quality.

### Conclusion

Duwe and Clark's (2015) evaluation provides an excellent example of how scholars must approach program evaluation in the future—with more research questions in mind than simply whether the intervention significantly reduced recidivism for treatment versus control groups. Inevitably, correctional agencies will continue to modify programs to fit their organizational needs in the manner that occurred with *Moving On* in 2011 at MCF—Shakopee. In their defense, agencies are often forced to adapt to political changes, fluctuations in population, funding streams, public opinion, legislative demands, and so on. My hope is that scholars continue to delve deeper into measuring all levels of implementation and work with agencies to assist them in learning how to sustain evidence-based practices. Until we do, we cannot be surprised when programs drift. And when they do, we should see it as an opportunity for further scientific inquiry, just as Duwe and Clark have done.

Finally, I am under no illusion that some will continue to argue that because women comprise only 7% of the total U.S. incarcerated population (Carson, 2014), we do not need to concern ourselves with women's distinct supervision and treatment needs. Or if we do, it is only insofar as we address it as a specific responsivity issue (the complexities of which have yet to be cogently outlined in the "what works" literature).

However, we cannot blind ourselves to the fact that even though women encompass a far smaller proportion of the offender population, traditional, male-based policies and programs affect each and every woman offender 100%—not 7%. If we were to reverse course and apply female-centric policies based on the 7% population of women to the 93% population of men with comparable limited empirical scrutiny, then it would be no more unethical (and scientifically indefensible) than what we currently do. In short, I applaud Duwe and Clark's efforts and encourage other scholars to consider evaluating gender-responsive programs with additional measures of implementation and integrity.

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# Rethinking Program Fidelity for Criminal Justice

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Criminal justice program evaluation has long been oriented around reduction objectives determined through quasi-experimental and related variable analytic designs (Petrosino and Soydan, 2005; Shover, 1979). Such purely quantitative approaches relate program impacts considered indicative of program effectiveness but neglect nonmeasured program drivers informing why or how outcomes are realized. More often than not, outcome evaluation in criminal justice imprudently assumes that program results are a function of treatment or intervention *sans* empirical confirmation. Helpfully, mixed-methods approaches coupling process and outcome phases have migrated from other disciplines and offer a more rigorous and scientific strategy for determining program efficacy.

Mixed-methods research in the milieu of applied criminology and criminal justice science, unfortunately, is generally underutilized as the objectives and design requirements of the process phase are poorly articulated and blurred with the functions of pure qualitative research. Although applied fieldwork enables an exploration of phenomena and contextualization of quantitative findings, process evaluation uses qualitative techniques to capture data for confirmatory as well as ethnographic purposes. Accordingly, the foremost objective of process evaluation is to ascertain *program fidelity*, a concept informing whether treatment services are delivered consistent with program theory and design.

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Hyperfocused on outcome design sophistication (e.g., propensity score matching and regression discontinuity modeling), criminal justice program evaluation has largely failed to understand either the singular value of process research or the methodological interdependence between process and outcome phases. Consequently, most extant process evaluations do not adequately address program fidelity, as reflected by inconstant conceptualization quality and even worse field execution. Duwe and Clark's (2015, this issue) study is a rare example of criminal justice program evaluation research featuring attention to program integrity, a synonym for program fidelity, and the point of departure for the balance of this policy essay.

A brief examination of fidelity research in criminology and criminal justice suggests the need for a more comprehensive conceptual framework attentive to multiple aspects of program design and delivery that, in turn, informs instrumentation. Accordingly, the *Justice Program Fidelity Scale* (a customizable tool informed by the extant literature and revised over several applications in federally funded evaluations between 2008 and 2014) is presented to provide an example of robust and systematic measurement of implementation intensity and modality adherence demonstrated by criminal justice programs. We conclude with consideration of other needs and action steps to normalize fidelity assessment moving forward.

### **Program Fidelity Research in Applied Criminology and Criminal Justice**

Fidelity is the extent to which delivery of an intervention, modality, or treatment adheres to program design (i.e., theory and delivery protocol). As programs are implemented and delivered in real-world settings, practical issues, politics, and unanticipated developments can prompt program innovation and adaptation (Blakely et al., 1987). It is thus vital to consider whether changes occurred during program start-up and then over the life of a program so that outcomes can be attributed to the treatment that is delivered as intended rather than some adapted version of a modality. In addition to theoretically increasing the generalizability of programming through model validation, establishing program fidelity can generate feedback to practitioners for program improvement and document program accountability in terms of whether service providers are compliant with grant conditions and treatment delivery expectations.

As detailed in the extant evaluation literature, program underperformance is considered a function of either theoretical or implementation failure (Bickman, 1987). Theoretical failure refers to whether an intervention is effective and assumes that modality delivery is as planned prior to implementation. Implementation failure entails activity that is sufficiently divergent from modality design, treatment timeframe, or delivery protocol so that programming is not representative of the modality *per se*. It is vital to distinguish between the two as implementation failure masks determination of theoretical failure. If program evaluation neglects fidelity, then observed outcomes may indeed be a function of delivered programming but not necessarily attributable to the modality. Rather, program results may

be responsive to some element of the varied rather than the intended treatment strategy, or may just be mere coincidence.

Despite the need to substantiate programming and other fidelity assessment advantages, the discipline has been reluctant to embrace process evaluation, leaving the extant knowledge base on program fidelity underdeveloped. To be fair, the history of program fidelity research in criminology and criminal justice is a fairly short one with limited efforts to introduce and mainstream fidelity and related process concerns into justice evaluation logic and designs. Although several criminologists and criminal justice scientists have addressed fidelity (Farabee, Prendergast, and Anglin, 1998; Ferguson, San Miguel, Kilburn, and Sanchez, 2007; McBride, Farrington, and Midford, 2007; Miller, 2014; Welsh, Sullivan, and Olds, 2010), most fidelity research in juvenile and criminal justice program settings has been conducted out of discipline. The contributions from Esbensen and colleagues (e.g., Esbensen, Matsuda, Taylor, and Peterson, 2011; Esbensen, Peterson, Taylor, and Osgood, 2012; Melde, Esbensen, and Tusinski, 2006) and Latessa and colleagues (see Latessa, 2004, and Lowenkamp, Latessa, and Smith, 2006) represent perhaps the most prominent demonstrations of the value and policy betterment potential of process and fidelity-related research in criminology and criminal justice. Esbensen and colleagues, through evaluation of the national G.R.E.A.T. program, introduced the discipline to fundamental fidelity concepts, whereas the research teams led by Latessa have infused definitional consistency, screening instruments (e.g., The Correctional Program Checklist and the Correctional Program Assessment Inventory), and related risk-responsive practices consistent with the National Institute of Corrections' *Principles of Effective Interventions*. As noted in Duwe and Clark's review of effective interventions, however, these instruments more often inform evaluation designs than they are directly applied.

#### *Comment on Duwe and Clark (2015)*

Randomly applied, process research is far from monolithic as evidenced by various approaches that often speak to, but fail to rigorously address, fidelity. Duwe and Clark's (2015) article is to be applauded for hypothesizing a link between program integrity and program performance, in this case, Moving On—a cognitive-behavioral treatment initiative for female offenders in Minnesota. Although categorically necessary per the methodological reasons noted previously, fidelity may be even more important in the context of gender-specific programming as many modalities simply assume treatment effectiveness for both genders.

Program evaluations too often fail to orient research questions and fieldwork around program validation, as indicated by common erroneous temporal ordering of outcome before process evaluation steps. Process evaluation *must necessarily precede* outcome evaluation per the axioms of causal inference; yet process work frequently continues to follow or coincide with the outcome phase. It is requisite first to confirm that the programming thought to affect offender behavior and, in turn, public safety is indeed as designed; otherwise, observed

program performance cannot be optimally attributed to intervention. Consequently, Duwe and Clark's (2015) article raises multiple design concerns.

The basic observation that the Moving On program featured fidelity during some periods but not others cannot be empirically substantiated and continues the long-standing pattern of neglecting qualitative criminology. The research design is incapable of informing the article's central premise of whether the studied program's "success was delivered with integrity" and is a good example of the problems associated with affording fidelity but token treatment.

Forms of program adaptation during the second of three chronologically observed periods of the Moving On program (variability in incarceration juncture at which the program was initiated, caseload, and dosage) are specified to the conclusion that nonadaptation during the other two periods is indicative of fidelity during those timeframes. It is likely correct that the adapted delivery period of the program lacked integrity or fidelity as the absence or modification of some fidelity elements can constitute a fatal fidelity threat. Similar singular adaptations jeopardizing program integrity include the mixing of treatment and general population inmates in settings intended to be a therapeutic community and the use of group counseling to satisfy individualized treatment expectations. However, the assumption that the absence of adapted delivery protocol during other periods constitutes program fidelity is suspect and signals related problems.

First, fidelity requires better conceptualization. A complex construct, fidelity comprises multiple structural and dynamic domains whose measurement requires indexing and multifaceted instrumentation. In calculation of an implementation indicator, Duwe and Clark (2015) measure integrity as a percentage of program components reflective of evidence-based practices present at the point of implementation (but not over time during program delivery) and weigh all program components equally (see Duwe and Clark, 2015, Table 1). This seems arbitrary, as it is unlikely that all components are evenly important and representative of programming content in terms of time allocation, expense, and implication for program objectives.

Next, purely quantitative designs cannot adequately substantiate fidelity. In that the absence of a fidelity threat cannot substitute for empirical confirmation of actual fidelity, it is necessary for researchers to collect process data directly—a process requiring multiple visits to the program delivery setting and the application of qualitative research methods. In that Duwe and Clark (2015) did not employ a mixed-methods design or measure fidelity directly, little confidence can be placed in their determination of program efficacy during deemed high-fidelity service delivery periods. Surely, ignored dynamic factors known to be consequential to program quality, such as counselor attitude, rapport between treatment providers and recipients, and participant engagement, are crucial to fidelity conceptualization and measurement. Fortunately, the current direction of national justice initiatives and related evaluation funding expectations have placed an unprecedented premium on fidelity-driven process evaluation within multi-method strategies.

## **Fidelity Research and Evidence-Based Culture**

Public policy shifts in recent years have generated an expanded role for crime and justice field research. Accountability has been a pronounced theme since the first Obama administration in terms of increased transparency and effectiveness in government services, including grant funding for state and local justice agencies (Mears, 2010). Consequently, U.S. Department of Justice agencies have become heavily vested in developing and promoting evidence-driven organizational cultures as indicated, in part, by federal funding reprioritization around more rigorous evaluation research ostensibly requiring both process and evaluation phases. As indicated by the requirements in recent funding opportunity announcements across U.S. Department of Justice agencies (e.g., Bureau of Justice Assistance, National Institute of Justice, and Office of Juvenile Justice and Delinquency Prevention), increased emphasis on accountability has implications for both practitioners and researchers.

For practitioners, funding is being increasingly reserved for programs that (a) use actuarial-based screening instruments consistent with the risk principle and capable of specifying targeted treatment populations (e.g., offenders with co-occurring conditions, veterans, or the homeless), (b) deliver modalities whose practices have been empirically observed as effective or at least promising, and (c) include an evaluation component to specify program performance. For researchers, evaluations must demonstrate enhanced scientific rigor in terms of validating program fidelity so as to increase confidence in observed non-spurious outcomes. These linked requirements signal heightened interdependence between researchers and system functionaries as a national movement toward an evidence-based culture is facilitating an intersection of technocratic and research objectives.

*Evidence-based practice*, the focal concept of the movement, contrasts with activities based on tradition, anecdotal evidence, politics, or practical expediency. Generally, it refers to the use of scientific research as the basis for specifying the best practices of an applied field. Originating in medicine and nursing during the 1990s and then psychology, education, and social work (DiCenso, Cullum, and Ciliska, 1998; Dobson and Craig, 1998; Gambrell, 2003; Sackett, Richardson, Rosenberg, and Haynes, 1997), evidence-based practice is steadily affecting criminal justice (Ameen and Loeffler-Cobia, 2010; Emshoff, Blakely, Gottschalk, Mayer, Davidson, and Erickson, 1997; Miller, 2012; National Institute of Corrections, 2009). To be considered “evidence based,” a program or practice must have been previously delivered, found effective by systematic evaluation, and successfully replicated. For criminal justice programs, this requires a stepwise process of first validating a program’s fidelity and then conducting experimental, randomized control trials or approximating random assignment through quasi-experimental design alternatives. Research design rigor and findings are then rated with effective programs included in national evidenced-based registries (see [crimesolutions.gov](http://crimesolutions.gov) and SAMHSA’s National Registry of Evidence-based Programs and Practices). Theoretically, only programs designated as evidenced based per the rating schemes are funded, thereby elevating agency need for research capacity

that is rarely “in house.” Because site research is expensive and almost exclusively conducted within sponsored initiatives, research–practitioner partnerships are equally critical for evaluators.

### **Conceptualizing and Capturing Fidelity**

Program fidelity comprises both the structural components of an intervention (e.g., evidence-based nature of modality elements, caseload, treatment team size, treatment provider credentials, and the frequency and timeframe of treatment sessions) and therapeutic environment dynamics reflective of the nature and quality of interaction between program stakeholders. Trial and error has resulted in loose consensus that five specific domains jointly encompass implementation intensity and modality compliance. *Adherence* refers to treatment design and delivery compliance during implementation and the life of a program that specify whether screening tools and practices are evidenced based. *Exposure* is a temporally indicated construct (e.g., frequency of counseling sessions and other services, number of sessions delivered, and session duration), whereas *delivery quality* is a function of treatment staff quality indicators such as professional credentials, attitude, and continued training. *Participant engagement* refers to the extent of demonstrated treatment participant “buy-in” to programming activities and objectives indicated by attitude regarding participation and degree of involvement. Last, *program differentiation* refers to whether the program is delivered consistently over time and cohorts in terms of maintaining approximate program size and individual counselor caseload, continuity of setting and treatment staff, and with little difference in dosage.

Collectively, these concepts inform the development of specific process and fidelity-focused research questions regarding (a) whether programming adheres to evidence-based practices that have documented success in addressing the targeted problem in the delivery setting and (b) whether delivery is consistent with prescribed program protocol. To answer these questions, researchers must use a combination of qualitative techniques to capture all of the dimensions of fidelity across successive implementation and delivery phases. Designs should combine document analysis (to confirm that training materials and delivery protocol are evidence based), in-depth interviews (with program administrators and treatment providers), focus group interviews (with offenders), and direct observation of treatment activities to determine fidelity, holistically.

Together, these methods enable the collection of data informing fidelity ratings as shown in the *Justice Program Fidelity Scale*—an instrument assuming multiple site visits and inter-rater reliability that we have employed in various sponsored juvenile and criminal justice program evaluations (see the Appendix). This scale features theoretical–methodological symmetry through multiple indicators for the measurement of five fidelity conceptual domains (adherence, exposure, participant engagement, delivery quality, and program differentiation). In addition to yielding domain-specific and overall program fidelity scores for individual programs, the scale can generate a fidelity variable for national evaluations

of the same modality and delivered in multiple settings. Although theory suggests that the identified domains are vital to capturing fidelity, their indicators are reconfigurable to various evaluation topics and purposes.

### **Recommendations**

To move forward, various barriers embedded within justice funding agencies and academe must be addressed to deliver optimal program evaluations specifying evidence-based programming and its replication across offender populations and jurisdictions. The traditional distance between researchers and practitioners has seemingly been closed somewhat through funding incentives. The funding agencies, however, could better practice what they preach as the prioritization of fidelity-driven process research, although significantly increased in recent years, is still not evidenced in most of the current awards and is far from a categorical requirement. Similarly, various rating schemes and registries to measure whether research is evidence based, although descriptively upholding the necessity of implementation and modality adherence confirmation, regularly feature interventions and treatments with unverified fidelity as promising and effective.

The primary barrier to moving fidelity research forward, however, invites academe to take an inward gaze. We have not been effective in regard to either explicating the methodological essence of establishing fidelity and its direct implications for outcome evaluation or establishing model fidelity designs with attendant instruments. Although these challenges can be addressed through advocacy and design rigor, the limited extent of process evaluation is almost certainly due to the near total lack of advanced instruction in applied fieldwork throughout the discipline (Copes and Miller, 2015). Relatively few qualitative criminologists exist and even fewer who conduct applied fieldwork, which is why much of the evaluation of juvenile and criminal justice programming is performed by other disciplines.

Qualitative research addressing implementation intensity and treatment services delivery across multiple program fidelity domains is necessary to attribute observed statistical outcomes (most often recidivism and relapse) to treatment elements rather than to modality variance or coincidence (Miller, Tillyer, and Miller, 2012). Process evaluation within mixed-methods designs documenting program validation should be considered a prerequisite to outcome analysis and marks a significant advancement opportunity for applied qualitative criminology. Quantitative evaluations that employ less-than-optimal comparison group or analytic strategies have been indicted as counterproductive to criminal justice policy and practice and even unethical (McCord, 2003; Sherman, 2009)—a logic equally applicable to the rigor of process design. In that causal inference is theoretically and methodologically dependent on the specification of fidelity, it is vital that the discipline better balance qualitative research methods if we hope to impact policy to the fullest extent possible.

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**Appendix: Fidelity Scale**

**JUSTICE PROGRAM FIDELITY SCALE\***

Site: \_\_\_\_\_ Rater 1 initials: \_\_\_\_\_ Rater 2 initials: \_\_\_\_\_ Date: \_\_\_\_\_

	Rater 1	Rater 2	Consensus	Actual Value
<b>Adherence (0/1)</b>				
Intake screening				
Intake timeliness				
Treatment plan components				
Caseload compliance				
Individualized service plans				
Dosage				
<b>Adherence Total:</b>				
<b>Exposure (0/1)</b>				
Contact frequency (hours per day)				
Duration; Program Length				
<b>Exposure Total:</b>				
<b>Delivery Quality (coded 1-5)<sup>1</sup></b>				
Staff qualifications				
Counselor/staff attitude				
Counselor/staff continued training				
<b>Delivery Quality Total:</b>				
<b>Participant Engagement (coded 1-5)<sup>2</sup></b>				
Participant attitude				
Participant involvement				
Participation barriers (reverse code)				
<b>Participant Engagement Total:</b>				
<b>Program Differentiation (reverse coded 1-5)<sup>3</sup></b>				
Program size fluctuation				
Program budget fluctuation				
Caseload fluctuation				
Continuity of staffing (coded 1-5)				
Continuity of setting (coded 1-5)				
<b>Program Differentiation Total:</b>				
<b>TOTAL FIDELITY SCORE</b>				

\* An earlier version of this scale was conceptualized through support from Grant No. 2010-RT-BX-0103 awarded by the National Institute of Justice, Office of Justice Programs, U.S. Department of Justice with assistance from Co-Principal Investigator, Dr. Rob Tillyer.

<sup>1</sup> Higher scores indicative of greater delivery quality.

<sup>2</sup> Higher scores indicative of greater participant engagement.

<sup>3</sup> Higher scores indicative of lower program differentiation.

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### Changing the Knowledge Base and Public Perception of Long-Term Prisoners

**Marc Mauer**

*The Sentencing Project*

Several decades ago, I spent a good deal of time with a self-help group of men serving life sentences in a Michigan prison. Many of their personal stories of transformation were quite compelling. One of the men as a 20-year-old had participated in an armed robbery. By the time I got to know him, he had become a 40-year-old who was saving some of his earnings from work in the prison kitchen to make a modest monthly donation to Save the Children, an international organization focused on giving children a healthy life with the opportunity to learn and be protected from violence. In his mind, that donation was a small way to pay back the larger community for the harm he had done, and one that stood in sharp contrast to the lack of such options in the prison environment.

In their argument for “the imperative” of including lifers and long-term prisoners in both research and policy discussions, Kazemian and Travis (2015, this issue) point to the disturbing situation whereby this population has been largely ignored in discussions about mass incarceration. Government data have long been lacking on the size and demographics of this population, and only in recent years have we begun to receive detailed analyses compiled by nonprofit organizations (in particular, The Sentencing Project and the American Civil Liberties Union).

The overall figures are striking. One of every nine people in prison today is serving a life sentence. Nearly one third of this group are serving terms of life without the possibility of parole, and of those who are parole eligible, politically driven decisions have frequently resulted in excessively lengthy periods in prison before release. The implications of these figures both for addressing mass incarceration and for human rights concerns are profound.

Kazemian and Travis (2015) first document how little we know about the experience of lifers in prison, including the physical or psychological impact of imprisonment over a period of many decades behind bars, and to what extent prison serves either a rehabilitative

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or a criminogenic function in these cases. We do know that the lifers who are released from prison have substantially lower rates of recidivism than the general prison population, but is this a result of “aging out” of crime or some aspects of prison life? And in either case, what does this tell us about the minimum length of time in prison that is necessary to achieve such outcomes?

Kazemian and Travis (2015) also suggest that we need to explore the degree to which the collateral effects of incarceration affect long-term prisoners. In recent years, a growing body of scholarship has explored the collateral consequences of conviction and incarceration, but we know very little about the ways in which these effects are similar or exacerbated for long-termers. The most significant issues in this area concern family relationships; for example, how, or under what circumstances, do family ties endure through decades of incarceration? How do children adapt to family environments in which they may have only limited contact with a parent for decades at a time? Does long-term incarceration contribute to the destabilization of high-incarceration communities in unique ways?

Kazemian and Travis (2015) hold out hope that attention to lifer needs in prison, along with appropriate levels of programming, can aid lifers in playing a constructive role in the prison environment, through their maturity and taking on leadership roles. Importantly, they argue that we as a society should support such interventions on normative grounds. That is, although we can hope that such programming will contribute to greater desistance from crime, such initiatives are what a compassionate society should support in any case.

In a response to their argument, Henry (2015, this issue) notes in her policy essay just how extreme the U.S. life imprisonment situation is in comparative terms. She demonstrates that “many developed nations throughout the world have determined that life imprisonment in *any* form is not a legitimate punishment,” and those that retain it generally require a sentence review after a mandated term of years. Henry also suggests that enhanced programming for long-term prisoners can become an essential component of a strategy for sentencing reform and reversing the impacts of mass incarceration. Sufficient evidence that individuals are released from prison better prepared to lead constructive lives may influence the public debate on prison terms.

Fleury-Steiner (2015, this issue) in his policy essay calls particular attention to the lack of consideration for the substantial physical and psychological health-care needs of many long-term prisoners. He documents the incidence of chronic illness behind bars and the often inferior level of health care in prison. These problems are exacerbated as individuals age in prison or are placed into solitary confinement for long periods, and he argues that there is a history of substandard care provided by for-profit health-care providers. One reason to be cautiously optimistic is the potential impact of the Affordable Care Act of 2010 (ACA), both for persons at risk of incarceration and for those returning home from prison. To the extent that the ACA can fill these critical gaps in health care, one can hope to see reduced engagement in criminal behavior and a smoother transition to the community for those leaving prison.

Although all of these contributions to the discussion of life sentences are noteworthy, it is clear that Kazemian and Travis (2015) also recognize the need to address the scale of punishment in the United States. As is true of most aspects of American criminal justice, our life imprisonment policies are extreme by the standards of comparable nations. Improvements in the life conditions of those sentenced to long-term confinement are critically needed, but at the same time, we need to explore ways to shift the political environment in which these punishments have come to be deemed as acceptable social policy.

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# Imperative for Inclusion of Long Termers and Lifers in Research and Policy

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### **Research Summary**

*Although numerous studies have highlighted the negative consequences of mass incarceration, life-course and criminal career research has largely failed to document psychological, social, and behavioral changes that occur during periods of incarceration. This oversight is particularly noteworthy in the case of individuals serving long sentences, as they spend a significant portion of the life course behind bars. The policies and programs targeting prisoners are seldom tailored to long termers and lifers, and we know little about effective interventions, or even how to measure effectiveness, for this population. By drawing on the relevant empirical research, this article underlines the importance of reorienting some research efforts and policy priorities toward individuals serving life or otherwise long prison sentences.*

### **Policy Implications**

*During the last 20 years, the prevalence of life sentences has increased substantially in the United States. We argue that there are various benefits to developing policies that consider the challenges and issues affecting long termers and lifers. In addition to the ethical and human rights concerns associated with the treatment of this population, there are several pragmatic justifications for this argument. Long termers and lifers spend a substantial number of years in prison, but most are eventually released. These individuals can play a key role in shaping the prison community and potentially could contribute to the development of a healthier prison climate. Investment in the well-being of individuals serving long sentences may also have diffused benefits that can*

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*extend to their families and communities. It would be advantageous for correctional authorities and policy makers to consider the potentially pivotal role of long termers and lifers in efforts to mitigate the negative consequences of incarceration.*

### **Keywords**

*life sentences, lifers, long termers, prisoners*

Incarceration rates have generally been on the rise in most developed countries during the past few decades (International Centre for Prison Studies, World Prison Brief Online, 2015),<sup>1</sup> but this trend has been particularly pronounced in the United States (National Research Council, 2014). The United States is the world leader in incarceration with approximately 2.2 million people incarcerated in the nation's state and federal prisons and jails; these figures reflect a nearly 500% increase in the incarceration rate during the past three decades (National Research Council, 2014). As a result of tough-on-crime policies (i.e., Three-Strikes legislation, "truth-in-sentencing" policies, and a reduced or delayed recourse to parole), the length of imposed sentences and the average time served by prisoners in the United States have increased substantially since the mid-1970s.<sup>2</sup> Between 1990 and 2009, the average time served increased by 37% for violent offenses, 36% for drug offenses, and 24% for property offenses (Pew Center on the States, 2012). As a result of these longer sentences, it is not surprising that the United States is faced with an increasingly aging prisoner population (Human Rights Watch, 2012) with distinctive needs and high economic, social, ethical, and health costs (Osborne Association, 2014).

The United States also sets itself apart from other countries with its excessive use of life sentences (and particularly life without the possibility of parole [LWOP]), which are generally employed sparingly in other parts of the world (Nellis, 2013) or even deemed unconstitutional in some nations (such as France, Germany, and Italy; Nellis, 2010). In 1987, on the basis of a set of guidelines developed by the U.S. Sentencing Commission, Congress eliminated the possibility of parole for individuals serving life sentences at the federal level. The prevalence of life sentences has been on the rise in the United States during the last few decades (Nellis and King, 2009). Nellis (2013) reported that in 1984, 34,000 individuals, or approximately 1 in every 13 prisoners, were serving life sentences; this figure increased to 159,520 prisoners in 2012 (or one in every nine prisoners), illustrating

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1. Correctional statistics are accessible by selecting the continent and country of interest at the left of the map.
  2. This issue is complex because sentence length can be measured in different ways (e.g., pronounced sentence versus time served). The evidence presented in this section suggests that the number of years that individuals spend in prison has increased in the United States. However, this information only can be documented accurately after release, and few follow-up studies have focused on individuals serving long sentences.

a more than fourfold increase in the number of individuals serving life sentences between 1984 and 2012.<sup>3</sup> Consistent with general correctional trends, blacks are over-represented among those sentenced to life in prison, making up as much as three-quarters of lifers in some states (Maryland: 77.4%, Georgia: 72%, Mississippi: 71.5%; Nellis, 2013). The population of lifers has grown despite declining crime rates during the last two decades, as well as shrinking prison populations in some states. For instance, in New York, although the prison population decreased by 19.6% between 2000 and 2010, the number of individuals serving LWOP sentences increased by 249% (Nellis, 2013). These figures draw attention to the growing population of prisoners serving multiyear, multidecade, or life sentences, and they highlight the importance of reexamining how long-term imprisonment impacts the study of criminal careers and life-course patterns, policy responses, effective programming, and preparation for release.

The United States is one of the few countries that imposes life sentences on juvenile offenders. In fact, no other country is known to have applied these sentences in recent years ([amnestyusa.org/our-work/issues/children-s-rights/juvenile-life-without-parole](http://amnestyusa.org/our-work/issues/children-s-rights/juvenile-life-without-parole)). Nellis (2013) offered the most recent U.S. figures on juvenile life sentences. Nearly 7,900 individuals are serving a life sentence (with the possibility of parole) for crimes committed before 18 years of age, and approximately 2,500 juveniles are serving LWOP sentences. The number of juvenile cases transferred to the adult system nearly doubled between 1985 and 1994, leading to an increase in the number of minors sentenced to life in prison. LWOP sentences grew increasingly prevalent among the population of juveniles convicted of murder between 1980 and 2000 (1980: 0.14% of juvenile murderers were sentenced to LWOP, 1990: 2.86%, 2000: 9.05%; Human Rights Watch, 2005: 32). Figures on the prevalence of juvenile life sentences are not collected on a regular basis, and little is known about more recent trends (Nellis and King, 2009).<sup>4</sup>

In this article, we wish to draw attention to the fact that researchers and policy makers have largely ignored the issue of long termers and lifers. We argue that there are various benefits to understanding more clearly and addressing the distinctive needs of this population. These benefits are not necessarily centered on the reduction of recidivism, although they may indirectly result in reductions in reoffending in the short and long

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3. It is important to highlight that these figures are likely to underestimate the number of individuals who will spend most of their lives in prison because these analyses typically exclude sentences that would "equate to one's life (e.g., a sentence of 90 years, after which one might be eligible for parole)" (Nellis and King, 2009: 2). Mauer, King, and Young (2004) regarded these individuals as "virtual lifers."

4. Recent Supreme Court decisions stipulated that sentencing juveniles to life in prison without the possibility of parole constitutes cruel and unusual punishment and violates the 8th Amendment (*Graham v. Florida*, 2010; *Miller v. Alabama*, 2012). We do not yet fully grasp the impact of these decisions, and trends will need to be monitored in upcoming years. A recent briefing paper published by The Sentencing Project (Rovner, 2014) suggested that although the Supreme Court ruling struck down laws in 28 states, most states are yet to implement any statutory reform or have replaced the juvenile LWOP sentences with multiple-decade sentences.

term. The treatment of prisoners more generally is a question that raises key ethical issues, and these concerns are particularly relevant to long termers and lifers. As argued by Tonry (2011), this is primarily an issue of social justice and human rights, and we are highly sympathetic to these concerns. However, we do not believe that the normative argument is necessarily incompatible with pragmatic considerations. Our call to pay more attention to long termers and lifers also entails several practical ramifications. Long termers and lifers spend a significant portion of their lives in prison. Notwithstanding the assumption that individuals serving life sentences will never leave prison, most are eventually released.<sup>5</sup> These individuals can make important contributions to the prison community and may potentially help to develop a healthier prison climate. The well-being of individuals serving long sentences is likely to have diffused benefits that extend to their families and communities. We conclude by discussing some promising directions for policy and research involving this population.

### **Long Termers and Lifers: A Neglected Population**

Important developments have been made in life-course and criminal career research in recent years. Researchers have moved beyond static measures of criminal career parameters, and more thorough and sophisticated statistical methods have been developed to address some of the challenges in capturing changes in life-course patterns. One of these advancements includes the recognition of patterns of intermittency in criminal careers and the importance of adjusting for “time at risk” in criminal career estimates (otherwise known as “exposure time” or “street time,” i.e., periods during which individuals are free to engage in criminal behaviors; see Piquero, 2004). Piquero et al. (2001) found that the failure to account for exposure time may lead to the false conclusion that some individuals have ceased offending. Piquero (2004: 119) cautioned researchers “not to confuse incarceration stints for intermittency.” Periods of incarceration often are regarded as inconvenient events in analyses of life-course and criminal career patterns. Statistical models adjust for time at risk. These adjustments are based on the premise that individuals are inactive in offending while incarcerated.

As a result of these assumptions, life-course and criminal career research has failed to examine and document changes that occur during periods of incarceration. This neglected dimension of the life course is particularly noteworthy for individuals serving long sentences as they spend a substantial number of years behind bars. Prison is one of many life events that may occur during the life course. For some individuals, this event takes up a substantial portion of their lives; they may frequently transition in and out of prison or can spend extended periods of time incarcerated. Significant changes may occur in their lives, and in their development as human beings, during these periods. Although some studies have

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5. It is difficult to determine the percentage of lifers who are released. Drawing on stock and flow analyses, Maurer et al. (2004) estimated an average time served of 29 years among lifers admitted to prison in 1997.

investigated prison behavioral misconduct (Cunningham and Sorensen, 2006; Flanagan, 1980; Siennick, Mears, and Bales, 2013; Sorensen, Wrinkle, and Gutierrez, 1998; Toch, 2008; Zamble, 1992), virtually no research has investigated these behaviors within a life-course or criminal career framework.<sup>6</sup>

Criminal justice policy makers also have largely overlooked the distinctive profiles of lifers and long termers. Interventions and prison programming have not traditionally been designed to address the needs of these individuals. Flanagan (1995a [1992]: 5) argued that “for most of the history of institutional corrections, correctional policy makers put long-term prisoners at the bottom of the list of priorities.” Flanagan offered several explanations for this lack of interest in long termers. First, because of the serious offenses that have led to their long sentences, these individuals often are regarded as less than ideal candidates for intervention programs. The public is not particularly optimistic about the potential for change among these prisoners. Second, because correctional resources often are scarce, which has been increasingly true during the current era of budget cuts and limited services, priority tends to be granted to those individuals who are approaching release. As a result, services provided to long termers and lifers are not prioritized (Gottschalk, 2014).

Nellis (2013: 20) explained the importance of considering lifers in reentry efforts:

The emergence of reentry as a criminal justice policy issue in the last decade has largely ignored persons serving a life sentence. Typically, reentry programs are provided to persons within six months of their release date and offer transition services in the community upon release. However, for persons serving a life sentence, their release date is not fixed and they are often overlooked as policymakers and correctional administrators consider reentry strategies. Additionally, persons serving a life sentence have unique reentry needs based upon the long duration of their prison term. The failure to design reentry strategies for persons serving a life sentence neglects one in nine persons in prison by denying them the opportunity to participate in valuable programming.

In his interviews with 59 long termers (i.e., incarcerated for at least 5 years), Flanagan (1979: 235) reported that the men felt unanimously neglected by the Department of Corrections, that they were denied access to programs, and that “the entire life cycle of correctional service programs and procedures revolves around the short-term prisoner. As a result, the long termer is left without any meaningful mechanisms to achieve progress.” The long termers in Flanagan’s (1979: 235) study regarded themselves as “forgotten men” within the correctional system.

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6. Two exceptions are noteworthy. Toch (2008) conducted a study on the prison careers of disruptive inmates in Scottish prisons and offered a case history analysis of officially recorded incidents over several years. Siennick et al. (2013) investigated the association between prison visits and disciplinary infractions, as well as the changes in these incidents across an 18-month period.

Although it is reasonable to presume that some of the needs and issues affecting long-term prisoners are similar to those encountered by all individuals exposed to the experience of incarceration, it is likely that “the element of *time* exacerbates all of the deprivations in the case of long-term prisoners and transforms them from noxious characteristics of imprisonment that can be accepted over the short-term into major problems of survival over the duration of a long prison sentence” (Flanagan, 1981: 212). Some stressors that impact prisoners more generally may be amplified for lifers and long termers. These prisoners are particularly affected by the threat of a permanent loss of relationships with family or friends, the challenges in establishing friendship networks in prison because of the high turnover rates resulting from transfers and releases, their unknown release date, and prolonged exposure to harmful dimensions of prison life (Flanagan, 1995b [1991]). Despite these potential challenges, we argue that long termers and lifers may constitute a valuable resource in the prison environment and could play a potentially key role in the improvement of prison life.

### **What Constitutes a “Long Termer”?**

Various authors have offered different definitions of long-term incarceration, and these definitions have shifted over time. Cowles and Sabbath (1996) discussed the difficulties in defining and operationalizing the concept of long termers. They argued that these challenges are enhanced by the use of indeterminate sentences with discretionary release decision making, the lack of consensus on the variable to be measured (“total sentence length, the time actually served by the offender, or the time remaining to be served”: 44), and the disagreement regarding the specific number of years required to constitute long-term incarceration. Cowles and Sabbath (1996) also showed the disparity in definitions of long timers in prior research, ranging from 5 years of continuous time served to a life sentence. In the 1990s, Flanagan (1995a [1992]: 4) noted:

Nearly 15 years ago, I felt confident in adopting a criterion of five years of continuous confinement to define long-term imprisonment. Five years was more than twice the average time served in state prisons in the U.S., and only 12% of the state prisoner population in 1974 had actually served five years or more. . . . Ten years later, other investigators defined long-term incarceration as seven years. . . . Given that the average prison sentence for violent felonies handed out in American state courts in 1988 ranged from 90 to 238 months, one could argue that, today, an expected time served of at least eight to 10 years would qualify a U.S. prisoner as a long-term inmate.<sup>7</sup>

The minimum number of years set forth in definitions of long-term incarceration has gradually increased over time (MacKenzie and Goodstein, 1985: 6 years; Cunningham

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7. It is important to mention that Flanagan’s publications rely on data that were collected in the 1970s, a period during which the prison population was only beginning to build up.

and Sorensen, 2006: 10 years; Crayton, 2012: 15 years), and this figure tends to be lower in European research when compared with American studies (e.g., Dudeck et al., 2011: 5 years). This trend is reflective of the significant shifts that have occurred in American correctional policy during the last four decades and suggests a higher threshold for what is regarded as excessive punishment.

Three other issues arise in the definition of a long termer. First, prisoners' definitions of long-term incarceration may differ from definitions developed by researchers (Flanagan, 1979). The effects of incarceration are likely to be subjective, to be dependent on a host of factors, and to vary from one individual to another. Similar sentences can have disparate effects on different individuals, and the threshold after which prison becomes increasingly harmful (or increasingly routine) may vary across different individuals. Second, even if we can reach a consensus on the number of years that qualify for a long sentence, several scholars have suggested that it may be erroneous to regard lifers and long termers as a uniform group; individuals within this group may be characterized by diverse backgrounds, needs, levels of risk, and coping abilities (Flanagan, 1982; MacKenzie and Goodstein, 1985). Third, the actual outcome of pronounced sentences is uncertain. Some individuals sentenced to LWOP may receive a commutation and be released. In contrast, individuals sentenced to life with the possibility of parole, or not sentenced to life imprisonment at all, may spend their lives in prison. These latter two scenarios have become increasingly likely in recent years given that the number of state prisoners who die in prison has increased by 17% between 2001 and 2011 (from 2,869 deaths to 3,353 deaths); these figures reflect a 5% increase in the mortality rate per 100,000 state prisoners during this period (from 242 to 254; Noonan and Ginder, 2013).

From a practical viewpoint, it is easy to understand why it would be desirable to establish a specific threshold to define long-term incarceration. However, these efforts miss a larger point: Prison life needs to be examined within the life-course framework. Often, we presume that lives are halted when individuals enter prison, but this may be a flawed assumption; life-course transitions may occur, psychological well-being may fluctuate, criminal careers may persist, and the desistance process may unfold during periods of confinement.

To summarize, we know that there has been a significant rise in the number of individuals incarcerated in the United States, as well as a substantial increase in the average sentence length imposed on these individuals. The sentencing framework typically presumes that many individuals will spend their lives in prison. Resources tend to be allocated to individuals who are approaching release, which has led to the neglect of a rapidly growing population of prisoners. In the following sections, we argue that increased investment in the needs of long termers and lifers may entail significant short- and long-term benefits for life inside the correctional facility, for progress toward desistance, as well as diffused benefits to community members affected by the experience of incarceration. We also underline the importance of investigating changes in life-course and criminal career patterns during

periods of incarceration, as well as the need to understand more clearly how individuals can make progress toward desistance while incarcerated.

### **Why Should We Pay More Attention to Long Termers and Lifers?**

The most comprehensive studies conducted with long termers and lifers were carried out several decades ago. We wish to stress that most of the research presented in the next sections may be based on data that are now dated and that the issues that impact contemporary prisoners (and particularly long-term prisoners) may be drastically different from what they were in the 1970s and 1980s. Overcrowding is more common, mental health issues are more prevalent, and the prisoner population is more racially and ethnically diverse (National Research Council, 2014). Individuals serving long sentences have not been the focus of research for a long time, and this shortcoming again highlights the need to reorient some of our research efforts toward this population.

#### *Sentences of Long Termers and Lifers Represent Many Life-Years in Prison*

For long termers and lifers, a prison sentence does not constitute a short absence from ordinary life in the community. First and foremost, these prisoners must be regarded as individuals who will spend a considerable portion of their lives in prison (Flanagan, 1982; Toch, 1977). By drawing on prison data from the Bureau of Justice Statistics, Mauer et al. (2004) estimated that of those lifers who are released, return to the community occurs after approximately three decades of incarceration. It may not be appropriate to expose long termers and lifers to the same programs and services intended for individuals serving short sentences (e.g., programs of short duration that target specific skills; see Flanagan, 1982). Flanagan (1982) argued that a more productive approach is to set out long-term goals for long termers and lifers. He discussed the importance of using prison time in a strategic way:

[I]t is incumbent on the correctional system to work with the offender to plan a worthwhile career, one that will be beneficial to both the offender and others, and that will be transferrable and capable of supporting the offender upon his eventual release. Moreover, there is no reason why, during their long imprisonment, many long-term inmates cannot make a substantial contribution to society through help provided to fellow inmates. (p. 89)

The prison career approach is not a novel idea. It was raised by Hans Toch several decades ago (see Toch, 1977, 1995), although the evidence to suggest that this approach is frequently applied in our current system is limited. This paradigm encourages the prisoner to “pursue a meaningful life in prison” (Flanagan, 1995b [1991]: 114). Flanagan (1982) described the situation of prisoners who complete education or training programs in prison and who move on to becoming instructors to other prisoners. This transition from student to teacher is a prime example of a beneficial use of prison time, and it has the added advantage of eventually enabling prisoners to provide many of the services offered in the



facility (Toch, 1977). It would be strategic for long termers and lifers to make productive use of their time in prison by participating in various forms of education, training, and service. The opportunity to exercise a meaningful role can be highly rewarding for the prisoner. Also, it is likely to benefit fellow prisoners and staff as it promotes collaborative work with staff members (Toch, 1977). The feeling that one has made some form of contribution to the prison community also may be beneficial in preparing individuals for their eventual release. The benefits of involving long termers and lifers in the routine activities of the prison will be discussed in more detail in the following sections.

### *Many Long Termers and Lifers Do Not Pose a Distinctive Threat to Public or Prison Safety*

The type of risk posed by prisoners can be classified in two categories: (a) threats to the safety of the prison environment (correctional risk) and (b) risk posed to the outside community (community risk). Individuals serving long sentences do not necessarily pose a significant threat to either. Although correctional administrators pay more attention to correctional risk, politicians are more concerned with the community risk posed by former prisoners, particularly those convicted of violent offenses. The paucity of longitudinal studies documenting prison and community behaviors across several periods of the life course is noteworthy, and it has limited our understanding of risks posed by long termers and lifers.

Concerns with community risks posed by lifers in particular are illustrated in parole board decisions. A study conducted by Stanford University researchers found that as of 2010, California lifers had less than a one-in-five chance (18%) of being approved for release by the parole board (Weisberg, Mukamal, and Segall, 2011). They also reported that this figure was less than 20% for most of the period between 1980 and 2010. In addition, the enactment of Marsy's Law (California Victims' Bill of Rights Act of 2008) resulted in greater delays for subsequent hearings when individuals were denied parole (Weisberg et al., 2011).<sup>8</sup>

To what extent do recipients of long sentences pose a threat to the outside community? Some research has suggested that lifers are not necessarily characterized by at-risk profiles and extensive histories of violence. Mauer et al. (2004) argued that California's Three-Strikes law resulted in life sentences for many individuals convicted of a nonviolent crime as the third strike (57.5%,  $n = 4,225$ , of all Three-Strikes cases). Although most lifers were convicted of a violent crime, 39% (at the federal level) and 4% (at the state level) were convicted of a drug offense. In addition, being convicted of a violent crime may not necessarily be indicative of a high risk of sustained violence (Gottschalk, 2014). Mauer et al. (2004: 13) provided several examples of scenarios involving lifers who were convicted

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8. The Victims' Bill of Rights Act of 2008, otherwise known as Marsy's Law, is a legal measure that amended the California Constitution to expand the legal rights of victims. It also provided parole boards with additional powers to deny parole to prisoners.

of violent offenses but for whom the life sentence seemed “overly harsh and inappropriate” because of either doubts about their culpability or doubts about the limited risk of future violence posed by these individuals. These include battered women, mentally ill offenders, minors, and individuals sentenced under “accountability” policies (e.g., legal provisions that allow life sentences for participants who play a secondary role in a crime and who may not be aware of the primary offender’s intent to use lethal violence).

Data and research examining the recidivism rates of released lifers and long termers have been limited, and existing studies on this topic tend to be dated, rely on small samples, use short follow-up periods, or present other methodological shortcomings. Mauer et al. (2004) found that the rearrest rate of released lifers was lower than that of other releasees; Mauer et al.’s results suggested that only one in five lifers (20.6%) released in 1994 was rearrested, compared with a rearrest rate of 67.5% for all individuals released from prison. They added that “lifers—90% of whom are incarcerated for a violent offense—are no more likely to be rearrested for a violent offense (18%) than property (21.9%) or drug offenders (18.4%)” (Mauer et al., 2004: 24). By drawing on release data from New York State between 1985 and 2008 and using a 3-year follow-up period, Kim (2012) reported the return-to-custody rate of individuals originally convicted of murder was much lower than that of other releasees (17.4% vs. 41.2%). In addition, most returns to custody among individuals originally convicted of murder occurred as a result of technical violations (86.2%; see also Crayton, 2012). In a Dutch study, Snodgrass, Blokland, Haviland, Nieuwebeerta, and Nagin (2011) found that when including relevant control variables, reoffending rates did not differ between matched groups having served short and long sentences. When excluding controls, individuals who served longer sentences were less likely to reoffend (Snodgrass et al., 2011). In a 5-year follow-up of 294 Canadian lifers and long termers (i.e., serving determinate and indeterminate sentences of 10 or more years), Weekes (1995) found that 58% of the released prisoners were not rearrested or reconvicted, approximately 20% were readmitted for a technical violation, and approximately 22% were readmitted for a new offense. Among the 75 releasees who had been incarcerated for murder, 14.6% ( $n = 11$ ) were reconvicted of a crime, but none of these convictions entailed murder or manslaughter. Similarly, Weisberg et al. (2011) found the recidivism risk (measured by the recommitment rate to state prisons) of recently released California lifers to be minimal (1% recidivism rate for lifers vs. 48.7% for the overall prisoner population).

Crayton (2012: 80) raised the important question of “whether these lower rates of return are achieved at an earlier point—or points—during a person’s long sentence.” By drawing on release data from New York State between 2000 and 2004, Crayton (2012) found that the rearrest rates of individuals convicted of violent and nonviolent offenses became comparable after 10 years of prison time. She also failed to find significant differences in the survival rates (i.e., time to recidivism) between long termers (sentences of 15 or more years) and other prisoners, suggesting that time served was a poor predictor of recidivism. Crayton’s (2012) analyses did, however, suggest that long termers were more likely to

return to prison as a result of parole violations. Overall, the findings from these studies suggest that individuals serving long sentences do not seem to pose a distinctive threat to the community when compared with other former prisoners.<sup>9</sup> However, methodological shortcomings (e.g., selection effects and a lack of comprehensive control variables such as age and criminal history) have made it difficult to draw definitive conclusions about the recidivism risk of released long termers and lifers (National Research Council, 2014). Furthermore, our understanding of the reasons explaining the lower recidivism rates of individuals serving long sentences remains limited. Is it a result of aging and increased rationality (Shover, 1996), improved social bonds (Laub and Sampson, 2003; Sampson and Laub, 1993), shifts in self-identity (Maruna, 2001), or other cognitive changes such as the openness to change (Giordano, Cernkovich, and Rudolph, 2002)? Life-course research, namely long-term follow-ups of individuals serving long sentences, can provide some insight into some of these questions.

The main problem remains that it may not be possible to create a matched sample that would yield a suitable comparison with released lifers. We know that age is a particularly crucial control variable in these comparisons. Released lifers tend to be older at the time of release because of their long sentences. A comparison group matched on age would inevitably include one of two types of individuals: (a) individuals with convictions later in adulthood, indicating late onset offending or persistent criminality beyond the point where most offenders desist from crime, or (b) “virtual lifers,” that is, long termers who have served sentences that are of comparable length to that of lifers. In the first scenario, matched individuals may present profiles that are starkly different from lifers; in the second, it may be inappropriate to regard those long termers who spend a similar number of years in prison as lifers as a distinct group. Thus, we ask: Are the efforts to compare the recidivism rates of released lifers with those of a matched sample futile?

In addition to the potential threat posed by lifers and long termers to public safety, some research has explored the correctional risk exhibited by this group. Although few empirical studies have contrasted the profiles of long- versus short-term prisoners, the limited studies that do exist have found that the former group does not seem to be characterized by more at-risk profiles. For instance, Weisberg et al. (2011) reported that 75% of lifers were classified as low risk by the California Static Risk Assessment instrument, a figure that is starkly different from that of the general prisoner population (28% were classified as low risk).

When focusing specifically on prison misconduct, Flanagan (1979: 131) found that “the median annual infraction rate of the short-term prisoner group is nearly double that of the long-term inmate group”; this trend persisted when controlling for the length

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9. Similar findings emerge for juveniles. Little evidence suggests that juvenile lifers are “super predators” (Human Rights Watch, 2005); 59% of all juveniles sentenced to LWOP were first-time offenders, and 26% were involved in incidents in which the individual had minimal involvement (e.g., he or she lacked the knowledge or intent to engage in murder).

of incarceration period. Subsequent analyses showed that infractions committed by long termers were of a more serious nature, although this was found to be a weak association. Later research conducted by Toch and Adams (1989) suggested that prison misconduct was more prevalent among the younger long termers. One study contrasting the prevalence of prison misconduct between lifers with and without the possibility of parole did not find any significant differences between the two groups (Sorensen and Wrinkle, 1996).

Cunningham and Sorensen (2006) explained the rationale behind the increased security measures targeted at LWOP prisoners, namely, that these individuals are expected to engage in prison misconduct because they have “nothing to lose.” By drawing on a sample of prisoners in Florida, the authors compared the disciplinary behaviors of nearly 2,000 LWOP prisoners with those of approximately 7,000 prisoners serving long sentences (minimum of 10 years). Overall, the prevalence of aggravated assault was low among LWOP inmates (0.6%). The results also showed that individuals sentenced to less than 20 years were more likely to be involved in prison misconduct and violence, whereas those sentenced to more than 20 years were less susceptible to violent behaviors; LWOP prisoners found themselves somewhere between these two extremes, even when controlling for other potential risk factors for prison misconduct. These findings prompted Cunningham and Sorensen (2006: 701) to assert that “there is no basis for concluding that LWOP inmates are “superpredatory” or would constitute a proportionally greater hazard to correctional staff than other long-term inmates.”<sup>10</sup>

In short, although limited in scope, the available research has suggested that long termers and lifers do not pose a greater threat to the community or to the prison environment when compared with other prisoners. As such, the argument that these individuals would not be amenable to interventions is not substantiated by existing empirical evidence.

### *Limited State of Knowledge on the Developmental and Life-Course Changes that Occur Throughout a Prison Sentence*

It is remarkable to observe the important gaps in our knowledge regarding the impact of long-term incarceration on prisoners. In our view, contemporary life-course studies are needed to shed some light on this issue. Incarceration tends to be regarded as a homogenous experience, but conditions of confinement vary greatly across facilities, states, and countries. These divergences render the assessment of the impact of incarceration on prisoners particularly challenging. The National Research Council (2014: 200) concluded that “some poorly run and especially harsh prisons can cause great harm and put prisoners at significant risk.”

The potential harmful effects of imprisonment have been discussed by many researchers. According to Sykes (1958: 286–292), the “pains of imprisonment” include the deprivations of liberty, goods and services, heterosexual relationships, autonomy, and security. The

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10. The lower rate of violence among LWOP prisoners may result from the tendency of other prisoners to avoid conflict with these individuals because of the perception that they have “nothing to lose.”

constant feeling of loneliness can cause significant distress among prisoners (Johnson and McGunigall-Smith, 2008). Liebling (2011: 536) describes a “new and distinctive kind of ‘prison pain’ . . . consisting of a kind of existential and identity crisis brought on by both the length and uncertainty of contemporary sentences, but also by the restricted facilities available.” Most empirical studies and meta-analyses that have investigated the impact of incarceration (and length of incarceration) on recidivism have found that imprisonment has either no impact or undesirable effects on subsequent offending (Bales and Piquero, 2012; Gendreau, Goggin, and Cullen, 1999; Nagin, Cullen, and Jonson, 2009; National Research Council, 2014; Villettaz, Killias, and Zoder, 2006; Weatherburn, 2010). Gendreau et al. (1999: 7) concluded that prison may promote offending behavior by damaging the “psychological and emotional well-being of inmates” (see also Maruna and Toch, 2005). Clemmer (1958) introduced the concept of *prisonization*, which refers to the process by which prisoners adopt the customs, values, and norms of prison, some of which may be inappropriate for life on the outside. One of the major concerns of long termers relates to the maintenance of a positive self-image and self-esteem despite the challenges posed by the prison setting over long periods of time (Flanagan, 1981).

*Trauma and mental health.* The significant prevalence of traumatic experiences and mental health disorders among the prison population has been underlined in various studies (e.g., Fazel and Danesh, 2002; Haney, 2006; James and Glaze, 2006; National Research Council, 2014; Wolff, Shi, and Siegel, 2009). Deterioration of mental health during the course of a prison sentence has been linked to overcrowding and solitary confinement (Haney, 2006; National Research Council, 2014). Others have drawn attention to the fact that individuals serving life sentences are characterized by distinctive mental health needs (Dye and Aday, 2013; Liem and Kunst, 2013; Taylor, 1986, Yang, Kadouri, Révah-Lévy, Mulvey, and Falissard, 2009). Mauer et al. (2004) drew on data collected by the Bureau of Justice Statistics to underline the greater prevalence of mental health problems among lifers when compared with the general population of prisoners; nearly one in five lifers had a mental illness versus one in six in the general prisoner population. Dudeck et al. (2011) also found that the prevalence of trauma is significantly higher among long-term prisoners when compared with the general population and with short-term prisoners. Long termers are likely to experience the repercussions of trauma more intensely than other individuals exposed to traumatic incidents in part because of the heightened risk of exposure to new traumatic experiences (Dudeck et al., 2011). Liem and Kunst (2013: 336) reported a “specific cluster of mental health symptoms” among 25 released lifers, including “chronic PTSD . . . , institutionalized personality traits (distrusting others, difficulty engaging in relationships, hampered decision-making), social–sensory disorientation (spatial disorientation, difficulty in social interactions) and social and temporal alienation (the idea of ‘not belonging’ in social and temporal setting).”

Although these studies emphasized the greater prevalence of mental health impediments and trauma among individuals serving long sentences, little is known about whether

prison time leads to the development of these problems or whether it merely exacerbates a preexisting condition (Schnittker, Massoglia, and Uggen, 2012). When controlling for health problems prior to incarceration, Massoglia (2008) found that incarcerated individuals were more likely to suffer from infectious diseases and stress-related illnesses, including anxiety, depression, and insomnia. These results compelled Massoglia to conclude that imprisonment exerted a negative impact on health outcomes. Similarly, Wildeman, Schnittker, and Turney's (2012) results suggested immediate and persistent effects of incarceration on major depression. Schnittker et al. (2012) found that individuals with histories of incarceration had a higher rate of exposure to early risk factors such as substance abuse, child abuse and neglect, and childhood poverty. Although incarceration was found to be linked to mood disorders, some psychiatric disorders that were prevalent among former inmates emerged earlier in the life course, prior to incarceration. Perhaps most relevant to the case of long termers and lifers, some significant associations between incarceration and lifetime disorders dissipated when focusing on disorders that had occurred in the previous 12 months, suggesting an attenuated relationship between incarceration and psychiatric disorders over time. These findings draw attention to the complexity of the incarceration–mental health link and stress the need to further investigate the long-term effects of incarceration.

Despite the important contributions of these recent studies, our understanding of how psychological well-being and other health outcomes vary over the course of long periods of incarceration remains limited. Some prisoners report having *been* in prison before *coming* to prison, highlighting thinking styles that may promote offending behavior. We know little about whether prison reinforces these cognitions or breaks them down over extended periods of time. Although we know that prison is a highly stressful environment (Hassine, 2004; Johnson and Toch, 1982; National Research Council, 2014), no study has, to our knowledge, conducted systematic and regular assessments of changes in cognitions as well as physical and mental health indicators during the course of a prison sentence with a reasonably sized, generalizable sample of prisoners. This shortcoming of prison research has particularly limited our understanding of the progress or relapses exhibited by long termers and lifers throughout their sentences.

*Prisoner coping and adaptation strategies.* Not all research has suggested deterioration in the well-being and adjustment of prisoners over the course of extended periods of incarceration. In a comparison of forensic–psychiatric examinations conducted with a sample of 87 long-term German prisoners at the beginning and end of their sentences (with an average sentence length of 14.6 years), (2012) found a decline in the prevalence of psychiatric disorders over time. Emotional stability improved between the first and last assessments, and depression and aggressiveness decreased. Dettbarn concluded that “there was no evidence that longer duration of sentence per se led to physical illness or a diminution of cognitive capacity” (2012: 238). This study is, however, limited by the absence of a control group, as well as by the fact that the analyses are mainly descriptive, do not include any control

variables, and draw on only two data collection points. In addition, the German prison setting is quite different from that of American correctional facilities.

In their comparison of the profiles of short- and long-term prisoners, MacKenzie and Goodstein (1985) found that prisoners who had recently arrived in the facility and who were anticipating a long sentence were most susceptible to stress, anxiety, fear, and depression, whereas long termers and lifers who had already spent several years in prison developed coping mechanisms to adapt to the incarceration experience (see also Leigey, 2010; Zamble, 1992; Zamble and Porporino, 1988). The negative effects of incarceration (and particularly long-term incarceration) can be moderated through the conditions of detention and the varying adaptation techniques employed by different prisoners. For instance, Flanagan (1981) found that increased maturity and interactions with other long termers led to a distinct outlook among long-term prisoners, who were characterized by specific attitudes and behaviors designed to facilitate survival in prison. Examples of such attitudes and behaviors included conflict avoidance inside the prison and a desire to use prison time in a fruitful manner in contrast to merely “serving time.” These findings also highlight the erroneous assumption that all long termers and lifers constitute a uniform group (MacKenzie and Goodstein, 1985).

Similarly, in a comprehensive longitudinal follow-up of 25 long termers, Zamble (1992) found that some prisoners learned to adapt to the circumstances of long-term incarceration. These individuals maintained contacts with the outside world, showed reduced emotional problems as well as “stress-related medical problems,” and were involved in fewer incidents involving disciplinary action. While making clear that these findings should not be used as a justification for increased recourse to long-term incarceration, Zamble (1992: 423) concluded that “the special conditions of imprisonment for long and indefinite periods may actually promote the development of more mature ways of coping and behaving.”

Liebling (1999: 287) argued that research that has found minimal effects of incarceration on prisoner well-being is partly biased by issues of operationalization of harm, and “the failure of research on the effects of prison life to ask the right questions or to ask in an appropriate kind of way how imprisonment is experienced.” Because long-term follow-ups of prisoners are infrequent, the body of knowledge on the impact of incarceration on long termers and lifers is particularly lacking. Although many of these studies are characterized by methodological shortcomings (e.g., small sample size) or are dated, they suggest that periods of incarceration, if used adequately, can promote positive change. Once prisoners have come to terms with the fact that they will be incarcerated for a significant number of years, they may seek a new meaning to their lives (Carceral, 2006; Hassine, 2004). Significant cognitive and behavioral changes may occur with adequate support from staff, as well as access to programs and activities that stimulate personal development (Toch, 2010).

Sampson (2011) argued that there has been an ideological shift in incarceration research during the course of the last few decades, from a focus on the potential benefits of imprisonment (e.g., the deterrence and incapacitation paradigms) to the undesirable or

criminogenic effects of incarceration. He argued that although we have gained a great deal of knowledge from these two approaches, it has come “at the price of complexity and a kind of stalemate of dueling advocates that view incarceration either as ‘good’ or ‘bad’” (p. 824). This dualistic view may not capture the intricacies of the prison experience and its consequences. More importantly, it limits our understanding of how prison time can be used strategically to produce desirable outcomes.

In short, although much of the research that has examined patterns of change among prisoners across time has not shown strong evidence of deterioration among those serving extended sentences (and have, in some studies, shown improved coping and reduced recidivism rates), these conclusions must be interpreted with caution. This research is limited in scope. Many of these studies were conducted outside of the United States, where the prison populations differ in both size and racial composition. Varying definitions of long-term incarceration have been employed in these different studies. The measurement of harm has been less than ideal (Liebling, 1999). In addition, most of this research has relied on data collected several decades ago. To our knowledge, no contemporary U.S.-based study (i.e., conducted in the last 25 years) has found positive effects of incarceration. However, for this same period, we do not know of any systematic longitudinal study of long-term prisoners. The contemporary prison setting is quite different from what it was 30 or 40 years ago. The significant increases in the number of individuals incarcerated and the length of prison sentences have led to a new set of problems associated with the management of the prison population, namely, issues of overcrowding, scarce resources, and limited access to programs and services. If the psychological well-being of prisoners deteriorates over time, then it is crucial to find ways to counter these negative repercussions and to use prison time as a means of stimulating positive change. Furthermore, if it is true that lifers may “act as a stabilizing rather than disruptive force in the prison environment” (Cunningham and Sorensen, 2006: 683), then these individuals have the potential to play a key role in minimizing the negative consequences of incarceration.

### *Long Termers and Lifers: Potentially Valuable Leaders in the Prison Environment*

Because they will spend many years in prison, long termers and lifers are important assets to the prison community and can become influential leaders in this environment. Leadership is a quality that shapes and enriches any given community, and the prison community is no exception. Given their prolonged presence in prison, long termers and lifers are ideal candidates for positions of leadership and mentorship in this environment. The changes that occur as a result of adopting a leadership position may lead to cognitive restructuring, attitudinal changes, improved behavioral outcomes, and an enhanced prison climate. The leadership role also may, inadvertently, lead to good behavior and early release. One example of the significant influence that can be exerted by prisoners engaged in a leadership role was observed by Lila Kazemian in a French jail. In this particular facility, the prison director selected prisoners to act as mentors in their wing. The responsibilities of these individuals



were comparable with that of a resident advisor in a dorm; prisoners in the wing were encouraged to consult with these mentors with any concerns prior to contacting the prison director or other staff members.

Sixty years ago, Cressey (1955) highlighted the value of involving prisoners and former prisoners in the rehabilitation process of fellow inmates and former inmates. He discussed the importance of group interventions in the correctional setting. Riessman (1965) later underlined the benefits of the “helper therapy principle,” namely the positive individual outcomes that emerge as a result of being in the “helper role.” These benefits include greater self-esteem, improved mood and psychological well-being, an enhanced sense of purpose, the development of a new identity, and modified (and positive) reactions and treatment that occur as a result of the new role (see also Piliavin, 2003; Skovholt, 1974).

The body of research investigating the impact of mentoring on offending outcomes is limited. Jolliffe and Farrington (2007) argued that these studies often have employed flawed research designs (i.e., small-scale studies with limited generalizability or lack of inclusion of a control group). In their review of research on the effectiveness of mentoring programs on offending outcomes, Jolliffe and Farrington (2007) found that mentoring may reduce recidivism but that these programs are most effective when integrated into a broader multimodal intervention. Maruna’s (2001) study of desistance suggested that the desisting self-narrative frequently involves adopting a mentoring role. Desisting offenders in Maruna’s study were more susceptible to adopting the role of a helper. By drawing on a sample of 228 formerly incarcerated individuals in New York, Lebel (2007) found that the helper role was incompatible with criminal attitudes and behaviors. More than half of the individuals in Lebel’s sample (58.2%) expressed the desire to engage in initiatives that would enable them to take on the role of a helper. Lebel (2007) also found feelings of remorse to be a strong predictor of the helper orientation, suggesting that coming to terms with the consequences and harm caused by past offenses may be required before an individual can engage in the role of helper. Lebel (2007: 20) concluded that the helper orientation “appears to transform individuals from being part of ‘the problem’ into part of ‘the solution.’” Similarly, Toch (2010) found that “altruistic activity” (i.e., activities that are designed to help individuals in need) resulted in many psychological benefits for the helpers, including improved self-esteem, a greater sense of purpose, and a sense of accomplishment (see also Roberts et al., 1999). Overall, assuming a mentor role has the potential to empower prisoners in an environment where they often may feel that their power has been taken away.

In addition to the potential positive effects on the helpers, interventions guided by long termers and lifers also benefit fellow prisoners. The helper is more likely to establish stronger bonds with participants if they share common past or current experiences. In this regard, other prisoners may perceive long termers and lifers as having more standing and integrity than practitioners who have not experienced incarceration. As such, long termers and lifers may be in a better position to exert a profound impact on their fellow prisoners.

The therapeutic community model is a good example of the type of intervention that serves both helpers and helpees (De Leon, 2000).

In short, as argued by Toch (2010: 276), “prisons have a great deal to gain—and little to lose—in multiplying the opportunities for inmates to engage in altruistic activities that add a human face (or a humane face) to corrections.” Toch also advocated for the creation of special groups for lifers, “for whom altruistic activities can become the valued core of an in-house career” (p. 277). It may be that long termers and lifers are already contributing to the enhancement of the prison community, but to our knowledge, virtually no data (other than anecdotal) exist to verify this claim.

Beyond the effects on the prisoners’ well-being, leadership roles granted to individuals serving long sentences also may contribute to an improved prison climate, which may entail benefits for both staff and prisoners. Because long termers and lifers have longer and more sustained exposure to the prison environment, they can play an important role in shaping the prison climate. It is reasonable to presume that prisoner maladjustment problems and other behavioral issues (mental health, substance use, violence, confrontational attitudes, etc.) would adversely affect the prison climate and create a more stressful work environment for correctional staff. Crewe, Liebling, and Hulley (2011) suggested that the links between prisoner and staff perceptions of prison quality of life need to be understood more fully, and more research is needed on this topic.

### *Mitigating the Long-Term Collateral Consequences of Incarceration: Impact on Families and Communities*

The effects of incarceration extend beyond the prison walls. Scholars have highlighted the collateral consequences of incarceration (National Research Council, 2014; Travis, 2005; Travis and Waul, 2003), which are likely to be amplified with longer prison sentences. The undesirable consequences of imprisonment expand beyond the prisoners to their intimate social networks (family and friends) and communities. Inspired by Clemmer’s (1958) work, Comfort (2008) introduced the concept of “secondary prisonization,” which refers to the process by which the prison world infiltrates and transforms the personal lives of the families of prisoners. The collateral effects of incarceration can be classified into three broad categories: (a) the disintegration of family ties, (b) the adverse impact on the children of prisoners, and (c) the destabilizing effect on communities. Virtually no research has examined whether these effects are distinct for prisoners serving life or otherwise long sentences, and little is known about whether these collateral consequences are amplified over time or whether families, children, and communities adapt to the permanent absence of the incarcerated individual.

*Deterioration of family ties during long periods of incarceration.* Prior work has underlined the negative impact of incarceration on social bonds (family, work, school, and the community; see King, Mauer, and Young, 2005; National Research Council, 2014;

Sampson and Laub, 1997; Travis, 2005; Travis and Petersilia, 2001).<sup>11</sup> Gust (2012) summarized the ways in which imprisonment impacts the family. It exerts an adverse effect on family structure and living arrangements, strains family relationships, creates a financial burden, causes significant emotional stress, and leads to stigma, which impacts the prisoner as well as his or her family members. Mothers with incarcerated partners are more likely to experience economic and housing insecurity (Geller and Franklin, 2014; Geller, Garfinkel, Cooper, and Mincy, 2009; Schwartz-Soicher, Geller, and Garfinkel, 2011). They are more likely to be exposed to increased levels of stress and to develop mental health issues (Wildeman et al., 2012). Changes in caregiver and living arrangements (which tend to be more common among children with incarcerated mothers; see Mumola, 2000) can cause significant disruptions in the lives of children. Marriages are more likely to dissolve among incarcerated than nonincarcerated men (Western, 2006), although it is unclear whether this association is more pronounced for individuals serving long sentences. Some studies have highlighted the heterogeneous effects of incarceration on partners and children, which partly result from variations in family systems, parenting styles, and individual propensities (Giordano, 2010; Turanovic, Rodriguez, and Pratt, 2012; Turney and Wildeman, 2015).

How much is known about the deterioration or reinforcement of family ties over time? We know little, given the scarcity of longitudinal follow-ups of prisoners and their families across various periods of the life course. Flanagan (1979: 234) explained that “the basic problem is this: [F]amily members and friends who can (and often do) wait for three years cannot (and often do not) wait for thirteen years.” Some authors have suggested that although family members and friends often are perceived as a “source of strength” for prisoners, many of these relationships do not survive long prison sentences (Bales and Mears, 2008; Flanagan, 1982).

Some research has indicated that more frequent visits during the course of a prison sentence are associated with a reduced likelihood of recidivism (Bales and Mears, 2008), as well as other outcomes linked to a successful reintegration into the community after release (Wooldredge, 1999). The importance of maintaining family ties in reentry efforts is emphasized in numerous studies on the desistance and reintegration processes of formerly incarcerated individuals (Laub and Sampson, 2001; Travis, 2005; Travis and Petersilia, 2001). However, the knowledge base on the short- and long-term effects of family visits on prison behaviors is limited. In an analysis of the link between visitations and prison infractions, Siennick et al. (2013: 435–437) found that the probability of prison misbehaviors declined before visits, increased immediately after the visits, and progressively dropped again to average levels of infractions, suggesting that although visits may reduce the “pains of imprisonment,” they “may not have the lasting effects needed to produce sustained

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11. Some studies have highlighted the caveats of this relationship. For instance, Edin, Kefalas, and Reed's (2004) study suggested that incarceration was not necessarily detrimental to relationships that were harmed by the incarcerated partner's lifestyle choices prior to prison.

improvements in behavior.” More longitudinal analyses are needed to assess whether prison visitations and frequent contacts with the family exert a lasting effect on prisoner misbehaviors and adjustment after release. Although Siennick et al. controlled for sentence length, the analyses did not distinguish the impact of prison visits on the behaviors of short- versus long-term prisoners.

Some have argued that the maintenance of contacts with an imprisoned parent during the period of confinement is effective in reducing the negative consequences of incarceration on children (e.g., see La Vigne, Naser, Brooks, and Castro, 2005), but others have found that these visits may exert negative effects (e.g., stress, anxiety, humiliation, etc.) on family members and particularly children (Comfort, 2008; Hairston, 1998). Despite the growing literature on this topic, much remains unknown about the mechanisms underlying the effects of incarceration on families (Dyer, Pleck, and McBride, 2012), both positive and negative. Specifically, it is unclear whether findings from this body of research would differ if we shifted the focus to long termers and lifers. We know little about whether family ties disintegrate after a certain number of years, particularly among individuals who have little hope of release. The potential role of lifer peer networks in helping to cope with these emotional challenges needs to be understood more fully.

*Negative effects of incarceration on the children of prisoners.* In 2007, 1.7 million children (2.3% of the population of children in the United States) had a parent in a state or federal facility (Glaze and Maruschak, 2008). Between 1991 and 2007, the number of incarcerated parents with children younger than 18 years of age increased by 79% (Glaze and Maruschak, 2008). A 2004 survey suggested that a substantial number of state (52%) and federal (63%) prisoners reported having at least one minor child (Glaze and Maruschak, 2008). Unsurprisingly, parental incarceration has been found to be a stressful experience for children (National Research Council, 2014). What remains unknown is whether these detrimental effects are amplified over the course of long periods of incarceration or attenuated as a result of the children adapting to the parent’s absence. Life-course studies are needed to address this gap in knowledge. Such research would allow for a better understanding of the “intergenerational transmission of offending,” as argued by developmental criminologists (e.g., Farrington and Welsh, 2007).

Children of incarcerated parents have been found to be at higher risk of developing inadequate self-esteem, issues relating to cognitive functioning, difficulties at school, behavioral problems and delinquency, as well as later incarceration (Hanlon et al., 2005; Huebner and Gustafson, 2007; Johnson, 2009; Johnson and Easterling, 2012; Kinner, Alati, Najman, and Williams, 2007; Murray and Farrington, 2005, 2008; Murray, Loeber, and Pardini, 2012; National Research Council, 2014; Poehlmann, 2005; Roettger and Swisher, 2011; Wakefield and Wildeman, 2011; Walker, 2011; Wildeman, 2009, 2010). Parental incarceration has been regarded as a traumatic experience for children (Arditti, 2012; Travis and Waul, 2003), and some authors have suggested that the strains of the incarceration experience often are transferred to children (Comfort, 2008; Hairston, 1998).

The incarceration of a father may result in turnovers in the mother's romantic partners, which can lead to poor parenting practices (Arditti, Burton, and Neeves-Botelho, 2010). Murray et al.'s (2012) systematic review and meta-analysis suggested that parental incarceration increased the risk of child antisocial behaviors but was not significantly associated with mental health issues, drug use, and performance in school. Because of data limitations, Murray et al.'s (2012) analyses did not make the distinction between short and long prison terms, and thus, it is unknown whether these findings are equally applicable to long termers and lifers.

*Impact of incarceration on communities.* In addition to the impact of incarceration on families, Clear (2008) argued that incarceration also exerts a significant effect on the infrastructure of communities, the types of relationships established among residents of the neighborhood, and the safety of the community. Because of the spatial concentration of crime and incarceration, the destabilizing impact of imprisonment disproportionately affects specific communities (Lynch and Sabol, 2004). The removal of a large number of residents, and for extended periods of time, impacts social networks and controls in the community (Bursick and Grasmick, 1993; Lynch and Sabol, 2004). It deprives the community of the contributions of these individuals to the local economy (Venkatesh, 2006). Communities that lose a disproportionate number of residents to incarceration are characterized by a reduced number of adult men who may have links and contacts to the employment world, resulting in limited opportunities for legitimate employment in given neighborhoods (Roberts, 2004); this disadvantage disproportionately affects minorities (Sabol and Lynch, 2003). The skewed male-to-female sex ratio in a neighborhood has been linked to a higher rate of family disruption (Sampson, 1995). The incarceration of a large number of males in a community creates more competition in the pool of eligible partners, which may add to the mothers' reluctance to end unstable relationships and to the men's reduced motivation to remain committed to their parenting and partner roles (Clear, 2008). Thomas and Torrone (2006) also found that communities characterized by high incarceration rates had higher subsequent rates of sexually transmitted diseases and teenage pregnancies. Despite these findings, the National Research Council (2014) cautioned against drawing definite conclusions about the impact of incarceration on particular neighborhoods, arguing that the evidence remains inconclusive as a result of the methodological challenges in establishing causality and the lack of reliable data.

The permanent or long-term removal of a large number of individuals (mostly males), as is the case for long termers and lifers, can be paralleled to a system of exile or a process of mass migration. Just as we know little about whether prisoners and their families adapt to long sentences, our knowledge of the deterioration (or adaptation) of communities to the long-term absence of individuals serving long sentences is equally underdeveloped. More research is needed to assess the impact of intermittent versus permanent removal of residents on the welfare of communities.

*Prison Career Approach and the Desistance Process*

Time in prison is assessed through two main indicators of success or failure: behaviors in prison (correctional risk) and postrelease outcomes (community risk). The concept of desistance, which we regard as a process involving a series of cognitive, social, and behavioral changes leading up to the cessation of criminal behavior, cuts across these two dimensions. Yet, the desistance literature has largely ignored changes that occur during periods of incarceration. The effectiveness of prison is usually assessed on the basis of postrelease behavior, principally the absence of recidivism. This practice poses important caveats. Because long termers and lifers spend a substantial number of years in prison, an emphasis on postrelease outcomes overlooks the important changes that occur while these individuals are incarcerated. Few studies have documented the progression (or disintegration) of criminal careers, of the desistance process, and of other social and cognitive changes that take place over the course of a prison sentence. This research is particularly scarce with samples of long termers or lifers. Liebling (2012) rightfully argued that theories of desistance may not take into account the full context of the prison experience.

Irwin (2009) described the desistance process of 17 incarcerated men serving sentences of 20 or more years. He found that most lifers changed drastically during the course of their prison sentence. Irwin described a process of awakening, the point at which individuals understand that their actions have led them to their current situation. This step in the desistance process is crucial, and it occurs at different points in time depending on a host of factors, such as maturity level, commitment to crime-promoting beliefs and values, and adherence to the prison lifestyle. Many authors have highlighted the importance of identity transformation in the process of desistance (Bottoms, Shapland, Costello, Holmes, and Muir, 2004; Burnett, 2004; Giordano et al., 2002; Maruna, 2001). Most prison environments might not be conducive to the development of a reformed, positive self-image and identity. To reduce and eventually abandon harmful behaviors and attitudes, individuals need to be exposed to socially acceptable alternatives. We need to more clearly understand the identity shifts that occur among long termers and lifers, and how these shifts impact their attitudes, behaviors, and relationships over time.

We know that individuals who serve long sentences tend to be older at release when compared with those who serve shorter sentences (Crayton, 2012) and that recidivism rates are lower among older individuals when compared with their younger counterparts. Toch (2010: 8) argued that “age is a proxy for whatever transformations have occurred among dedicated middle-aged prisoners that we do not fully understand.” This does not imply that long termers or lifers will spontaneously desist or age out of crime, or that they are a lost cause and that it would be wasteful to invest resources to promote their process of self-transformation. All individuals do not age out of crime at the same rate. Blumstein, Cohen, and Hsieh’s (1982) work suggested that individuals who remained active in crime in their early 30s had the most prominent residual criminal careers. Kazemian and Farrington

(2006) also noted that the decline in the residual number of offenses was not as linear as the decline in the residual number of years remaining in criminal careers, suggesting that offending rates do not decline at the same rate for different individuals.

Policy makers and researchers alike favor a result-oriented approach and fixate on recidivism as an indicator of success and failure. A recidivism-focused approach disregards changes and progress exhibited in other behavioral, cognitive, and social outcomes. Studies have found that criminal careers are characterized by a great deal of intermittency, and several researchers have acknowledged the relevance of perceiving desistance as a gradual process (Bottoms et al., 2004; Bushway, Piquero, Broidy, Cauffman, and Mazerolle, 2001; Bushway, Thornberry, and Krohn, 2003; Kazemian, 2007; Laub and Sampson, 2001, 2003; Le Blanc and Loeber, 1998; Maruna, 2001). As a result, the complete abandonment of offending activities is unlikely to occur suddenly, especially among individuals who have been highly active in offending from a young age; criminal career researchers have consistently established the strong link between early onset and persistent offending (see review in Piquero, Farrington, and Blumstein, 2003). Therefore, focusing solely on the final state of termination provides limited guidance for intervention initiatives and neglects to offer support and reinforcement during periods when they are most needed (i.e., periods of reassessment and ambivalence toward desistance or persistence; see Burnett, 2004). The time for reflection and potential scope for change is particularly significant for long termers and lifers, who spend extended periods of time in prison.

How can we study desistance in the prison context? Future research needs to determine whether the knowledge base about desistance is applicable to prisoners. Life-course and criminal career research often has turned a blind eye to offending that occurs during periods of incarceration. Individuals can and do engage in offending behaviors while incarcerated, albeit at a lower rate and in different forms; this fact has been evidenced in research on institutional misconduct (e.g., Cunningham and Sorensen, 2006) and stands in contrast to the assumptions made in the life-course and criminal career literature. If offending can occur while in prison, it follows that significant changes in the desistance process also may ensue during periods of incarceration. Consequently, it is imperative to integrate prison time into analyses of criminal career patterns to understand how the desistance process operates during these periods, particularly during long sentences. Despite their growing presence in American prisons, a narrow body of research has been dedicated to individuals serving life sentences, and our understanding of prison lives remains inadequate. The National Research Council (2014) is correct in stating that prison is often regarded as a “black box” and that the state of knowledge on the changes that occur during extended periods of incarceration is limited.

### **Suggestions Going Forward**

Based on the research presented in this article, we wish to offer some recommendations for future research and policy.

### *Differential Treatment and Interventions for Long Termers and Lifers*

Most prisoners are eventually released, but a growing number of U.S. inmates spend a considerable portion of their lives in prison. We know little about whether long termers and lifers are characterized by distinctive psychological, social, and health needs when compared with other prisoners, but it is clear that sentence planning is likely to be distinctive for this population. We have suggested that it may be highly beneficial to encourage long termers and lifers to engage in leadership positions in prison. Such initiatives may help both the helpers and those that they are seeking to help, and may lead to an improved prison climate. In addition, the preparation for the release of an individual who has spent a large part of his life in prison is likely to be quite different from the release of a short-term prisoner. We need to reassess, with contemporary samples of prisoners, the effectiveness of differential release preparation programs and other intervention strategies for long termers and lifers.

Participation in prison programs and prisoner-led groups can contribute greatly to the transformation process of lifers (Irwin, 2009). According to Toch (2010: 8), “programs can make a difference not only because they teach skills, but also because they can instigate or facilitate personal transformation . . . program involvements permit prisoners who are ready and willing to change to demonstrate that they have done so.” The National Research Council (2014) highlighted the need to develop and invest in prison programs that may minimize the harmful and criminogenic effects of incarceration, which include extreme idleness and boredom, mental health deterioration, disintegration of family ties, and increased risks of recidivism. Promising programs include interventions based on the risk–need–responsivity model (Andrews, Bonta, and Hoge, 1990), substance use treatment with postrelease follow-up services, and cognitive-behavioral programs. French and Gendreau’s (2006) meta-analysis suggested that intervention programs that draw on behavioral strategies (i.e., focusing on the criminogenic needs of high-risk offenders) are the most effective in reducing subsequent misconduct in prison and recidivism rates in the community.

Interventions targeting long termers and lifers would ideally assist individuals as they transition to life in prison for a long sentence. Some programs have specifically targeted lifers, and two examples are noteworthy. *Coming to Terms* is a 15-week group-based program, which was written by Kathy Boudin and Cori Chertoff and developed by the Osborne Association. The core objective of the program is to promote self-assessment, responsibility, remorse, and apology, as well as to encourage individuals to make amends with their past selves and behaviors. The intervention draws on group exercises, writing assignments, and various other activities that enable participants to grasp the harm that they have caused to others as a result of their past behaviors (see full description on the Osborne Association website, [osborneny.org](http://osborneny.org)). A pilot version of the program was implemented in two correctional facilities in New York State, and preliminary analyses reveal promising outcomes. The second program, *Lifeline*, was first developed in Canada. It involves lifers who have successfully returned to the community (i.e., who have remained crime free for at least 5 years and



who are regarded as positive role models). These individuals serve as mentors to prisoners who are to be released, help them cope with the adversities of detention, and assist them in preparing for the pending challenges after release. The program was found to have positive effects on the successful reintegration of lifers after release (Correctional Service of Canada, 2009).

### *Developing Research in Prisons*

Prison research is a complex endeavor partly as a result of the difficulties in gaining access to correctional facilities for research purposes. We have stressed the need for the research community to study life-course and criminal career patterns during periods of incarceration. We need contemporary, prison-based longitudinal studies to reassess the effects of incarceration during prolonged periods of time in prison (National Research Council, 2014). Such studies would involve systematic and regular assessments of the changes that occur throughout the course of a prison sentence by drawing on a generalizable sample of prisoners. Empirical tests of the potential benefits of investing in the needs of prisoners serving long-term sentences also are lacking. Ideally, experimental or quasi-experimental longitudinal designs (or, minimally, matching procedures) should be employed to compare the short- and long-term effects of needs assessments and programming on subsequent attitudes, behaviors, and expectations for release for those serving short and long sentences.

This type of research poses many challenges. It would require longitudinal data collected at several points during the period of incarceration with questions of a potentially sensitive nature (i.e., offending, mental health outcomes, etc.). This might pose problems with institutional review boards, which are highly sensitive to research involving vulnerable populations (for an account of the tedious IRB process involved in prison research, see Kazemian, 2015). In addition, research of this nature requires the cooperation of the Department of Corrections and the willingness to provide additional staff and resources when researchers are present in the facility, which can be a tall order when resources are limited. The reluctance of correctional administrations to collaborate with researchers may stem from the perception that academics can be overly disparaging of prison practices and of the correctional system as a whole. Correctional administrators may feel that they have little to gain from research inside their facilities and that it is likely to lead to a great deal of criticism. We believe that this divide can be best reconciled if we attempt to reach a more balanced view of correctional institutions and to develop our knowledge base on how to improve these environments, as opposed to simply take a stance on whether we regard prisons as “good” or “bad.” These are significant barriers, and better partnerships between academics and correctional officials are crucial to addressing these challenges.

Of course, it is expected that correctional authorities should exercise some control over who is granted access to facilities for research purposes to ensure that the presence of researchers does not compromise security concerns inside the facility and that the research makes a useful contribution to both research and practice. Because of the heavy burden on

correctional systems and relatively limited resources, access to prisons for research purposes is inevitably selective, and it is incumbent on the researcher to demonstrate that a given study is of practical value to the correctional authorities and prisoners.

### *Transparency*

We have drawn attention to the relatively limited knowledge base on the progression of prison lives over time; these data are lacking because we do not consistently document the perspectives of prisoners and because prisons are closed environments. In the United States, few initiatives are in place to promote transparency in prisons, such as governmental oversight measures, independent commissions, access to prisons for judges, the publication of prisoner newspapers, and other similar resources. Other countries have adopted measures to uphold accountability in their correctional systems. For instance, since the enactment of a correctional law in 2009, the French legal system includes a provision that occasionally allows ordinary citizens to enter prisons and provide feedback on the disciplinary sanctions imposed on prisoners. Given the more limited contacts of long termers and lifers with the outside world, such practices are especially valuable for this population.

Increased exposure to prisons and prisoners, for ordinary citizens as well as politicians and key decision makers in the criminal justice system, may shatter the perception of social and moral distance with inmates and the perspective that these individuals constitute a distinct class of human beings. Johnson and McGunigall-Smith (2008: 337) explained that “outsiders find it hard to put themselves in the shoes of prisoners.” This view was expressed by Mark Earley, the former Attorney General of Virginia (R-VA) at the 10th Annual H.F. Guggenheim Symposium on Crime in America, held at John Jay College of Criminal Justice in February 2015. Earley explained that he was supportive of tough-on-crime policies (e.g., Three-Strikes laws, truth-in-sentencing legislation, abolition of parole, prison building programs) at an earlier point in his career. He changed his outlook as a result of his work with the Prison Fellowship organization, which took him inside prisons and, in his own words, made him realize that prisoners were not inherently different from him:

For most of my life, I viewed them as something other than myself, someone with whom I could not identify with, had little empathy or compassion for . . . then as you begin to talk to the actual people who are there, at least for me, I realized there was only a few degrees of separation between them and myself, between my children and some of these young people.

This former politician’s perspective highlights the potential ideological shifts that may occur as a result of greater exposure to adjudicated populations, although these shifts are most likely to stimulate significant change if they occur while individuals are in office and in a position to exert a direct impact on policy. Gottschalk (2014: 189) argued that the reluctance of politicians to endorse more sensible penal policies does not stem from the

threat posed by long-term prisoners to the outside community, but rather from the fact that these individuals “pose a potential risk to political careers.”

### *Parallel Universe*

The commitment to reassess the incarceration experience from the perspective of long termers and lifers is heightened. For this population, the immediate priority may not be related to preparation for release, but it is linked to how time in prison can be used in a productive manner during long periods of incarceration, which may indirectly impact release outcomes.

The current prison environment tends to be incompatible with the outside world, and these differences are most felt by individuals serving long sentences. How can prisoners be expected to be prepared for a regular work schedule if they have remained inactive during the day for several years or decades, or be expected to interact in a socially acceptable manner and to trust others when the prison environment thrives on mistrust and displays of masculinity and aggressiveness? The nature and structure of the prison system may result in individuals losing the ability to make plans and decisions after long periods of incarceration (Haney, 2006). The problem-solving solutions adopted in prison may be incompatible with strategies promoted in the outside world (Jamieson and Grounds, 2005). This disconnect is not necessarily reflective of the individual characteristics of prisoners; the prison environment may be inherently conducive to such responses, even among individuals who do not display at-risk profiles. Although some individuals may present inadequate conflict resolution skills prior to their arrival in prison, the prison system can be structured in a way to either enhance or help to break down these undesirable attitudes and behaviors. Just like exposure to environmental risk factors may have differential effects on offending behavior depending on a person's genotype (i.e., gene-environment interaction; see Caspi et al., 2002), the prison environment may enhance the influence of individual traits linked to violent and other problem behaviors. The knowledge base examining how individual characteristics interact with features of the prison environment to impact behavior is, to our knowledge, nonexistent.

It would be worthwhile to initiate a discussion about setting up prisons as alternative societies in which individuals live according to standards that are not starkly incompatible with the outside world. The concept of a “parallel universe” was introduced in 2000 by Dora Schriro, the director of the Missouri Department of Corrections at the time. This new strategy was “premised on the notion that life inside prison should resemble life outside prison, and that inmates can acquire values, habits, and skills that will help them become productive, law-abiding citizens” (Schriro, 2000: 1). Schriro promoted a system that encouraged prisoners to make decisions and to be held accountable, that stimulated personal responsibility, and that enabled individuals to understand community expectations and make them compatible with their personal attitudes. In essence, Schriro suggested that prisons should parallel the outside world.

The growing emphasis on prisoner reentry, although largely constructive, may have had one major unintended consequence: By shifting the focus to life after prison, it has taken some attention away from life inside the prison and from individuals who are not approaching release. The reentry discourse has emphasized outcomes related to the return of prisoners to the outside community (e.g., preparation for release and the prevention of recidivism) and may have inadvertently resulted in a diminished focus on the quality of life inside prisons. This oversight particularly impacts long termers and lifers, who will not be released for several years or even decades. As a result, many long-term prisoners “are being denied access to programs and activities that might make their days without end more bearable” (Gottschalk, 2014: 170).

### **Conclusion**

A life sentence seldom means life in prison. Most individuals (93%, according to Petersilia, 2009) sent to prison are eventually released. No similar estimate has been provided for lifers. Good behavior, the possibility of parole, and overcrowding may lead to the release of individuals serving life sentences or to early release for individuals serving long sentences. Nonetheless, for a growing number of prisoners, the reality is that a prison sentence does not constitute a short absence from ordinary life in the community. In this article, we sought to draw attention to the fact that long termers and lifers are a unique population that requires special consideration. These individuals have been largely neglected by both researchers and policy makers. Among researchers, this neglect is caused in part by the common belief that criminal careers are halted during periods of incarceration and that it is irrelevant to study the process of desistance from crime among individuals who are removed from the community. In addition, research has been hampered by the lack of comprehensive data from the Bureau of Justice Statistics in this area, despite the increasing presence of lifers in prisons in recent decades. We argued for the need to document more accurately the changes that occur during periods of incarceration, particularly among individuals serving long sentences. We know from the body of research on institutional misconduct that some prisoners continue to engage in offending behaviors while incarcerated. As a result, we highlighted the importance of integrating prison time in analyses of criminal career patterns, which would enable us to better grasp the shifts in the desistance process during periods of incarceration and to reassess the effects of long-term imprisonment.

Because of the central importance granted to recidivism as an outcome measure and the distant release date of long termers and lifers, criminal justice policies do not prioritize the needs of long termers and lifers. Correctional officials and policy makers are particularly concerned with the threat posed by prisoners after release, and any harmful behaviors in which they may engage while incarcerated are not deemed to pose a direct threat to the outside community. As such, there is no perceived sense of urgency in investing in the needs of this population. Moreover, because of the gravity of their convictions, long termers and lifers may be regarded as “irredeemable,” resistant to change, and unresponsive

to interventions. Notwithstanding the unrealistic expectation to undo, in a few months, habits that have been developed over several years, reentry programs are typically only offered within 6 months of release.<sup>12</sup>

We have argued that long termers and lifers may constitute a valuable resource in the prison environment, that they may help to mitigate the negative consequences of incarceration, and that their well-being is likely to entail diffused benefits for concerned families and communities. Because of their prolonged presence in the prison setting, long termers and lifers represent important assets to the prison community; they can be influential leaders in this environment and may serve as a stabilizing force. Prior studies have suggested that these individuals do not pose a distinctive threat to public or prison safety when compared with other prisoners. Our suggestion to pay more attention to long termers and lifers has little to do with risk. As argued by Johnson and McGunigall-Smith (2008: 332), lifers are “manageable prisoners, some are even model prisoners, but their decent adjustment does not change the fact that their lives are marked by suffering and privation.” It is our view that the deprivation of freedom is in itself a severe punishment, and that imprisonment is most detrimental to the development of individuals when it promotes values, norms, and behaviors that are too harshly incompatible with the outside world.

Although this article has underlined the practical value in considering the needs of individuals serving long sentences, the treatment of this population remains a largely normative issue (Tonry, 2011). We have argued that moral arguments are not inevitably incompatible with pragmatic considerations. Increased investment in long termers and lifers is, first and foremost, an issue of human rights and decency, but it also serves the interests of correctional facilities. Such efforts may promote potentially valuable contributions of long-term inmates to the prison community, enable a productive use of the years spent in prison, and speed up the desistance process. Our call to grant more attention to long termers and lifers does not necessarily emphasize the ultimate measure of “effectiveness” (i.e., the absence of recidivism), but it entails practices that may improve the quality of life inside prisons and for the families of prisoners, which may lead to better behavioral outcomes. Although we do not know whether such practices would have a significant impact at the aggregate level, they are nonetheless compatible with principles of justice. The perceived sense of injustice is a powerful feeling that can foster anger and resentment (Matza, 1964). Although the

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12. The United States is not alone in delaying reentry programs until just prior to release. For instance, Spain has a three-tier inmate classification system in which first-degree offenders are regarded as those who pose the most serious threats to the community or who have committed the most serious offenses ([institucionpenitenciaria.es/](http://institucionpenitenciaria.es/)). Intervention programs are not generally available for these individuals until they progress to the second or third levels (which most do). In a research study conducted by Kazemian in a maximum-security facility in Paris, the limited availability of intervention programs was a frequent concern expressed by the prisoners. Such programs, as well as employment opportunities, become increasingly available as prisoners progress to the next level of security and facility.

effects of individual intervention efforts may not be discerned at the aggregate level, failure to offer such services may reinforce preexisting beliefs about the lack of social justice.

We agree with Tonry (2011) that large-scale and lasting change can only occur at the systemic level. However, this does not imply that we need to cease attempts to improve the situation of the justice-involved population. Although many social policies do not target the root causes of inequality, they constitute attempts to restore equity and balance. For instance, we do not cease to implement affirmative action policies simply because they do not tackle the source of the problem or fail to address the systemic inequalities that may occur at earlier stages of progression through the system. We continue to adopt these policies because it is the just thing to do. A similar argument can be made about prison-based programs. Even if we establish that the explanatory power of many intervention programs on subsequent offending behavior is not overwhelmingly high, especially as they are currently implemented (Lipsey and Cullen, 2007), ceasing to offer these programs would send a strong message about the priorities of decision makers and the importance granted to the well-being of the prisoner population.

This article has not emphasized the financial costs associated with long-term incarceration, but it is evident that maintaining a large population of long termers and lifers exerts a great amount of financial strain on the system (for a more detailed discussion of correctional costs, see National Research Council, 2014). Largely driven by a desire to reduce the financial burden on state budgets, there is currently bipartisan support for reductions in the prison population. A promising new reform movement, funded by large national foundations, has coalesced around the goal of making significant reductions in the nation's prison population (Travis, 2014). These efforts to reduce the prison population will have a limited impact unless the recourse to life sentences is reconsidered. Gottschalk (2014) reminds us that focusing on low-level offenders will not lead to significant reductions in the prison population, and we need to reevaluate our sentencing practices beyond the "low-hanging fruit" that represent nonviolent offenses. Tonry (2014) offered 10 concrete steps to reduce mass incarceration in the United States, including the elimination or significant reduction of LWOP sentences (see also Nellis, 2013). In his testimony before the Charles Colson Task Force on Federal Corrections, Mauer (2015) called for a 20-year cap on federal prison sentences, with provisions to extend these sentences in exceptional cases. Although all of these recommendations, if implemented, offer promising outcomes, they are unlikely to be adopted swiftly, and it would take some time to observe marked reductions in the prison population.<sup>13</sup> Our call to invest in the needs of individuals serving long sentences should be interpreted not as an endorsement of current incarceration trends but as a reminder to remain cognizant of issues that impact the daily lives of individuals who are currently

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13. For a detailed discussion of the political and legal challenges involved in the reassessment of the extensive use of life and otherwise long sentences, see Gottschalk (2014).

incarcerated while these sentencing reforms are under way and until they lead to significant reductions in imprisonment rates.

All evidence suggests that we have gone too far with the use of incarceration in the United States, far beyond the point of effectiveness and human decency. In our view, the dichotomous view of prison as either beneficial or harmful has stagnated our efforts to understand how this environment can be modified to produce positive change and to promote desistance during extended periods of incarceration. In some circumstances and for particular individuals, prison may be inevitable. We need to understand more fully how prison time can be used in a strategic manner to develop an environment that stimulates personal transformation, minimizes the potentially growing individual and collateral harms caused by confinement over time, ensures an improved well-being of prisoners, and maximizes the likelihood of a successful return to the community. It may indeed be true that “with longer periods of incarceration, individuals are likely to become less like they were in the community, less like the people they knew in the community, and more like the prisoners with whom they live” (Wolff and Draine, 2004: 462). In the words of an incarcerated individual, prisoners have lost their “personhood” and their humanity in the process of incarceration, and we often underestimate these individuals’ potential for change. Liebling (2012) asked a question that still requires an answer: Is it possible to develop a prison structure that promotes the desistance process? Other countries have made great progress on this front, but this would require a drastic shift in American punishment philosophy, correctional practices, and political will to change the *status quo*, and not merely a desire to reduce strain on correctional budgets.

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## Reducing Severe Sentences

### The Role of Prison Programming in Sentencing Reform

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Some academics and policy makers have begun to challenge the use of severe sentences not only for nonviolent offenders but also for all offenders (Henry, 2012; National Research Council, 2014; Nellis, 2013; Tonry, 2014; Travis, 2014). The call for an across-the-board reduction in severe sentences reflects several basic truths: Severe sentences are a significant driver of both increased incarceration rates and increases in the absolute number of people in prison. Severe sentences carry staggering financial and social costs with millions of dollars expended on sustaining mass incarceration policies that have decimated poor communities of color. Severe sentences are not always necessary or effective in meeting punishment goals such as retribution, deterrence, or incapacitation; nor are they aligned with human rights, human dignity, or international punishment norms.

Kazemian and Travis (2015, this issue) write extensively about the need to research and to create prison programs that target individuals serving severe sentences, who are collectively referred to throughout this policy essay as “long termers and lifers” (LTLs). Kazemian and Travis make a persuasive case for including LTLs in interventions and prison programming. Through programming, LTLs can experience positive cognitive and behavioral changes, as well as develop important life skills. In addition, LTLs are well suited for prison leadership roles, to mentor and aid fellow prisoners, and to contribute overall to an improved prison climate.

Kazemian and Travis (2015), however, explicitly refrain from proposing prison programs for LTLs as part of a larger push toward the reform of severe sentences. Because Kazemian and Travis believe that systemic reform proposals are “unlikely to be adopted swiftly,” they conceptualize prison programming as an important means of improving prisoners’ lives now and in the immediate future as a matter of “human rights and decency.”

Yet, prison programming can be an essential component of severe sentencing reform. Prison programming that targets LTLs has the important effect of recognizing the dignity

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and humanity of the offender. These ideals have been lost in today's punitive sentencing regime. The impetus for modifying severe sentences will come, at least in part, when policy makers recognize an offenders' intrinsic human value. In addition, LTLs who successfully engage in prison programming may be reformed, and positively transformed, by their prison experience. LTLs' success in prison programming could translate into reduced future public safety risks and could bolster the argument that lengthy or whole-life sentences are rarely, if ever, necessary to satisfy crime control concerns. At minimum, successful completion of prison programming that targets LTLs could foster the argument that all LTLs deserve, perhaps after a specified number of years or after reaching a certain age, a meaningful opportunity to be considered for release.

### Scope of LTLs

With approximately 2.3 million people incarcerated, the U.S. prison population now exceeds that of a small nation-state such as Botswana or Latvia. There has been extensive discussion about the causes and impacts of mass incarceration (Alexander, 2010; Garland, 2001; National Research Council, 2014; Simon, 2007; Wakefield and Uggen, 2010; Western, 2006).<sup>1</sup> The extraordinary rate of incarceration was driven, in large part, by the proliferation of policies throughout the 1990s that increased the use of severe sentences. Political decisions to get "tough on crime" resulted in the passage of three-strikes laws, mandatory minimums, truth-in-sentencing requirements, and habitual offender laws; although normative decisions by criminal justice actors to pursue and mete out increasingly harsh penalties, including life and life without parole (LWOP), sent an actual, symbolic, and sometimes hyperbolic message that crime is being taken seriously (National Research Council, 2014). These policies not only yielded severe sentences in terms of an absolute number of years but also resulted in a general upward ratcheting effect through which all kinds of crimes were and continue to be punished by increasingly severe sanctions. For example, LWOP sentences are not imposed solely for the "worst of the worst" crimes such as the narrow category of first-degree murder, but these sentences encompass a wide range of offenses, including nonviolent property and drug offenses (American Civil Liberties Union [ACLU], 2013). Life sentences, in their various forms (LWOP, virtual life, and life), and long-term sentences are no longer exceptional.

More than 159,000 people are serving some form of life imprisonment, including nearly 50,000 people sentenced to LWOP (Nellis, 2013). Those serving life sentences with the possibility of parole may be released at some point but only after serving an average of 29 years in prison (Mauer, King, and Young, 2004: 12). An additional unknown number of

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1. Although racial disparities are outside the limited scope of this essay, it must be emphasized that the impact of severe sentences has fallen heavily, and disproportionately, on people of color—particularly poor people of color—resulting in long-lasting adverse consequences for individual offenders, families, and entire communities (Alexander, 2010; Nellis, 2013).

people are serving “virtual life” sentences, which are defined as prison sentences that are not technically “life” but that exceed a person’s natural life expectancy (Henry, 2012; Nellis and King, 2009: 2). Although the number of virtual lifers has not yet been quantified, a recent pilot study of 26 states identified 31,000 people in state prison facilities who were serving a sentence of 50+ years; these data are conservative as they do not include states such as California or New York, which tend to have larger prison populations (preliminary data on file with author). In addition, an unknown number of people, likely tens of thousands, are serving “long-term” sentences. Although Kazemian and Travis (2015) were reluctant to define a long-term sentence, they noted that the “minimum number of years set forth in definitions of long-term incarceration has generally increased over time” and ranges from a minimum term of 6 years of imprisonment to a maximum term of 15 years. In sum, the actual scope of the LTL population varies considerably from a conservative estimate of more than 190,000 people to a number substantially higher depending on the complete number of virtual lifers and which definition of long-term sentence is employed.

LTLs are not a monolithic group. From a sentencing perspective, LTLs can be divided into two distinct categories. The first category involves offenders who have the possibility of release at some point during their sentence, which includes offenders serving long-term sentences and offenders serving life with the possibility of parole. The second category of offenders involves those with no real possibility of release, which includes offenders serving LWOP or those sentenced to a term of years that exceeds an offender’s natural life expectancy.<sup>2</sup> Beyond sentence categories, however, LTLs also have different prison experiences as they enter prison at different stages and for various reasons. Some LTLs committed crimes as juveniles, whereas others committed crimes at later periods in their life course; some committed violent offenses, whereas others committed nonviolent offenses; some suffer from mental illness or drug addiction, whereas others are members of street gangs. All LTLs, however, are unified by the reality of a lengthy prison term. Given their distinctive traits, a single program might not effectively address the individualized needs of each offender. It is nonetheless true that almost all LTLs will spend long periods of time in harsh prison conditions, ineligible for prison programs that could ameliorate some of the most traumatic aspects of the prison experience.

### **Prison Programming Affirms the Dignity, Value, and Humanity of LTLs, and Provides an Impetus for Reexamination of Severe Sentences**

Life sentences and long-term sentences deliberately, and sometimes permanently, remove an offender from society. These sentences communicate to the offender, and to the community

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2. All offenders have the theoretical possibility of release through clemency or executive pardon, both of which are rarely employed. It also bears noting that offenders serving life sentences may have the *possibility* of release, but they may never, in fact, be released. Even those lifers who are released spend nearly three decades in prison.

at large, that the offender, through his or her actions, has forever forfeited his or her right to be free. It denies even the smallest possibility of redemption. The offender is forever branded and banished and often will reach old age or die in prison without ever having the opportunity to prove that he or she has been transformed. An offender's crime becomes the entire sum of his or her personhood. In this way, severe sentences negate the offender as a person with intrinsic human dignity and worth. And if an offender has no worth or value, then there is little moral incentive to reconsider punishment policies that result in lengthy incarceration.

Prison programs that target LTLs send a clear and opposite message to policy makers, society, and individual offenders that their personal growth matters, that their humanity is valued, and that their inherent dignity as a person is recognized. Prison programs afford transformational opportunities and allow an offender to be more than just a nameless inmate, waiting only until death to be released. Most severe prison sentences do not recognize the possibility for change and fail to provide even the hope of release that should accompany true transformation. In contrast, other nations believe that prisons have a reformatory function deeply rooted in concepts of human dignity.

Some developed nations throughout the world have determined that life imprisonment in *any* form is not a legitimate punishment. Several European countries, including Germany, France, and Italy, have declared LWOP unconstitutional, whereas Portugal, Norway, and Spain have outlawed any other form of a life sentence. Some South and Central American countries, including Brazil, Costa Rica, Colombia, El Salvador, Peru, and Mexico, do not permit *any* form of life imprisonment because it has been deemed inconsistent with human rights and human dignity (van Zyl Smit, 2006).

Even countries that retain life imprisonment as a potential sentence require review of that sentence after a mandated term of years, allowing for the *possibility* of release. Belgium, for instance, requires a review of life sentences after 10 years, whereas Austria, Germany, Luxemburg, and Switzerland permit review after 15 years. These sentencing policies reflect the internationally held belief that "no human being should be regarded as beyond improvement and therefore should always have the prospect for release" (van Zyl Smit, 2010: 40). Constitutional courts from Germany, France, and Namibia each have recognized that offenders serving life sentences "have a fundamental right to be considered for release" (Appelton and Grover, 2007: 608; see also van Zyl Smit, 2002). The Rome Statute, with signatures from almost 100 nation-states, requires the review of all life sentences after 25 years even for the most egregious types of offenses such as genocide. In contrast, there is no review available for LWOP and often no meaningful review for virtual life and life sentences in certain jurisdictions even for the most petty types of offenses such as shoplifting.<sup>3</sup>

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3. LWOP, by definition, affords no opportunity for release. Many non-LWOP LTLs are serving de facto unreviewable sentences. Appellate courts rarely reverse sentences in noncapital cases, and parole release, even when theoretically available, has become a rare event in many jurisdictions.

Prison programs that target LTLs serve as an important policy statement that this “forgotten” population exists, that the people serving lengthy sentences have value, and that LTLs can both better themselves and contribute to their community in meaningful ways that are described by Kazemian and Travis (2015). The affirmation of an offender’s worth and dignity may prompt policy makers to remember that actual people—not just numbers and statistics describing a correctional phenomenon—are serving thousands of years in the collective often without recourse or redress and often for unnecessarily lengthy periods of time. Prison programs reinforce the idea that LTLs possess inherent human dignity and, importantly, that LTLs retain the possibility for reformation. As such, prison programs for LTLs provide a foundation on which sentencing reform can be built.

### **Prison Programming Can Bolster the Argument that Severe Sentences Are Not Justified by the Goals of Retribution, Deterrence, or Incapacitation, and Can Ensure LTLs Are Prepared for Release If Sentences Were Reduced**

Most of the public and many policy makers continue to embrace the idea—now entrenched in public discourse—that long, severe, and often permanent prison sentences are necessary to ensure public safety and to punish offenders for their crimes. Many fear that offenders serving lengthy sentences will recidivate if released. As Kazemian and Travis (2015) caution, LTLs often are “regarded as less than ideal candidates for intervention programs” because of the “serious offenses that have led to their long sentences.” As a result, LTLs are denied access to programs that could help improve behaviors inside—and outside—the prison gates. This section briefly considers severe sentences in light of the goals of retribution, deterrence, and incapacitation, and it suggests that prison programming can contribute to overall sentencing reform by ensuring the preparedness of LTLs to be released.

In the United States, a popular saying is “you do the crime, you do the time.” This retributivist ideal argues that crime control can be achieved by holding offenders accountable for their actions. Under retributivist theory, punishment is warranted because it is deserved, but it should be no more severe than is necessary to ensure “just deserts” (von Hirsch, 1976). As a corollary, punishment should be closely apportioned to the criminal offense. But the principle of proportionality has been lost in today’s sentencing schema. Thousands of people are serving LWOP and other severe sentences for crimes that include nonviolent and sometimes trivial offenses, such as shoplifting three belts from a department store or siphoning gasoline from a truck (ACLU, 2013), whereas others are serving those same severe sentences for heinous crimes, such as premeditated murder (Mauer et al., 2004). A system that does not distinguish in severity between petty thievery and murder is inconsistent with retribution as it is devoid of fairness and proportionality. Although some offenders might “deserve” to be incapacitated for lengthy terms, their numbers are certainly and significantly less than the existing LTL population.

Deterrence theory also does not support the current reliance on severe sentences. It is the certainty and speed, rather than the severity, of punishment that best deters

offenders (American Law Institute, 2011: 22). As summarized by Kazemian and Travis (2015), “individuals serving long sentences do not seem to pose a distinctive threat to the community when compared with other former prisoners.” Although explanations for these data are mixed, LTLs seem no more prone to violence or repeat offending than other released offenders.

In fact, LTLs may actually be *less* likely to offend than other released populations. In general, recidivism rates decrease with an offender’s age. LTLs, by definition, grow older as they serve their sentences. LTLs may naturally outgrow their criminal behavior by the time they would be eligible for release (Nagin, Cullen, and Jonson, 2008), or simply might be too old or too infirm to engage in criminality (Human Rights Watch, 2012). As set forth in a recent report by the Osborne Association (2014: 5):

Despite the staggering costs of incarcerating the elderly—which far exceed any other correctional population—aging adults in prison have the lowest recidivism rate and pose almost no threat to public safety. Nationwide, 43.3% of released individuals recidivate within three years, while only 7% of those aged 50–64 and 4% of those over 65 are returned to prison for new convictions. . . . Similarly, arrest rates among older adults decline to a mere 2% by age 50 and are close to zero percent by age of 65.

Although it is true that some older offenders might commit new crimes after release, it is also true that most will not. Prison programs that target the specific needs of LTLs could increase the likelihood of desistance after release. This, in turn, would strengthen the argument that LTLs could be subject to shorter sentences without great risk to public safety.

Incapacitation also must be predicated on the likelihood of that future harm. If an offender is unlikely to cause future harm, then continued incapacitation is perhaps unwarranted. As stated previously, age is a strong predictor of desistance, and LTLs often are imprisoned past an age where recidivism is likely. Moreover, the United States spends more than \$16 billion annually on incarceration for individuals 50 years of age or older. On average, it costs twice as much (and sometimes up to five times more) to incarcerate someone 50 and older than it does to incarcerate a younger more able-bodied person (Osborne Association, 2014: 2). The massive financial commitment needed to care for a geriatric prison population seems highly disproportionate to their minimal threat to public safety. In other words, the need to incapacitate people as time passes in their sentences may be far outweighed by the financial cost of retaining them in prison. This, too, supports the argument for reduced sentences.

Michael Tonry (2014) recently called for a policy that would entirely eliminate LWOP or significantly reduce the use of LWOP to only first-degree murders that otherwise would have been punishable by death. Tonry also proposed that all offenders become *eligible* for release after serving 5 years in prison and that all offenders 35 years of age or older be eligible for release after 3 years (Tonry, 2014: 524). Although reasonable people may differ



with the specifics of Tonry's proposal, the idea of creating meaningful release opportunities for most LTLs at some point during their sentence—whether that opportunity is based on age or after a certain number of years served—seems worthy of careful consideration in light of the literature on deterrence, aging, and recidivism.<sup>4</sup> Also, it would, as noted previously, serve to bring the United States into alignment with international norms and would reaffirm our own correctional system's stated commitment to "humanity" and "the dignity of individuals" (National Research Council, 2014: 328).

Although Kazemian and Travis (2015) suggest that prison programs for LTLs would "not necessarily" be centered on recidivism, these programs would likely improve desistance outcomes, although the extent of their impact is an area for continued study. Low recidivism rates will be necessary to convince skeptical policy makers and the public that public safety is attainable even with reductions in severe sentences. To help increase the likelihood that LTLs will not reoffend after release, they will need to participate in programs that specifically address their unique needs. It would make little sense to argue for sentencing reform that results in the release of disaffected, traumatized LTLs who have adopted prison values and norms that are inconsistent with community life (Haney, 2006). In addition, prison programming that targets LTLs could help redress the impact of the prison experience itself. In today's era of mass incarceration, prisons are overcrowded and underresourced, which results in LTLs who are subject to long periods of incarceration in isolating, tedious, rote, and often violent conditions. Effective prison programming would have to account for the severe deprivation experienced by prisoners. Kazemian and Travis correctly recognize that future research is warranted into what kinds of programming might actually ameliorate the stifling and traumatic conditions that now mark much of the modern prison experience.

Kazemian and Travis's (2015) call for additional research and programming for LTLs is a welcome one. As they suggest, effective programs might enable prisoners to develop and even transform during their incarceration. This, in turn, would aid in the argument that severe sentences after a certain point in the incarceration process are unwarranted. So too, successful prison programs might provide the foundation for, and even inform, future policies under which LTLs could become eligible for some form of release given their low risk of future harm as they age, their prospects for reform, their preparedness for release, and the high costs of continued incarceration. In this way, prison programming for LTLs could be a valuable tool in dismantling the carceral state. To the extent that a call for the release—or at least the possibility of release—of LTLs from lengthy prison sentences is ever to gain traction, research and programs such as those envisioned by Kazemian and Travis are a necessary part of an overall, systemic reform that ultimately reduces the reliance on severe sentences.

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4. A meaningful review process would need to be implemented to enable offenders to move from eligibility to actual release with potential reinvestment in parole supervision. This process, perhaps, could include a presumption of release if certain criteria were met.

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## Effects of Life Imprisonment and the Crisis of Prisoner Health

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[O]ne of the dangers when studying criminology is that one can come to view the prisoner as an object rather than a subject, engaging in dispassionate and supposedly neutral analyses of whether human beings suffer “pain,” or indeed are affected in any way, by the experience of imprisonment.

—Andrew Coyle, Foreword to *The Effects of Imprisonment* (Liebling and Maruna, 2005: XX)

The call for an ambitious program of research that focuses on America’s largely forgotten life-sentenced prisoners is an important and welcome one. I applaud the commitment Kazemian and Travis (2015, this issue) have made to this vitally important program of research, especially as it embodies the sentiment in the preceding epigraph. Coyle’s words are an approving “answer” to the rhetorical question on the need for “another” book on prison effects posed by the editors of *The Effects of Imprisonment* in the volume’s introduction (Liebling and Maruna, 2005: 1–22). That is to say, the studies presented in this important volume provide a rich and compelling updated version of Sykes’s classic (2007 [1958]) “the pains of imprisonment” as rearticulated by Johnson and Toch (1982) in an edited volume of the same name. The studies in *The Effects of Imprisonment* (Liebling and Maruna, 2005: 13) “focus on issues such as mental and physical health (including addiction issues), the possibility of post-traumatic stress disorder (PTSD), the developmental health and well-being of prisoner families, and the impact of imprisonment on the ability to successfully desist from crime.”<sup>1</sup>

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1. See Fleury-Steiner and Longazel (2014) for an alternative, Americanist account of the pains of mass imprisonment that focuses less on individual prisoner deprivations and more on ideal types of racialized and gendered penal oppression (e.g., containment, exploitation, coercion, isolation, and brutality).

A major part of this line of inquiry involves a decidedly *prisoner-centered* approach. In this way, the study by Kazemian and Travis (2015) of life-sentenced prisoners in the United States will expand our understanding of prisoners' multiple needs and, in the case of chronically ill lifers, suffering and death. Disturbingly, many prisoners serving life sentences, including those sentenced to life without parole or "death by imprisonment," will likely die in prison. Such a project holds, moreover, the promise to be an important complement to recent biographies by both men and women serving life sentences (George, 2014; Hassine, 2010), as well as to recent empirical case studies of maximum and super-maximum prisons in the United States (Comfort, 2007; Leigey, 2015; McCorkel, 2013; Rhodes, 2004). With a few notable exceptions (e.g., Leigey, 2015), there has been a dearth of recent empirical research on the experience of prisoners doing life in the United States. Given the well-documented coarsening of life behind bars in an age where incapacitation continues to be the chief goal of prisons in the United States (e.g., Fleury-Steiner and Longazel, 2014), systematically documenting the experiences of those under sentences of life imprisonment should be of primary interest to scholars, activists, policy professionals, and indeed, corrections officials.

The following discussion will begin with some particular concerns about the conditions of confinement. Most obviously, the study of lifers' experiences will be contingent on how the prison is organized (e.g., access to educational programs, restrictions on prisoners' mobility, access to prison grievance procedures, etc.). My principle concern with the proposal made by Kazemian and Travis (2015) is its lack of serious attention to the health-care needs of life-sentenced prisoners. Perhaps more than any other issue, a prisoners' physical and mental health is key for attending to the modern effects of life imprisonment:

[T]he prison is a terrible place to cope with a serious ailment. The main reason for this is that the prison system does not have staff or resources to deal with major health problems such as heart disease, AIDS/HIV, hepatitis C, or tuberculosis (TB). Because so many prisoners have histories of risky health behaviors, including intravenous (IV) drug use, HIV/AIDS and hepatitis C infections are rampant in prison. (Irwin and Owen, 2005: 95)

Beyond a secondary issue, prisoner health problems have implications for how conduct is judged behind bars (Fleury-Steiner and Longazel, 2014), relations with prison staff and officials (Calavita and Jenness, 2015), and reentry outcomes (Mallik-Kane and Visher, 2008). One of the chief reasons prisoner health problems may be conflated erroneously with misconduct is that health care behind bars "is provided with an eye to reducing costs and is based upon the military model, which assumes a healthy male" (Irwin and Owen, 2005: 96). At least one empirical study has shown prison staff resistance to assisting older prisoners who struggle to meet even the basic requirements (e.g., bathing oneself) of prison life (Crawley and Sparks, 2005). This essay concludes with a discussion of the impact of health on reentry and the unintended consequences of mass imprisonment on health care

services in the impoverished urban communities to which disproportionate numbers of prisoners return.

### **Variation in the Effects of Life Imprisonment**

Although I will provide reflections on health and ex-prisoner reentry for life-sentenced prisoners (released early after lengthy imprisonment) in a subsequent section of this policy essay, one relevant variable that receives little attention from Kazemian and Travis (2015) is the variation in the effects of life imprisonment *between* institutions. In an acknowledgment at the beginning of their article, Kazemian and Travis thank members of the Network Therapeutic Community Program at the Otisville Correctional Facility (New York). If Kazemian and Travis plan to study lifers and those released from long-term imprisonment from Otisville, it is important they make explicit that this is a *medium*-security prison. Indeed, they may want to consider a comparative analysis of lifers confined in New York maximum-security prison such as Auburn, Bedford Hills, Sing Sing, or Green Haven. Prisoners at Otisville have access to many programs, and levels of deprivation are very likely to be lower when compared with maximum-security prisons in New York. Although Otisville's prisoners may serve life sentences and seem to qualify for the kind of study that Kazemian and Travis have in mind, it is important to observe that most prisoners serving life sentences in the United States are in institutions characterized by extremely harsh conditions of confinement (Fleury-Steiner and Longazel, 2014). Unless I am mistaken, and Kazemian and Travis do plan to conduct comparative analyses of lifers from different institutions, they must clarify that their sample of life-sentenced prisoners from Otisville is by no means a representative group.

Variation in the effects of life imprisonment is also a *within*-prison issue. Although prisoners serving life may be no more or less likely to engage in actual misconduct behind bars (Sorensen, Wrinkle, and Gutierrez, 1998), this does not mean that official records of disciplinary infractions are *actual* instances of misconduct. The case of mentally ill prisoners is a particularly instructive example (Kupers, 1999). Such prisoners may be more likely to have grotesque records of misconduct behind bars. However, the inability of mentally ill prisoners to desist from institutional misconduct is likely a conflict between treatment and control as first recognized by Clemmer (1940) more than seven decades ago. And there remains "no simple and easy resolution for the conflict" (Adams and Ferrandino, 2008: 917). Fellner (2006: 391) cogently captured the perhaps intractable problems of confining mentally prisoners in carceral settings:

The formal and informal rules and codes of conduct in prison reflect staff concerns about security, safety, power, and control. Coordinating the needs of the mentally ill with those rules and goals is nearly impossible.

Perhaps most critical is the lack of adequate resources prisons have to provide adequate treatment to mentally ill prisoners (Clements et al., 2007).

### Prison Health Crisis

Kazemian and Travis (2015) pay far too little attention to the crisis of chronic illnesses behind bars. For the purposes of illustration, consider a recent survey of Otisville prisoners' health-care needs conducted by the Correctional Association of New York. Not only was the institution cited for gross understaffing of nurses and doctors, but also 52% of prisoners rated the quality of physician's care as "poor" (Correctional Association of New York, 2011: 16). When considering the reentry needs of ex-prisoners, criminologists rarely consider medical histories as much as they do criminal histories even though the two intersect in important ways (Clear, 2007). Given that life-sentenced prisoners may be released early to impoverished communities that lack access to sufficient health care, it is not surprising that their illnesses only worsen behind bars (Greifinger, Bick, and Goldenson, 2007). With the prison's emphasis on discipline and strictly regulated eligibility for work and educational programs—typically, available only to healthy prisoners—lifers with serious illnesses are more likely to be isolated in understaffed and often dangerously ineffective prison health wards. The isolation of long-term imprisonment also may result in psychological pains that take place behind bars but not in formal prison health settings. Indeed, prisoners may go years without seeing a physician (Fleury-Steiner, 2008), and others in protracted solitary confinement may suffer numerous psychological problems (Haney, 2003).

Most prisoners begin their sentences without adequate health care. Because a disproportionate majority lack sufficient health benefits and access to treatment (Lara-Millán, 2014), they also are more likely to enter prison with untreated, often chronic illnesses. During the many years spent behind bars, especially as prisoners grow older, prisoners may experience multiple chronic illnesses that require comprehensive health care (Aday, 2003). Yet there is a strong probability that elderly prisoners will not receive adequate care (Human Rights Watch, 2012). The failure to meet a prisoner's basic rights to human dignity is often a function of grossly underresourced and disorganized prison health contractors. Hired by the state to cut costs, for-profit prison health providers have notoriously shabby records (Fleury-Steiner, 2008). In the context of everyday prison life, health problems can have a debilitating impact on all prisoners, including, if not especially, life-sentenced prisoners. In many institutions, missing a job or a class because of illness or injury could mean the loss of certain privileges. A prisoner who dares to challenge his or her health needs via the prison's grievance system may suffer retaliation from prison officials. Indeed, a recent study of California's grievance system demonstrates that many prisoners are reluctant to file a claim out of fear of retaliation:

Potential barriers to filing go well beyond subjective factors such as self-blame, stigma, and the related concern about "trouble." A central aspect of the trouble these men spoke of was retaliation by officials against prisoners who file grievances. More than 61 percent of inmates raised the issue of retaliation



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in their interviews, sometimes in response to a question and sometimes impromptu. (Calavita and Jenness, 2015: 68)

Chronically ill prisoners often are left in the untrained hands of corrections officers. Although some corrections officers do engage in heroic attempts at care for chronically ill prisoners, including those dying from AIDS (Fleury-Steiner, 2008), many officers are indifferent to prisoner health-care needs (*Brown v. Plata*, 2011). Consider the case of Marciano Plata (Prison Law Office, n.d.). After tearing his meniscus when he fell while working in the kitchen at Salinas Valley State Prison (California), Plata spent hours on the floor writhing in pain. When finally examined by a physician, Plata took only a low dose of over-the-counter medication. Still suffering after two weeks of bed rest, he finally managed to hobble to the prison health clinic where a medical technician denied him entry telling him, “there’s nothing we can do.”<sup>2</sup> It is clear that the needs of a patient can be trumped by the prisoner label and the behavioral control imperatives of the prison more broadly:

Where humanistic health practice requires an acknowledgment of interconnectedness, prisons are based on principles of exclusion, separation, and confinement. Where physicians and nurses provide care and comfort to those in pain and those who are disabled, a prison system demands discipline and the stripping of identity, possessions, affection, and touch. And where medicine attempts to provide cure and management of disease, the primary goal of 21st century corrections (despite the implications of training and rehabilitation in the word “correction”) is typically detention and punishment. (Stoller, 2003: 2265)

The tension between prisoner health and prison discipline may be especially evident in the lives of elderly long-term prisoners. Although some states have created separate institutions for such populations, there is reason to believe that life inside the prison walls is a harsh and unforgiving existence for aging prisoners (Aday, 2003). Although elderly lifers may show maturity and the ability to negotiate prison rules, it is incumbent on criminologists who study this population to explore their health needs, the quality of care they have received, and how their health needs impacted relations with fellow prisoners and corrections officers and administrators. An empirical study showed how corrections officers may be especially reluctant to address the basic needs of elderly prisoners. Consider the following exchange from the study by Crawley and Sparks (2005: 365):

Interviewer: How much caring work would you be prepared to do with prisoners? Things like washing them, helping them to get dressed, things like that?

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2. The information concerning Plata’s injuries comes from the Prison Law Office’s (n.d.) original complaint in the *Brown v. Plata* case, which also named Otis Shaw and other prisoner plaintiffs.

Prison Officer: Well we are in no way nurses and we are in no way carers. We have a duty of care but we are not . . . I mean there's no way I'm going to do stuff like washing prisoners. We make sure there's clean sheets available, things like that, but if they need, say, nappies for incontinence things, that's health care. We try and keep a nice dividing line.

This so-called “dividing line” points to a relationship older prisoners have with corrections officers that may be fraught with shame, fear, and anger that could create especially difficult relations with those that hold ultimate power over the quality of their lives behind bars.

### **Health, Prisoner Reentry, and Spillover Effects**

In the case of prisoners released after long-term imprisonment, attending to health-care needs is important for understanding the challenges to reentry. Mallik-Kane and Visser's (2008) report *Health and Prisoner Reentry: How Physical, Mental, and Substance Abuse Conditions Shape the Process of Reintegration* provides crucial insight. By focusing on a representative sample of 1,100 former mostly short-term prisoners, they find that nearly all suffered from chronic illnesses that required care. Many ex-prisoners had multiple medical needs. Although most participants in their study received some health care while imprisoned, “their rates of treatment for specific health conditions deteriorated, suggesting that they received episodic care for acute problems but that continuous treatment of specific health conditions suffered” (Mallik-Kane and Visser, 2008: 3). This finding of an inconsistent approach to prisoner health echoes the findings of my own research involving HIV-infected prisoners (Fleury-Steiner, 2008). Although my research touched only briefly on the challenges ex-prisoners with serious health care needs face after release, Mallik-Kane and Visser's (2008: 8) research illuminated how health problems create serious challenges to successful prisoner reentry:

Returning prisoners with health problems may be unable to engage in work or other activities because of pain or sickness, and their families may be unwilling or unable to serve as a fallback support. They are additionally confronted with the tasks of managing their health problems, such as accessing health care and keeping up with medications or appointments. Those with severe or unmanaged health problems face an increased risk of adverse outcomes, including physical illness, relapse into drug use or, particularly in the case of mental illness, inappropriate behavior that provokes a police response. It stands to reason that successful treatment of returning prisoners' health conditions could increase their chances of reentry success by improving their ability to work, support themselves, and abstain from substance use, all of which have been shown to contribute to desistance from criminal activity.

In the case of life-sentenced prisoners released early, such challenges are likely to be exacerbated. Although some may be able to return to robust social networks after release, it is likely that most will confront serious health-related problems in the absence of sustained care that will make staying out of prison much more difficult.

Exploring health and reentry beyond individual prisoners is of obvious importance. Several recent empirical studies documented how negative health care outcomes extended to ex-prisoners' families and had aggregate negative impacts on the surrounding community (Patterson, 2010; Schnittker, Massoglia, and Uggen, 2011; Wildeman, 2011). Recent research, moreover, shed important light on the unintended spillover effects of mass incarceration on diminished access to public health care (e.g., emergency room care) in a large poor urban poor community of color (Lara-Millán, 2014). Most consequentially, this research showed how poor urban residents of color who had *no* prior formal contacts with the criminal justice system were nevertheless stigmatized by emergency room health-care workers as criminal and routinely denied care. Lara-Millán's (2014: 882) deep ethnography of an emergency room in a large impoverished urban community and analysis of more than 1,000 admissions decisions of ER nurses found that:

When the urban poor seek care in the public ER, regardless of their criminal status, they will find fewer beds available to them and simultaneously face delays, policing, and deterrence from accessing health care. Moreover, their medical diagnosis and chances of gaining admission will, in part, be shaped by crime control language that is pervasive among health care workers.

This research reinforced the point that health-care inequity is a problem that can create challenges beyond ex-prisoners and extend to their families and neighbors. The crisis of health care in impoverished urban communities and its connection to mass imprisonment presented in Lara-Millán's (2014) research shows vividly how the imperative to cut costs through early release could backfire in a society where the fight over affordable health care for the uninsured looms large.

### **Concluding Remarks**

Taking seriously Kazemian and Travis's (2015) concluding reflections on how prisons can be transformed to prepare prisoners for successful reentry is of obvious importance. There is no doubt that "maximum effort should be made to encourage ties with the family and community throughout the prisoner's stay, and prerelease programs should focus on actively connecting the prisoner to the host community" (Petersilia, 2003: 245). The debilitating effects of prison on many prisoners' health and well-being must also be a cornerstone of such initiatives, or in many cases, they will ultimately fail. This is not to end on a pessimistic note. The time may be right for radical penal reforms (Simon, 2014). U.S. criminologists would do well to consider seriously Liebling's (2011: 546) call for empirically rigorous

research that is attentive to prison policy reform as a shared struggle for the human rights of prisoners:

The pains of imprisonment may vary by institution, jurisdiction and culture, and historical period, but some “essential features” of imprisonment and generalized responses to those features also exist. Carefully collected empirical detail on these matters, within an evolving moral and conceptual framework, and extensive dialogue between prisoners and staff, social researchers, official and oversight bodies, and activist and campaigning organizations, is essential.

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## Target Suitability and Terrorism Events at Places

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The study of terrorism in criminology has expanded dramatically in the past decade, paralleling the increase in both government funds for research and counterterrorism policies (DeFlem, 2004; LaFree and Freilich, 2012; Lum, Kennedy, and Sherley, 2006; Tracy, 2012). Many scholars have observed that the early research on the correlates and causes of terrorism lacked both theoretical guidance and empirical analysis (Lum et al., 2006; Schmid and Jongman, 1988; Silke, 2001). Arguably, the field has made much progress conducting theoretically driven, empirically based research as a means of informing counterterrorism policies. Data sets on terrorism activity have become increasingly available and accessible, and several researchers have applied criminological theories, such as deterrence and rational choice (Clarke and Newman, 2006; Dugan, LaFree, and Piquero, 2005), to explain terrorism attacks.

More recently, LaFree and Bersani in the August 2014 issue of *CPP* applied arguments from social disorganization theory and used data from the Global Terrorism Database (GTD) to examine the effects of county-level structural traits on annual counts of terrorism incidents that occurred between 1990 and 2011 for all counties in the United States. Their results indicated that high levels of ethnic and language heterogeneity, residential instability, and urbanization are related to higher levels of terrorism, whereas county-level concentrated disadvantage is negatively related to terrorism activity. They suggested that counterterrorism policies implement law-enforcement-based strategies that encompass elements from community-based policing (LaFree and Bersani, 2014; Pelfrey, 2014) and tactics from problem-oriented policing (Wormeli, 2014).

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This essay is written in response to the section *Correlates of Terrorist Attacks in the United States in Criminology & Public Policy*. 2014.13.3: 451-97.

LaFree and Bersani's (2014) examination of terrorism events at the county level is undoubtedly an important contribution because substantial research in criminology has indicated the importance of policing places for crime prevention (Braga and Weisburd, 2010), and many have suggested this approach would be useful for counterterrorism strategies and tactics as well. Recent studies on terrorism at places have examined the distribution and correlates of terrorist events across units of space, such as countries, provinces, and more recently, counties. Interestingly, few studies within criminology have examined the distribution and correlates of being a target of a terrorist event. Situational crime prevention and routine activities theory are both well suited for guiding an empirical examination of terrorist targets in the United States, and they could potentially yield useful policy implications for local-level counterterrorism efforts.

In this special essay, I suggest that a focus on the nature, distribution, and correlates of being targeted by terrorists can complement existing approaches that examine the geographic distribution of events across places. Additionally, as outlined by several other scholars (Clarke and Newman, 2006), targets of terrorist events can be identified by a variety of situational and environmental characteristics. Examining target attractiveness—by focusing on the environmental and situational features of targets—can result in both specific and general target-hardening tactics within large areas such as counties, and it could provide a useful starting point for information gathering/surveillance and other intelligence-led policing counterterrorism efforts.

### **Targets of Terrorist Events and Situational Crime Prevention**

Although crime and terrorism certainly share many etiologic features (LaFree and Dugan, 2004; Rosenfeld, 2003, 2004), terrorism may also be different than more traditional forms of criminal activity because terrorists seek to engage in attacks that garner widespread public media attention (Hoffman, 1998; Jenkins, 1975; LaFree and Dugan, 2004). Thus, the types of targets most likely to accomplish such a goal are those attacks at entities that have the potential to inflict maximum damage, both tangible (mass casualties) and intangible (heightened fear), or those entities that hold symbolic or ideological relevance to the terrorist group (Asal et al., 2009; Clarke and Newman, 2006; Crenshaw, 1998; Drake, 1998; Hoffman, 1998). Asal et al. (2009: 262) stated that, “the terrorist organization must first decide what their goals are for an attack and then decides which target to select.” Targets reflect the “enemy” to the terrorist group and are viewed as “legitimate targets” (Drake, 1998: 56; Hoffman, 2006). Numerous factors could underlie decisions to attack potential targets, including ideology of the group, availability of group resources, number of opportunities, and calculation of expected costs and rewards involved with attacking the target (Asal et al., 2009; Cauley and Im, 1988; Drake, 1998; Sandler and Lapan, 1988).

Prior research on targets has largely examined trends and changes in types of terrorist targets, reporting a general decline in hard targets and an increase in soft targets over time (Sandler, 2014). The movement from hard targets (military and government officials) to soft



targets (businesses, private parties, civilians, and nongovernment buildings) occurred for both transnational and domestic terrorism events, although the trend was more pronounced for domestic terrorism (Brandt and Sandler, 2010; Gailbulloev, Sandler, and Santifort, 2012; Sandler, 2014). Similarly, Brandt and Sandler (2010) reported that, after the early 1990s, private parties—rather than property—were more frequent soft targets. Less studied are the situational and environmental features of the target and the correlates of various target types. Clarke and Newman (2006) suggested applying a situational crime prevention approach to counterterrorism efforts that focuses on assessing features of potential targets that increase their vulnerability and harm, as well as environmental or situational traits factors that enhance target attractiveness. Their approach is compatible with other scholars who have argued that terrorist target selection is driven by the decision-making process (Asal et al., 2009; Crenshaw, 1998; Hoffman, 2006). The situational crime prevention approach assumes that individuals make a series of choices related to the location of the event, one of which includes assessing the attractiveness and vulnerability of the target. Clark and Newman (2006) suggested several target features, ranging from features of the environment in which the target is located in, to the inherent features of the target such as the extent to which it is iconic. For example, certain entities will have a higher risk of being a target of terrorism if they are more visible and if entities are embedded in structures or areas in which large groups of people can be gathered. Situational crime prevention's approach to assessing target attractiveness and suitability is very much in line with a study of terrorist events at places and may bolster existing counterterrorism efforts that focus on geographic areas, such as counties or boroughs. There are many plausible reasons for why terrorist events may be higher in urban areas or areas of high ethnic heterogeneity. However, perhaps an explanation that points to the characteristics of the event or the place of the event, rather than to the characteristics of the individuals residing in that area, is also equally plausible. The environmental surrounding and features of the target that increase the attractiveness for a terrorist attack may be associated with the structural traits of the larger surrounding geographic area, such as counties, cities, or communities.

Urban areas and counties with high levels of ethnic heterogeneity can be attractive targets for terrorism attacks because they are located in close proximity to symbolic targets that represent particular social, religious, or government agencies. Urban areas may be ideal because of the abundance of soft targets; thus, there is greater potential for mass casualties because these areas are associated with more people or a greater number of people in smaller spaces. Santifort, Sandler, and Brandt (2013:26) stated that the most at-risk areas are public spaces such as “market squares, public transit, shopping malls” and other areas of public gatherings that are likely located in close proximity to urban areas (Santifort et al., 2013). Focusing research attention on the environmental and situational characteristics of public spaces that are targets of terrorist attacks can shed light on the process in which terrorists select targets and guide local-level counterterrorism efforts toward those specific areas in which attacks are likely to occur.

*Terrorism Targets at Places*

LaFree and Bersani (2014) found that terrorism is highly concentrated in a few counties. This finding is in line with results from other studies examining the geographic distribution of crime and terrorism at other levels of aggregation (Weisburd, Bushway, Lum, and Yang, 2004). Of the 3,144 counties in the analysis, approximately 10% experienced at least one terrorist event and 5 counties accounted for 16% of the 597 total recorded terrorist acts. The finding that counties characterized by high language diversity, residential instability, and close proximity to urban areas are more likely to experience a high concentration of terrorist incidents reasonably leads to the conclusion that characteristics of places are important for predicting variations in terrorist events. Although we know what counties are at high risk for experiencing a terrorist attack, which is useful for all the reasons articulated by Pelfrey (2014) and Wormeli (2014), we know less about the environmental and situational features of the entities targeted within these counties.

One main reason for the ambiguity is inherent in the geographic unit of analysis. Although the “crime at micro-places” literature has indicated that directed, targeted police presence at block groups or street segments can result in significant declines in criminal activity (Braga and Weisburd, 2010), counties are substantially larger geographic areas. Determining where to allocate resources within a large geographic space becomes problematic when resources are limited. McGarrell, Freilich, and Chermak (2008: 153) noted that, within the United States, counterterrorism efforts represent “an unfunded mandate for local law enforcement.” Assuming a limited or finite amount of law-enforcement-based resources, the distribution of resources within larger areas of space becomes less targeted compared with focusing on smaller areas of geography such as communities, block groups, or street segments. The 3,144 counties in the analysis vary in their geographic size and total population, as well as in their population density. Identifying where to distribute resources within counties becomes of ultimate practical importance as complete county-level coverage is likely not feasible.

One way to direct resources efficiently to specific areas within counties may be to, as LaFree and Bersani (2014) and Wormeli (2014) suggested, examine terrorist events at smaller levels of geographic aggregation, such as communities, blocks, or street segments. This approach would require a different theoretical framework and should focus on identifying the environmental features of micro-places that result in suitable targets for terrorist events. Alternatively, focusing on the type and features of targets attacked within counties can also help direct resources to other specific areas and targets that are at highest risk. This does not discount the importance of county-level correlates of terrorist events for counterterrorism efforts. Indeed, evidence indicates that a nontrivial number of terrorists lived and engaged in preplanning within close proximity (30-mile radius) to the target (Cothren, Smith, Roberts, and Damphousse, 2008; McGarrell et al., 2007). The close spatial proximity between areas in which the planning of terrorist events takes place and the ultimate target of the terrorist

attack seems sufficient cause for an increased focus on the distribution and nature of terrorist targets. Thus, examining the nature of terrorist targets and the surrounding environmental and situational characteristics of the targets might facilitate problem-solving-oriented tactics such as target hardening, but it can also provide the starting point for determining the radius in which to engage in community-policing-based tactics that center on information gathering and other intelligence-led policing strategies (McGarrell et al., 2007).

### *Potential Problems of Focusing on Terrorism Targets*

Just as potential problems, unintended consequences, and ethical issues are associated with directing law-enforcement-based resources to counties characterized by high ethnic heterogeneity, there are issues associated with the assessment of target risk and the subsequent design and implementation of target-hardening counterterrorism tactics. Moreover, the existing research in the terrorism literature has indicated substantial changes over time in the nature of terrorist targets. This highlights the dynamic interplay between target hardening and terrorist responses to target hardening. Many have suggested that target hardening can lead to displacement or transference of terrorist targets (Brandt and Sandler, 2010; Cauley and Im, 1988; Sandler, 2014).

Specific target-hardening tactics, such as the implementation of metal detectors, have substantially impacted attacks targeted at airlines (Dugan et al., 2005). However, although there was a decline in skyjackings and thus airlines were less likely to be targets of terrorist's attacks, there were increases in other types of attacks and substitutions of targets (Cauley and Im, 2015). Similarly, Enders and Sandler (1993) reported an increase in kidnappings and lethal terrorist incidents after metal detector installation. This finding does not necessarily suggest that we should abandon target-hardening efforts. In particular, it is important to note that the existing evidence examining displacement and transference in terrorism has done so indirectly, and it has not examined whether target-hardening tactics have led to more attacks directed at similar targets in the surrounding geographic areas. A meta-analytic review of crime displacement studies concluded that displacement is a possibility; however, most evidence indicates displacement effects to be minimal and there could be greater diffusion of benefits to adjacent areas (Bowers, Johnson, Guerette, Summers, and Poynton, 2011; Weisburd et al., 2006). The possibility of substitution to other types of targets also suggests that strategies should continue focusing on specific hardening techniques given the target, as well as general techniques that could apply to all targets.

Specific target-hardening tactics are tailored to specific targets, such as the implementation of metal detectors and enhanced airport security as a means of preventing hijackings of airplanes. General target-hardening strategies are those tactics that are designed for all targets that share key environmental and situational features that increase the probability of risk of being an attractive terrorist target. Based on existing conceptualizations of the process underlying terrorist selection of targets, these can include areas that encourage mass public gatherings, have high visibility, and offer the potential for mass destruction and

casualties. Previously employed tactics include electronic surveillance, increased monitoring of public spaces, and enhanced security during situational contexts that facilitate mass public gatherings. But there are many areas such as these located throughout communities, cities, and counties, and many researchers are in agreement that resources cannot be directed at all potential targets (Apostolakis and Lemon, 2005; McGarrell et al., 2007; Taylor, Krings, and Alves-Foss, 2002; Willis, 2007). Less agreed on are the tools and methods used to determine the level of terrorist attack risk and subsequent resource allocation (Willis, 2007).

Willis (2007:598) stated that many problems of counterterrorism resource allocation have centered on three issues: the criterion guiding resource allocation (based on risk or risk reduction), assessing and estimating risk, and determining “tolerable levels of terrorism risk.” Willis (2007) argued that resource allocation should be based in terrorism risk, defined as a function of threat, vulnerability, and consequences of attacks. Risk represents the expected consequences of the attack given the attack occurred and is successful. Threat refers to the probability that a specific target experiences an attack during a specified time period and vulnerability is the probability that damages and harm result from a specific attack on a target. Finally, consequences refer to the expected magnitude of damage and harm resulting from the attack. Willis stated that this approach for defining terrorism risk has two advantages. First, it allows a comparison of specific risks across different targets (e.g., risk of injury from bombings for two different types of targets). Second, Willis argued this approach will facilitate more tailored approaches to reducing or managing terrorist risk. In particular, increasing surveillance, intelligence-gathering efforts, and target-hardening tactics should reduce the vulnerability of a target if it reduces risk of successful, completed attacks. Additionally, focusing on emergency preparedness and responses post-attack should reduce the impact or damage of the attack (Willis, 2007).

Problems with counterterrorism resource allocation are also associated with determining “tolerable levels of terrorism risk.” Tolerable risks refer to those risks that are tolerated or accepted because the risk is relatively small compared with the benefits of an attack or because addressing the risk through counterterrorism measures may result in greater risks (Willis, 2007). Determining tolerable risks is linked to assessment and estimation of an entity’s risk of being a terrorism target. There are several scenario- and decision-based simulation modeling approaches for assessing and prioritizing a target’s risk of attack, such as the Probabilistic Terrorism Model, Risk Analysis and Probabilistic Survivability Assessment, and Probabilistic Risk Assessment (Risk Management Solutions, Newark, CA; Apostolakis and Lemon, 2005; Taylor et al., 2002; Taylor, Oman, and Krings, 2003; Willis, 2007; Willis and LaTourrette, 2008). All have been used to assess the risk and vulnerability of various critical infrastructures such as large universities and electric power industries (Apostolakis and Lemon, 2005; Taylor et al., 2002, 2003).

Another issue related to problems of resource-allocation-based assessment of terrorist threat is determining what agency—federal, state, or local—should be tasked with the assessment and prioritization of terrorist risk across targets, as well as with subsequent

target-hardening efforts. The Urban Area Security Initiative, sponsored by the Department of Homeland Security, is a grant program designed to provide and allocate financial assistance to large metropolitan areas throughout the United States. The main purpose is to enhance regional-level collaboration and counterterrorism tactics, as well as general terrorism preparedness and responses (Willis, 2007). Willis applied the Probabilistic Terrorism Model to assess the risks of terrorist attacks at targets in 47 large, urban areas. This approach estimates the threat of different types of attack modes (tactic) and targets by using a set of varying decision-making criterion such as target selection by terrorists and resources and capabilities for different attack modes (Willis, 2007; Willis et al., 2005). Using this approach, Willis found that risk is also highly concentrated among urban areas, with six cities accounting for a substantial amount of terrorism risk. The Willis (2007) study highlighted existing counterterrorism policies at larger levels of aggregation that could also be compatible with approaches that focus on the environmental and situational features of targets and subsequent target-hardening strategies and tactics. It is also likely that regional and local-level law enforcement will play a critical role in the assessment, information sharing, and monitoring of potential terrorism targets as several scholars have noted the increased demand for law enforcement to be involved in counterterrorism and homeland security efforts (McGarrell et al., 2007; Roberts, Roberts, and Liedka, 2012; Schafer, Burruss, and Giblin, 2009).

A final potential unintended consequence and ethical concern of focusing research and policy on terrorism targets concerns public perceptions of trust and legitimacy in the government and related agencies. Both Pelfrey (2014) and Wormeli (2014) have highlighted the potential issues with information sharing, monitoring, and surveillance in areas of high ethnic heterogeneity. Wormeli also warned that increased electronic monitoring and surveillance, post-Snowden incident, could exacerbate public mistrust in government and lead to public objections about further infractions (perceived or real) of civil liberties. This is certainly a possibility; however, one way of offsetting such a scenario might be to direct and limit increased electronic surveillance to public spaces or critical infrastructures that facilitate the targeting of soft targets.

## **Conclusion**

The study of terrorism targets is not a novel suggestion, and in fact many other scholars have suggested this before. Similarly, the suggestion to apply principles from situational crime prevention theory to counterterrorism efforts, especially those related to target attractiveness, is also not new. Nonetheless, few studies have examined the environmental and situational features surrounding targets that make them more attractive and suitable for terrorist attacks. This strategy is compatible with situational crime prevention theory and complements counterterrorism strategies and tactics directed at larger geographic places. Both approaches require incident-level data sets, such as the GTD, and can explain the distribution of both targets and events more generally across geographic areas. Finally, focusing on targets within

these larger areas of geography could guide and funnel limited target-hardening resources, as well as intelligence-based and information-sharing tactics, to the specific areas and targets that are most attractive and vulnerable.

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