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Programming in restrictive Housing

Considerations for Improving Outcome Evaluations

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A number of studies have identified “what works” in regard to the successful implementation of correctional programming over the past several decades. Few studies, however, have examined the complexities associated with programming in restrictive housing. Using data from a Midwestern department of corrections, we examined whether the provision of programming in restrictive housing achieved desired outcomes (e.g., reductions in inmate misconduct). The findings revealed the amount of time served in restrictive housing and confinement in different types of restrictive housing may influence estimations of a treatment effect. As a growing number of states seek to reform the use of restrictive housing, the proper implementation of cognitive-behavioral programming may increase institutional security and safety.

Keywords: restrictive housing; prisons; inmate misconduct; programming

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Introduction

Restrictive housing, also referred to as solitary confinement, removes inmates from the general population of a facility for the purposes of punishment (e.g., disciplinary segregation), protection (e.g., protective custody), and the safety and security of the institution (e.g., administrative segregation; Frost & Monteiro, 2016; Labrecque & Smith, 2013). Confinement in restrictive housing typically entails a 23-hr-a-

day lockdown in specialized cells with limited access to amenities, programming, and social interactions (Beck, 2015; Browne, Cambier, & Agha, 2011; Butler, Griffin, & Johnson, 2013; Foster, 2016; Haney, Weill, Bakhshay, & Lockett, 2016; U.S. Department of Justice, 2016). These particular conditions of confinement have led to increased concerns from international groups and agencies about inmate well-being from the United Nations, Canada, United States, and inmate rights groups (American Civil Liberties Union, 2014; Amnesty International, 2014; *British Columbia Civil Liberties Association v. Canada [Attorney General]*, 2018; Lowen & Isaacs, 2012; Riveland, 1999; United Nations, 2016; U.S. Senate Subcommittee on the Constitution, Civil Rights, and Human Rights, 2014). One of the primary concerns voiced by these organizations includes the need for rehabilitative programming (U.S. Department of Justice, 2016).

Few evaluations examine the effectiveness of rehabilitative programming in restrictive housing (Mears, 2013; Smith, 2016). This scant evidence makes it difficult to determine “best practices” regarding the design or evaluation of programs for inmates in restrictive housing (Smith, 2016). In this study, we conduct an outcome evaluation to determine whether participation in a treatment program designed for restrictive housing reduces mis- conduct (e.g., rule violations). We also account for several factors that may influence estimations of a treatment effect, such as the treatment dosage that is provided to offenders. Incarcerated offenders who serve less time in restrictive housing may be unable to complete programs that are lengthy and intensive. Similarly, the provision of treatment services may differ across types of restrictive housing settings (e.g., disciplinary and administrative segregation). Inmates who serve time in disciplinary segregation typically serve fewer days in restrictive housing than inmates in administrative segregation (Browne et al., 2011). Finally, researchers need to account for time served in restrictive housing during outcome evaluations, which may affect estimations of a treatment effect. Taken together, our findings contribute to the literature surrounding programming in restrictive housing that also provides recommendations for future policy and practice. The following sections of this article describe effective correctional programming, detail correlates of misconduct in restrictive housing, and describe the program that is the focus of this outcome

evaluation.

Correctional Program Effectiveness

A growing body of research has identified correctional treatment practices that can lead to reductions in reoffending (Andrews et al., 1990; Cullen & Gendreau, 2001; Duwe & Clark, 2015; Gendreau, Goggin, French, & Smith, 2006; Lowenkamp & Latessa, 2005; Mackenzie, 2000; Wilson, Bouffard, & Mackenzie, 2005). The success of these practices is due in part to a greater understanding of “what works” in correctional treatment (Andrews et al., 1990; Dowden & Andrews, 2000). Most successful rehabilitative programs target high-risk offenders who have attitudes or behaviors that increase the propensity for crime (e.g., reoffending), while also ensuring participants are responsive to the goals of the treatment (Andrews & Bonta, 2010; Andrews et al., 1990; Lowenkamp & Latessa, 2005). These principles of effective intervention posit that treatment decisions should be based on the risk, needs, and responsivity of offenders (Andrews & Dowden, 2007; Andrews et al., 1990). Offenders who are deemed high risk or likely to reoffend tend to have greater criminogenic needs (e.g., substance abuse, antisocial attitudes) that are amenable to change (Andrews et al., 1990; French & Gendreau, 2006; Lowenkamp, Latessa, & Holsinger, 2006; Smith, Gendreau, & Swartz, 2009). The needs principle posits that a variety of factors (e.g., biological, interpersonal, situational) influence the likelihood of offending (Andrews & Bonta, 2010; Smith, 2016). Some factors are static and do not change (e.g., age at first arrest), but other factors are dynamic (e.g., substance abuse, antisocial attitudes) and are amenable to change (Gendreau, Little, & Goggin, 1996; Smith et al., 2009). Finally, the responsivity principle includes efforts to maximize participants’ ability to learn and absorb the core tenants of a treatment program (specific responsivity) through cognitive-behavioral or social-learning strategies (general responsivity; Andrews, Bonta, & Wormith, 2006; Andrews & Dowden, 2007; Gendreau & Smith, 2011). Together, the principles of effective intervention may provide insight into the correlates that influence program participation and reoffending.

Correlates of Misconduct and Program Participation in

Restrictive Housing

Evaluations of programming in restrictive housing require the identification of correlates that are related to the outcomes of interest (e.g., misconducts) in addition to correlates associated with program participation. The principles of effective intervention provide a framework to understand how inmates in restrictive housing, who have increased risks to engage in misconduct, may be ideal participants for treatment in correctional settings. The following paragraphs describe several correlates that are related to program participation and misconduct.

Researchers have used nationally representative data to identify the characteristics of individuals who serve time in restrictive housing. This research revealed that offenders in restrictive housing who were younger, lacked a high school diploma, were incarcerated for a violent offense, had extensive criminal histories, and were involved in assaults against other inmates or staff were more likely to serve time in restrictive housing (Beck, 2015; Butler & Steiner, 2017). These findings suggest that incarcerated persons who have extensive criminal histories, mental health problems, or serious commitment offenses are suitable program participants because these inmates are more likely to violate institutional rules and be confined in restrictive housing (Gendreau, Goggin, & Smith, 1999; Nesovic, 2003; Steiner, Butler, & Ellison, 2014). Inmates who have a mental illness may have greater difficulties following institutional rules (Walters & Crawford, 2014; Wood & Buttaro, 2013). In addition, persons who have served time in restrictive housing may also have greater mental health needs (e.g., mental illness leads to a violation of institutional rules that may lead to placement in restrictive housing; Beck, 2015; Butler, Johnson, & Griffin, 2014; Haney, 2003; Kapoor & Trestman, 2016). Individuals convicted of violent offenses may have a greater likelihood of being placed in restrictive housing (Andrews et al., 2006; Beck, 2015). Together, these findings reveal that offenders confined in restrictive housing have risk factors that increase the odds of misconduct and postrelease reoffending (Butler, Steiner, Makarios, & Travis, 2017; Smith, 2016).

Inmates in restrictive housing may also have greater criminogenic needs that influence the likelihood of engaging in misconduct (Beck, 2015). Many of these criminogenic needs are amenable to change such as the ability to manage and cope

with frustrations that may lead to assaults against other inmates or staff. In addition, incarcerated individuals' accessibility to family or friends and gang membership may be criminogenic needs that are associated with misconduct (Gendreau, Goggin, & Law, 1997; Goodstein & Wright, 1989; Schenk & Fremouw, 2012; Steiner et al., 2014; Wooldredge, 1991). A growing body of research has identified the importance of inmate visitations with family and friends (Cochran, 2012; Siennick, Mears, & Bales, 2013). Inmates who receive visits from family and friends may have lower odds of misconduct because these prosocial bonds may help inmates cope with imprisonment and provide incentives to comply with institutional rules and regulations (Cochran, 2012; Tasca, Mulvey, & Rodriguez, 2016). Considerable research has also examined the role gang membership may have on inmate misconduct, which generally finds gang members are more likely to engage in misconduct (Gaes, Wallace, Gilman, Klein-Saffran, & Suppa, 2002; Griffin & Hepburn, 2006; Worrall & Morris, 2012). Membership in gangs typically leads to maladaptive responses to handling disputes and problems in prison, which may lead to subsequent placement in restrictive housing (Butler et al., 2013; Gaes et al., 2002; Pyrooz, 2016). These criminogenic needs necessitate the use of programs that can be administered to restrictive housing populations.

Inmates who enter restrictive housing may also have unique challenges that inhibit responsiveness to treatment (Smith, 2016). Challenges associated with the specific responsiveness principle in restrictive housing include mental health needs, educational deficits, and extended time served in prison and restrictive housing. Inmates who have mental health problems may experience cognitive difficulties progressing through a program and these individuals may require additional assistance. Furthermore, inmates' functional reading level may be affected by their educational attainment (Andrews et al., 1990). Inmates who lack the ability to read and comprehend program materials may have difficulty progressing through a program. Time served in prison and restrictive housing may also influence a person's willingness to participate and complete a program. Individuals who have served lengthy prison sentences may feel unmotivated to participate in programming. In addition, individuals confined in restrictive housing may also lack motivation to engage in programming despite having the greatest needs and risks (Beck, 2015). Although program coordinators are tasked to develop unique

strategies to ensure individuals are capable of learning and retaining the information within a program, it may be necessary that program coordinators consider incentives to motivate inmates to progress through the treatment program (Smith, 2016).

Outcome evaluations and Programming in restrictive Housing

Although the principles of effective intervention outline strategies to provide meaningful treatment (e.g., selection of participants), scholars have identified problems that arise when programs are not properly evaluated (e.g., biased treatment effects; Austin, 2009; Duwe & Clark, 2015; Gendreau et al., 1999). These problems may complicate attempts to perform an outcome evaluation (Duwe & Clark, 2015). Outcome evaluations examine whether changes in an outcome (e.g., misconduct) are due to program participation and not other external factors (Rossi, Lipsey, & Freeman, 2004). Through the process of an outcome evaluation, researchers and evaluators may identify whether external factors influence estimations of a treatment effect. According to Rossi and colleagues (2004), an outcome evaluation can provide much needed context for program findings. For instance, an outcome evaluation may reveal that a program is effective at reducing recidivism. However, after controlling for several confounding factors (e.g., length of program participation), researchers may find that the effectiveness of a program diminishes. It is important to note that outcome evaluations differ from process evaluations. In a process evaluation, evaluators and researchers closely examine whether the tenets of a program are being implemented with fidelity and integrity (Rossi et al., 2004).

Relevant to the outcome evaluation performed in this study, there are several factors unique to the provision of programming in restrictive housing that may bias estimations of a treatment effect. Generally, the release of inmates from restrictive housing is deemed beneficial to their psychological well-being (Haney, 2003). However, participants who complete programming may differ from individuals who are released early from restrictive housing, resulting in selection effects that will likely bias outcomes. Other challenges include the need to differentiate between administrative and disciplinary types of confinement, the unique needs of each correctional agency, and the

confinement of individuals in restrictive housing during an outcome evaluation. First, administrative segregation and disciplinary segregation are functionally designed to house two separate types of inmate populations. Administrative segregation is the removal of inmates from the general population for a variety of reasons. For example, correctional administrators and staff may determine that certain inmates are a threat to institutional security and safety, such as posing a risk for violence, riots, escape, self-harm, or participation in security threat groups (e.g., gang membership; Butler et al., 2013; Labrecque, 2016; Lanes, 2011). Disciplinary segregation is reserved for inmates who engage in misconduct and are sanctioned to be removed from the general population for a finite amount of time (Browne et al., 2011). Individuals in administrative segregation, however, may be removed from the general population for an indefinite amount of time (Kurki & Morris, 2001). These differences are important, as inmates who serve time in administrative segregation are typically confined for longer amounts of time than inmates in disciplinary segregation. Offenders who receive programming in disciplinary segregation, as compared with administrative segregation, may be unable to complete the program because they serve less time in restrictive housing. In addition, inmates in administrative segregation may have little motivation for completing programming if it does not influence their likelihood of release from restrictive housing (Kurki & Morris, 2001).

The manner in which correctional agencies manage restrictive housing may also pose challenges that influence estimations of a treatment effect. Although placement in administrative segregation is generally considered to be lengthy, correctional administrators may release offenders in restrictive housing for a variety of reasons. For instance, if an individual is no longer deemed a threat to institutional security then this individual may be released early from administrative segregation (Kurki & Morris, 2001). Similarly, inmates may serve lengthier amounts of time in disciplinary segregation if the punishable offense is serious (e.g., assault against inmates or staff; Morris, 2016). People who are released from restrictive housing before completing programming may not receive adequate dosages to see noticeable effects in outcomes of interest (e.g., misconduct).

A final challenge that merits discussion includes the amount of time incarcerated

individuals serve in restrictive housing during evaluation periods.¹ Inmates who are in restrictive housing have fewer opportunities to engage in certain types of misconduct. For instance, inmates in restrictive housing should have few, if any, opportunities to engage in physical assaults.² These inmates should also have fewer opportunities to procure or solicit contra- band or drug paraphernalia. An outcome evaluation of programming in restrictive housing may mistakenly conclude that a program is effective if the comparison group has a large number of persons who were confined to restrictive housing, which inhibits opportunities for certain types of misconduct. Together, these challenges influence the evaluation of out- comes (e.g., misconduct).

Transformation Project

Transformation Project is a pilot cognitive-behavioral program that targets criminogenic thoughts and attitudes to promote prosocial outcomes that has yet to receive rigorous evaluation. Transformation Project seeks to help inmates identify and alter thinking pat- terns that lead to antisocial behavior that is associated with placement in restrictive housing.

Transformation Project is unique because it does not focus on improving one's skill (e.g., education, anger management, job training), rather, it focuses on changing general attitudes that lead to maladaptive behaviors. It is important to emphasize that the goal of Transformation Project among restrictive housing inmates is to promote prosocial behavior.

Transformation Project's curriculum consists of 13 self-study modules (one orientation and 12 substantive modules), which are based on the transformative prison experience of Malcolm X. Modules utilize real-life examples of change from Malcolm X by relying on excerpts from his autobiography. Each module is linked with an excerpt and focuses on a variety of topics. While the content of each module is unique, each module focuses on mal- adaptive thought processes related to the specific topic. In addition, the modules evoke "change-talk" from participants by identifying dissonance between current values and future goals. For instance, Transformation Project incorporates examples of "real world role models" who underwent a "transformation" toward prosocial change. After learning about the life experiences of these role models,

participants are required to role-play and describe (in writing) how they would respond when confronted with similar adversity. Next, participants describe similar situations in which they were presented with a difficult situation and how they attempted to handle the problem.³ Throughout this process, participants are provided feedback by program staff and facilitators who are trained to provide constructive and detailed feedback to participants.

As stated above, Transformation Project is administered in restrictive housing by program facilitators (e.g., caseworkers, Transformation Project Staff, correctional staff) who have completed resolution-conflict training (e.g., motivational interviewing [MI]). MI is a client-centered (i.e., inmate-centered) therapeutic approach used to enhance readiness for change by resolving ambivalence (Hettema, Steele, & Miller, 2005; Miller & Rollnick, 2013). MI recognizes that clients have differing levels of readiness to change. The role of MI practitioners is to help clients become aware of the consequences of changing or not changing in a nonjudgmental manner (Lundahl, Kunz, Brownell, Tollefson, & Burke, 2010; Miller & Rollnick, 2013). MI emphasizes client autonomy and allows clients to assess whether change is necessary, when to change, and how to change. In corrections, MI techniques have been used with probationers and an international sample of prisoners (Anstiss, Polaschek, & Wilson, 2011; Armstrong, Atkin-Plunk, & Gartner, 2016). MI has been shown to be an effective tool for communication between probation officers and probationers (Armstrong et al., 2016), and participating in brief MI counseling sessions in prison reduced the likelihood of reconviction among a sample of New Zealand prisoners (Anstiss et al., 2011).

Transformation Project facilitators distribute modules to participants who complete the modules and return them to facilitators for feedback. Facilitators review each module and provide individualized written feedback to inmates using resolution-conflict techniques (e.g., address negative thoughts or actions with prosocial alternatives and solutions). Through written feedback, facilitators attempt to identify areas within the modules where participants identify a need to change a certain thought pattern or behavior. Facilitator feedback reinforces what participants have written in modules; facilitators do not point out areas where inmates need to change, nor do they make recommendations about why participants should think about change. The

facilitator then returns the completed module and feedback to participants and distributes the next module. Modules are designed to be completed one at a time, and themes from earlier modules reappear throughout later modules. On average, modules take 1 week to complete. Thus, the program is designed to take 13 weeks to complete. Modules were designed by an independent contractor who is an expert in MI and cognitive programming.

Current Study

The purpose of this study is to perform an outcome evaluation of Transformation Project to examine whether program participation reduces misconducts and whether several factors (i.e., administrative vs. disciplinary segregation, time served in restrictive housing) influence estimations of the treatment effect. We begin by examining the average length of program participation for inmates in our sample. As discussed earlier, there is considerable variation in the amount of time individuals serve in restrictive housing, and we examine whether this influences program completion. Next, we examine the effect of the program on assault, drug/alcohol, and other nonviolent types of misconduct during a 6-month evaluation. After this initial assessment of the program, we examine whether the delivery of the program in disciplinary segregation or administrative segregation influenced misconducts. It is important to note that the program was designed for offenders confined in administrative segregation, and so the delivery of the program in disciplinary segregation would not be in accordance with program design. Finally, the amount of time served in restrictive housing during evaluation periods may bias estimations of a treatment effect because confinement in restrictive housing reduces opportunity for certain types of misconduct (e.g., assaults). However, confinement in restrictive housing does not preclude inmates from engaging in misconducts. Therefore, we estimate a treatment effect with a sample of inmates who served 90 days or less in restrictive housing and another treatment effect with inmates who served more than 90 days in restrictive housing.

Data

Method

The data for this study were collected as part of an evaluation of a program designed for restrictive housing populations in a Midwestern state. The target population for this study included males who participated in Transformation Project and successfully completed any module beyond the orientation module of the program between 2012 and 2014 ($N = 374$). At the time of this outcome evaluation, no participant successfully completed the program, and the program was not operating at female facilities. Transformation Project was implemented in three high-security male facilities that housed medium- and maximum-security offenders. We used a retrospective comparison group of inmates exposed to restrictive housing between 2010 and 2011 to ensure no inmates in this group were exposed to the program ($N = 1,007$).⁴ We spoke extensively with administrators at the study site to discuss whether any substantive changes in policy or practice may influence the use of restrictive housing between 2010 and 2014. We were told no substantive changes occurred during this time that may influence estimates of a treatment effect. In total, 52 inmates were missing relevant information pertaining to program participation and progress or the covariates that are described below, and these inmates were removed from subsequent analyses.⁵ No significant differences between missing and nonmissing cases were found in regard to race, age, and education.

Measures

A treatment effect was estimated by examining the prevalence of assault, drug/alcohol, and nonviolent misconducts during a 6-month evaluation period. The evaluation period started the date inmates submitted their final Transformation Project module, which typically coincided with an imminent transfer from restrictive housing. The evaluation period for inmates in the comparison group started when they were provided a transfer to a different housing unit, which also typically coincided with release from restrictive housing. We estimated three outcome measures of misconduct because of the unique nature of restrictive housing that may inhibit certain types of misconduct (e.g., assault) but potentially increase other types of misconduct (e.g., nonviolent). The operationalization of these outcomes is also in accordance with recent misconduct literature that indicates that correlates of misconduct may have unique

effects on specific types of misconduct (e.g., effect of race/ethnicity on drug/alcohol misconduct; Camp, Gaes, Langan, & Saylor, 2003; Steiner & Wooldredge, 2013). The measures of assault, drug/ alcohol, and nonviolent misconduct represent prevalence or dichotomous (e.g., 0/1) measures of misconduct. The data provided for this study (outcomes and correlates) come from official administrative records. Research examining comparisons between self-report and official measures of misconduct has shown both types of data are valid and reliable (Steiner & Wooldredge, 2014).

Several covariates were included to ensure the treatment and comparison groups were comparable prior to examining a treatment effect and the data from which these measures were derived was obtained from a Midwestern correctional agency. The following covariates included in this study are frequently examined in studies of misconduct that may also influence program participation and completion (Steiner et al., 2014): age (years), race (non-White), marital status (married), education (\geq high school diploma), gang member, received mental health programming, and visited by family (past 6 months). Except for age, each of these covariates is a dichotomous measure. Other covariates include dichotomous measures of commitment offense (sentenced for property offense, sentenced for drug/alcohol offense, and sentenced for other public commitment offense), with sentenced for violent offense serving as the reference category. Time served (in months), served over half of current sentence in RH, and prior misconducts in the past year represent time incarcerated for current sentence, a dichotomous measure of whether the inmate served over half of the current sentence in restrictive housing, and the number of misconducts committed within the past year. Prior misconducts can increase the likelihood of confinement in restrictive housing, in addition to also serving a greater amount of time in restrictive housing. The measure of prior misconducts was top coded at 28 to capture additional variation within the distribution, and the natural log of this measure was taken because the distribution was positively skewed. This number of prior misconducts may appear to be high, but this population is at high risk to engage in misconduct (e.g., inmates in restrictive housing typically have a higher propensity to violate institutional rules; Beck, 2015; Butler & Steiner, 2017). Similarly, a top coded measure of time served was taken at 180 months, using the complete sample to capture additional variation, and the natural log of this

measure was also taken due to the positive skew of the variable (see also Butler et al., 2017).⁶

Statistical Analyses

Due to the nature of confinement in restrictive housing, a randomized experiment would be difficult to perform due to ethical concerns (e.g., withholding rehabilitative programming from certain inmates may have adverse effects on inmate behaviors and institutional security; Mears, 2013; Smith, 2016). Therefore, we rely on propensity score matching to develop a comparison group of statistically identical inmates to estimate a treatment effect. Propensity score matching provides the capability to estimate a treatment effect when experimental designs are not feasible (Guo & Fraser, 2010; Rosenbaum & Rubin, 1984). Propensity score matching estimates propensity scores that calculate the probability of being placed in the treatment or comparison group given included covariates (Rosenbaum & Rubin, 1984). It is important to include covariates that are related to the outcome and the probability of receiving treatment because the omission of key covariates may bias the propensity scores.⁷ After calculating propensity scores, we rely on nearest neighbor one-to-one matching to identify a comparison group that permits the calculation of a treatment effect. Nearest neighbor one-to-one matching pairs inmates in the treatment group with inmates in the comparison group based on the proximity of propensity scores to one another (e.g., closest matching propensity scores are paired and removed from any subsequent matching). This matching technique results in a final sample of 374 inmates in the treatment group who are paired with 374 similar inmates in the comparison group when estimating the treatment effect of the full sample.

We conduct several supplemental analyses using different samples that required the estimation of new propensity scores using one-to-one nearest neighbor matching. This includes the estimation and matching of propensity scores for inmates who received the program in disciplinary segregation, inmates who received the program in administrative segregation, inmates who served 90 days or fewer in restrictive housing during the evaluation period, and inmates who served more than 90 days in restrictive housing during the evaluation period. We determined successful matches by estimating

standardized bias statistics (SBS), which reflect how closely the treatment and comparison group are to one another regarding the included covariates (Rosenbaum & Rubin, 1985). The postmatching results for the supplemental analyses are available upon request.

TABLE 1: Descriptive Statistics Full Sample

Variables	Prematching				SBS ^a	Postmatching				SBS ^a
	Treatment		Nontreatment			Treatment		Nontreatment		
	<i>M</i>	<i>(SD)</i>	<i>M</i>	<i>(SD)</i>		<i>M</i>	<i>(SD)</i>	<i>M</i>	<i>(SD)</i>	
Outcome variables										
Assault misconduct	0.28	(0.45)	0.32	(0.47)						
Drug/alcohol misconduct	0.67	(0.47)	0.73	(0.45)						
Nonviolent misconduct	0.06	(0.23)	0.09	(0.28)						
Independent variables										
Natural log of time served (in months)	3.39	(1.04)	3.02	(1.21)	32.74	3.39	(1.04)	3.43	(1.07)	-3.79
Served over half of current sentence in RH	0.41	(0.49)	0.17	(0.38)	54.73	0.41	(0.49)	0.34	(0.48)	14.43
Age (in years)	29.49	(8.62)	31.92	(11.13)	-24.42	29.49	(8.62)	29.98	(9.69)	-5.34
Natural log of prior misconducts past year	2.04	(0.81)	1.78	(0.74)	33.51	2.04	(0.81)	2.04	(0.76)	0
Non-White	0.59	(0.49)	0.53	(0.50)	12.12	0.59	(0.49)	0.55	(0.50)	8.08
Sentenced for property offense	0.15	(0.36)	0.16	(0.37)	-10.20	0.15	(0.36)	0.17	(0.37)	-5.48
Sentenced for drug/alcohol offense	0.06	(0.24)	0.09	(0.29)	-11.27	0.06	(0.24)	0.05	(0.22)	4.34
Sentenced for other public commitment offense	0.08	(0.27)	0.08	(0.27)	0	0.08	(0.27)	0.06	(0.25)	7.69
Gang member	0.39	(0.49)	0.18	(0.38)	47.89	0.39	(0.49)	0.34	(0.48)	10.31
Visited by family (past 6 months)	0.49	(0.50)	0.44	(0.50)	-30.26	0.49	(0.50)	0.48	(0.50)	2
Married	0.15	(0.36)	0.18	(0.38)	-8.11	0.15	(0.36)	0.15	(0.35)	0
≥ High school diploma	0.17	(0.37)	0.19	(0.39)	-5.40	0.17	(0.37)	0.19	(0.39)	-5.26
Received mental health programming	0.38	(0.49)	0.40	(0.49)	-4.08	0.38	(0.49)	0.38	(0.49)	0
<i>N</i>	374		1,007			374		374		

Note. SBS = standardized bias statistics; RH = restrictive housing.

^aThe standardized bias statistic illustrates whether differences exist between treatment recipients and treatment nonrecipients. Values greater than 20 indicate a significant difference.

Results

Descriptive statistics for the participation and comparison groups are provided in Table 1. Inmates who participated in the program had slightly fewer assault, drug/alcohol, and non-violent misconducts during the evaluation period (e.g., 6 months).

Prior to matching, six of the 13 included covariates were significantly different between the treatment and nontreatment groups ($N = 374$). The significantly different covariates included age, time served in prison, time served in restrictive housing, prior misconducts, gang member, and non-White. The postmatching analyses revealed no significant differences between the treatment group and the nontreatment group.

We first examined the average amount of time participants spent in the program. As stated previously, no inmates successfully completed the program. Table 2 illustrates, on average, how many days each participant spent in the program. The majority of participants spent approximately 28 days in the program prior to stopping or being released from restrictive housing. Less than 10% of the program participants spent more than 50 days in the program.⁸

TABLE 2: Average Time Spent in the Program (in Days; $N = 374$)

No. of days	Frequency, %	Cumulative frequency, %
14	30	30
21	17	47
28	8	55
35	16	71
42	10	81
49	6	87
56	5	92
63	4	95
70	2	97
77	1	98
84	1	100

Note. On average, program facilitators estimated inmates spend approximately 7 days on each program module, which provided the calculations for this frequency distribution.

Next, we examined whether exposure to the program influenced participants' odds of engaging in assault, drug/alcohol, or nonviolent misconduct during the evaluation period, using the full sample of program participants.⁹ Table 3 shows that participants were significantly less likely to engage in assault (28% vs. 35%), drug/alcohol (6% vs. 13%), and non-violent (67% vs. 75%) misconduct relative to the comparison group. Researchers or evaluators may conclude from these analyses that program participation does reduce the odds of engaging in misconducts, but there are additional considerations that may influence estimations of a treatment effect.

TABLE 3: Average Treatment Effect Among the Treated for the Full Sample (N = 748)

Outcomes	Treatment	Nontreatment	Difference	SE	z test
Assault misconduct	.28	.35	-.07	.03	2.13*
Drug/alcohol misconduct	.06	.13	-.07	.02	3.31*
Nonviolent misconduct	.68	.75	-.07	.03	2.10*

Note. The z-test values greater than ± 1.96 indicates a statistically significant difference. Due to one-to-one nearest neighbor matching, there are 374 participants in the treatment group and 374 nonparticipants in the comparison group.

* $p < .05$.

We now examine whether a treatment effect persists for inmates who received the program in different types of restrictive housing (e.g., administrative segregation and disciplinary segregation). It is important to note that the sample size is reduced when examining specific subsamples (e.g., inmates who received the program in administrative segregation).¹⁰ We found no significant differences in the included covariates for the subsample of inmates who received the program in administrative segregation compared with inmates who did not receive the program but served time in administrative segregation ($n = 140$). Table 4 shows that inmates who participated in the program while confined in administrative segregation were not significantly less likely to engage in assault, drug/alcohol, or nonviolent misconducts compared with nonparticipants who were also confined in administrative segregation. Although there were significant differences in misconduct outcomes between the treatment and nontreatment groups when examining the full sample (see Table 2), no significant differences were observed for this subsample.

TABLE 4: Average Treatment Effect Among the Treated for the Administrative Segregation Sample (N = 140)

Outcomes	Treatment	Nontreatment	Difference	SE	z test
Assault misconduct	.33	.37	-.04	.08	.53
Drug/alcohol misconduct	.09	.14	-.05	.05	1.06
Nonviolent misconduct	.74	.69	.05	.08	-.75

Note. The z-test values greater than ± 1.96 indicate a statistically significant difference. Due to one-to-one nearest neighbor matching, there are 70 participants in the treatment group and 70 nonparticipants in the comparison group.

* $p < .05$.

Estimations of a treatment effect may also be influenced when examining a subsample that includes inmates who received the program while confined in disciplinary segregation ($N = 608$). We found no significant differences in misconduct outcomes between inmates who received the program in disciplinary segregation and

inmates who served time in disciplinary segregation but did not receive the program (see Table 5). These findings, together with the administrative segregation findings, reveal the importance of conducting separate estimations of treatment effects for the different types of restrictive housing (e.g., full sample revealed significant differences but the reduced samples were nonsignificant).

TABLE 5: Average Treatment Effect Among the Treated for the Disciplinary Segregation Sample (N = 608)

Outcomes	Treatment	Nontreatment	Difference	SE	z test
Assault misconduct	.27	.30	-.03	.04	.81
Drug/alcohol misconduct	.05	.06	-.01	.02	.71
Nonviolent misconduct	.66	.73	-.07	.04	1.85

Note. The z-test values greater than ± 1.96 indicate a statistically significant difference. Due to one-to-one nearest neighbor matching, there are 304 participants in the treatment group and 304 nonparticipants in the comparison group.

* $p < .05$.

We also examined whether confinement in restrictive housing during the evaluation period influences estimations of a treatment effect. We attempted to estimate treatment effects by examining smaller intervals of time served in restrictive housing during the evaluation period (e.g., no time in restrictive housing and in 30-day intervals) but the sample size was too small to obtain adequate one-to-one nearest neighbor matching. Table 6 presents additional analyses examining whether exposure to restrictive housing for 90 days or less during the evaluation period influences misconduct outcomes between the treatment and comparison groups. We found no significant differences across misconduct outcomes. In other words, inmates who participated in the program and served 90 days or less in restrictive housing during the evaluation period were not significantly less likely to engage in assault, drug/alcohol, and nonviolent misconducts compared with the comparison group.

TABLE 6: Average Treatment Effect Among the Treated for the Sample Who Served Less Than 90 Days in RH During Evaluation Period (N = 340)

Outcomes	Treatment	Nontreatment	Difference	SE	z test
Assault misconduct	.28	.34	-.06	.05	1.17
Drug/alcohol misconduct	.08	.11	-.03	.03	1.11
Nonviolent misconduct	.74	.76	-.02	.05	.38

Note. The z-test values greater than ± 1.96 indicate a statistically significant difference. Due to one-to-one nearest neighbor matching, there are 170 participants in the treatment group and 170 nonparticipants in the comparison group. RH = restrictive housing.

* $p < .05$.

Table 7 reveals inmates who participated in the program and served more than 90 days in restrictive housing during the evaluation period were significantly less likely to engage in nonviolent misconduct (62% vs. 73%, respectively). We found no significant differences for assault or drug/alcohol misconduct. Each of the supplemental analyses revealed most inmates, regardless of program participation, engaged in nonviolent misconduct during the 6-month evaluation period. Similarly, we found inmates in the treatment and nontreatment group were less likely to engage in drug/alcohol misconduct across each of the subsample analyses. More research is needed to examine the types of misconducts inmates are engaging in that are included in the context of nonviolent infractions.

TABLE 7: Average Treatment Effect Among the Treated for the Sample Who Served More Than 90 Days in RH During Evaluation Period (N = 408)

Outcomes	Treatment	Nontreatment	Difference	SE	z test
Assault misconduct	.27	.36	-.09	.05	1.81
Drug/alcohol misconduct	.04	.08	-.04	.02	1.86
Nonviolent misconduct	.62	.73	-.11	.05	2.33*

Note. The z-test values greater than ± 1.96 indicate a statistically significant difference. Due to one-to-one nearest neighbor matching, there are 204 participants in the treatment group and 204 nonparticipants in the comparison group. RH = restrictive housing.

* $p < .05$.

Discussion

There is growing concern regarding the well-being of inmates confined in restrictive housing (American Civil Liberties Union, 2014; Amnesty International, 2014; Lowen & Isaacs, 2012; Riveland, 1999; U.S. Senate Subcommittee on the Constitution, Civil Rights, and Human Rights, 2014). One concern includes the availability and accessibility of rehabilitative programming to inmates (Smith, 2016). To date, however, there is limited research on the effectiveness and applicability of programming in this type of environment. The principles of effective intervention posit treatment is most effective when high-risk offenders with dynamic criminogenic needs are provided programming that is receptive by participants (Andrews & Bonta, 2010; Andrews et al., 1990). Inmates confined in restrictive housing have misconduct histories, in addition to other criminogenic needs (e.g., mental health), that make them ideal participants for rehabilitative programming (Beck, 2015; Butler & Steiner, 2017).

In this study, we examined whether participants of a cognitive-behavioral program in restrictive housing were less likely to engage in institutional misconducts during a 6-month evaluation period. Although we observed, through the use of propensity score matching, that program participants engaged in fewer misconducts than the comparison group; we found limited support that the program was effective at reducing misconducts when examining factors that influence estimations of a treatment effect (e.g., type of restrictive housing and time served in restrictive housing after program participation). One of our key findings is that no inmate successfully completed the program despite the program operating for more than 2 years. One explanation is that there is limited space in restrictive housing, so correctional administrators may release inmates early from disciplinary or administrative segregation who exhibit good behavior in an effort to accommodate new admissions. Correctional administrators are tasked with maintaining safety and security for other inmates and staff, and so the temporary removal of inmates from the general population may help achieve this goal even if inmates do not serve prolonged amounts of time in restrictive housing (Dilulio, 1987; Mears & Castro, 2006). Although several scholars have recommended inmates receive limited exposure to restrictive housing, this does affect the implementation of rehabilitative programming (Haney, 2003; Toch, 2001). We recommend researchers and program coordinators engage in a thorough site evaluation that includes estimations of time served in restrictive housing. We also recommend the development of “aftercare” programming that is available to inmates who may leave restrictive housing early. Aftercare programming may include the continuation of the program or a revised version that continues to help inmates and target criminogenic needs while in the general population of a prison. These programs are frequently used to help inmates transition to society (Wexler, Melnick, Lowe, & Peters, 1999).

Next, we examined whether participation in the program while in disciplinary or administrative segregation influences misconduct outcomes. We found no significant differences in the odds of engaging in misconduct when inmates were matched by segregation type to corresponding comparison groups (e.g., inmates who received the program in disciplinary segregation matched to nonparticipants in disciplinary segregation). Our null findings may be due to several factors. First, no participants

successfully completed the program despite the program being designed for inmates who would serve lengthy periods of time in restrictive housing (e.g., administrative segregation). As outlined by the principles of effective intervention, inadequate treatment “dosages” would produce minimal treatment effects (e.g., reductions in misconducts). Furthermore, we anticipated that the implementation of a program in disciplinary segregation may look much different than one provided to inmates in administrative segregation. Offenders confined in disciplinary segregation often serve less time in restrictive housing than inmates confined in administrative segregation (Browne et al., 2011; Butler & Steiner, 2017). These inmates would, in very rare circumstances, have the opportunity to complete a 13-week program in disciplinary segregation. We advise that the findings from this portion of the evaluation be taken cautiously as there are considerable differences between confinement in disciplinary segregation and administrative segregation that may influence the integrity of programming (e.g., amenities and time served in restrictive housing). We strongly recommend future research continue to delineate whether programming provided to inmates in disciplinary or administrative segregation influences misconduct.

The amount of time individuals serve in restrictive housing during the evaluation period may also influence the estimation of a treatment effect. Inmates who served more than 90 days in restrictive housing during the evaluation period engaged in slightly more assault misconduct than inmates who served less than 90 days (34% vs. 28%). However, inmates who served more than 90 days in restrictive housing during the evaluation period engaged in slightly less drug/alcohol (6% vs. 9%) and other nonviolent misconduct (68% vs. 73%). Although these comparisons do not include program participation, it shows that exposure to restrictive housing during an evaluation period may influence estimations of a treatment effect. Furthermore, our evaluation was limited by a small subsample of inmates that inhibited matching groups based on no exposure (0 days) or complete exposure (180 days) during the evaluation period. Evaluations of the effectiveness of a program in restrictive housing, and potentially any institutional corrections program, should control for time served in restrictive housing. Future research could also use a measure of recidivism that includes return to restrictive housing during the evaluation period. However, such an evaluation would need to identify the type of restrictive housing

that inmates were returned to due to differences in admission processes (e.g., behavior vs. perceived threat to institutional security; Butler et al., 2013).

There are limitations with this study that merit discussion. First, the comparison group was drawn from a sample of inmates exposed to restrictive housing prior to the start of Transformation Project to eliminate threats to the evaluation of the study (e.g., we are certain no inmates in the comparison group received the program). Although we are unaware of any major changes to the use of restrictive housing between the two different sampling periods, there may be unmeasured differences between the treatment and comparison groups that coincided with the provision of programming in restrictive housing. Sample size was also a limitation for some of the supplemental analyses. For instance, we were unable to generate matches on subsamples that included various amounts of time served in restrictive housing (e.g., inmates who served almost the entirety of time during the evaluation period in restrictive housing) due to sample size. There are also important covariates related to misconduct that are not captured with our data. These covariates include prior criminal record (e.g., prior incarceration), misconduct commitment offense, housing unit of inmate, and sentence length. Future research should further examine whether exposure to restrictive housing incapacitates or aggravates the likelihood of certain types of misconduct. Finally, no participant completed the program. It is important to note that despite these limitations, this is one of the first studies to examine programming in restrictive housing. We recommend administrators, practitioners, and researchers to consider these potential threats when developing or evaluating programming in restrictive housing.

A growing body of research has examined the factors that influence successful and unsuccessful correctional programs (Duwe & Clark, 2015; Latessa, Cullen, & Gendreau, 2002). This study contributes to this literature through the use of an outcome evaluation that revealed several factors may influence estimations of a treatment effect (e.g., reductions in misconduct). This study also contributes to the literature by use of a rigorous quasi-experimental design with treatment and comparison groups to better understand the effects of restrictive housing on offender behaviors (e.g., see Morgan et al., 2016, for recommendations to improve restrictive housing research). Researchers and practitioners should continue to use the principles of effective intervention as a guide

to implement programming in restrictive housing (Smith, 2016). However, adherence to the principles of effective intervention is not enough to achieve desired program outcomes (Duwe & Clark, 2015). Efforts to evaluate programming in restrictive housing should consider proper strategies to estimate a treatment effect. The findings from this study revealed that estimations of a treatment effect during the evaluation period should account for the number of days served in restrictive housing. As a growing number of states develop strategies to manage restrictive housing populations, programming in restrictive housing may help inmates cope with confinement, reduce misconducts, and reduce restrictive housing populations.

Notes

1. There are considerably more challenges that may affect the implementation and effectiveness of programming in restrictive housing not discussed here. We hope future research will discuss these problems in depth, but we wanted to focus on three challenges that may have a considerable effect on future programs in restrictive housing.
2. This is not an absolute statement because inmates in restrictive housing may require forcible cell extractions where assaults against correctional officers are a possibility.
3. This is one of many examples of how Transformation Project attempts to provide cognitive-behavioral programming to participants. Cognitive-behavioral programming is broadly defined by Dobson and Kharti (2000) as “An emphasis on broad human change, but with a clear emphasis on demonstrable, behavioral outcomes achieved primarily through changes in the way an individual perceives, reflects upon, and in general, thinks about their life circumstances” (p. 908).
4. Inmates who were originally sampled to be included in the comparison group, but later received the program between 2012 and 2015 were coded and treated as program participants in an effort to ensure no inmate in the comparison group received any programming in restrictive housing.
5. Missing data included seven inmates from the treatment group and 18 inmates

from the comparison group for the education variable. Relevant information on release or start dates to the program in restrictive housing included two inmates from the treatment group and 25 inmates from the comparison group.

6. Approximately, 95% of the sample served 180 months or fewer in prison at the time of the evaluation and 95% of the sample also engaged in 28 or fewer misconducts at the time of the evaluation.
7. Relevant covariates that may influence our ability to accurately match participants to nonparticipants include sentence length, prior incarceration (e.g., criminal history), housing unit, and misconduct commitment offense to restrictive housing.
8. It is important to note that early program modules (Orientation-Module 5) are the cognitive-behavioral foundation of the program. The majority of participants made it to Module 4 or 5 (i.e., 28-35 days in the program). Thus, most inmates who participated should have exposure to the cognitive-behavioral foundations provided by the program.
9. Power analyses indicated that a sample of 101 inmates would be sufficient to detect medium-sized effects with confidence. Thus, we have an adequate sample size to detect treatment effects with confidence (Cohen, 1992).
10. A smaller sample reduces the number of available matches when using one-to-one nearest neighbor matching with propensity scores. We found three significant differences across our supplemental analyses. These differences include time served in restrictive housing and gang membership for the disciplinary segregation subsample, and gang membership for the subsample of inmates who served more than 90 days in restrictive housing during the evaluation period. We ran several supplemental analyses when significant differences were found between the treatment and nontreatment groups (e.g., one-to-many matching and removal of the covariates in question; see Guo & Fraser, 2010). Each of these supplemental analyses provided similar findings to those reported in the tables.

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