

ABSTRACT

Title of Dissertation: EXPLORING HETEROGENEITY IN
DISCIPLINARY CUSTODY SANCTIONING
AND SUBSEQUENT INMATE
MISCONDUCT

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Solitary confinement is often used as a form of punishment for inmate misconduct, a practice known as disciplinary custody. One justification for the use of disciplinary custody is that it should deter future misconduct by increasing the perceived costs associated with committing an infraction. However, research on the deterrent effect of disciplinary custody is limited, largely due to a conceptualization of it as a singular experience, which ignores significant heterogeneity within that punishment. The few studies that have examined one type of treatment heterogeneity, length of stay, are limited in the grouping of varying forms of isolation, such as administrative custody and disciplinary custody, or in the scope of behavior examined post-release from disciplinary custody.

This dissertation built on past studies by examining two types of treatment heterogeneity: (1) length of stay by focusing specifically on disciplinary custody and

expanding on the types of misconduct (beyond violent acts) considered post-release from disciplinary custody; and (2) an early release mechanism. With data from a large state correctional system, this study utilized a sample of first time admissions from 2012 to 2014 who experienced a disciplinary custody stay, and their institutional outcomes were followed through August 2017. This study used inverse probability weighting with regression adjustment, including a large array of relevant covariates to account for pre-existing differences in the treatment conditions examined.

The results of this study do not support specific deterrence theory justifications for the use of disciplinary custody. There was no evidence that increased severity of disciplinary custody stays, either through longer lengths of stay or through serving more than the original sanction length assigned, resulted in lower likelihoods of subsequent misconduct or fewer days until a subsequent misconduct. Implications and future directions are discussed.

EXPLORING HETEROGENEITY IN DISCIPLINARY CUSTODY SANCTIONS
AND SUBSEQUENT INMATE MISCONDUCT

by

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Chapter 1: Introduction

Statement of the Problem

Over the past 40 years, the number of prisoners housed in federal and state detention facilities increased dramatically, from about 200,000 in 1973 to 1.5 million in 2009 (Travis, Western, & Redburn, 2014). During the past several years this growth has slowed and the incarcerated population has begun decreasing slowly; but even with this slight decline, an estimated 1,489,363 individuals were incarcerated at year-end 2017 (Bronson & Carson, 2019). As mass incarceration increased the number of individuals housed within correctional systems, the number of individuals housed within solitary confinement and restricted housing in these systems also increased (Travis et al., 2014). Today, estimates range on the number of individuals housed within these settings, but a National Research Council report identified 5% of the U.S. prison population living in isolation at any given time (Travis et al., 2014). In terms of actual numbers, estimates identify at least 80,000 prisoners housed in some form of isolation (Browne, Cambier, & Agha, 2011), with about 25,000 housed in so-called “supermax” facilities (Mears, 2006).

One primary purpose of solitary confinement is disciplinary custody¹ in which isolation is used as a punishment for committing an infraction or misconduct within prison (Browne et al., 2011). Inmate misconduct is any act undertaken by an inmate that violates a prison facility’s rules (Steiner, Butler, & Ellison, 2014) and can range from

¹ Throughout the remainder of this dissertation, I will distinguish between the terms solitary confinement and disciplinary custody. Solitary confinement will refer to all forms of isolation from the general population while disciplinary custody specifically refers to the use of isolation as a punishment for a specific infraction.

violent events, such as assault on a staff member, to more minor infractions, such as being out of bounds or disobeying a correctional officer's order. The most common justification for disciplinary custody is that it serves as a specific deterrent against future misconduct among individuals sentenced to that punishment (Lucas & Jones, 2017). As highlighted in a Vera Institute of Justice report, "Many prison officials support the use of segregated housing for managing disruptive and violent behavior because they believe that it has both a general and individual deterrent effect on misbehavior" (Shames, Wilcox, & Subramanian, 2015, p. 20). Simply put, the idea is that individuals exposed to disciplinary custody will abstain from committing future misconduct in order to avoid returning to that environment (Shames et al., 2015).

Inmate misconduct within prisons provides an interesting testing ground for deterrence theory. The three components to deterrence theory, certainty, severity, and celerity, can be applied to punishment practices within prison. Misconduct within prison has a much higher likelihood of detection and punishment relative to criminal behavior outside of a correctional system. Additionally, sanctions for misconducts are typically swift, often within days or weeks of the reported infraction. Together, these factors provide a potential test for deterrence theory, given arguments that the criminal justice system is not set up to enact deterrent effects due to the delays and uncertainty that is inherent in the process between a criminal event and eventual punishment (Paternoster, 2010).

Few studies directly test the presumption of specific deterrence for disciplinary custody by examining the effect of short-term solitary confinement on behavioral outcomes within prison (Lucas & Jones, 2017). As a collective, the research that does

exist concludes there is no evidence of a deterrent effect (Labrecque, 2015; Labrecque & Smith, 2019; Lucas & Jones, 2017; Medrano, Ozkan, & Morris, 2017; Morris, 2016; Woo, Drapela, Campagna, Stohr, Hamilton, Mei, & Tollefsbol, 2019). For example, Labrecque (2015) examined a sample of Ohio state prisoners and the effect of spending time in disciplinary custody in the prior three-month period on misconduct in the current period and concluded that his most important finding was “the lack of evidence of any effect of solitary confinement on subsequent inmate misconduct” (p. 112). The state of research can be categorized as those that examined a dummy indicator of solitary confinement and subsequent misconduct, and those that examined the length of stay in solitary confinement and subsequent misconduct.

The studies in the first group utilized data from a single state correctional system, restricting the sample to the first misconduct committed by an individual, and compare those receiving a disciplinary custody sanction as a result of that misconduct to those receiving a different punishment or no formal punishment for that misconduct to examine the deterrent effect of disciplinary custody. Such research focused on the effect of a disciplinary custody stay and treats all stays in disciplinary custody as monolithic, leaving many areas of unexplored heterogeneity in the solitary confinement experience. These studies concluded that disciplinary custody, at least as measured, does not reduce the likelihood of subsequent misconduct (Labrecque, 2015; Lucas & Jones, 2017; Medrano et al., 2017; Morris, 2016) nor shorten the time to the next misconduct (Morris, 2016). In fact, disciplinary custody may even increase subsequent misconduct (Medrano et al., 2017).

Although this research was an important first step in understanding the consequences of short stays in solitary confinement as a punishment, as a whole it treated disciplinary custody as a singular experience, similar to the early work on the effect of incarceration on subsequent recidivism, which compared all carceral sentences to those receiving non-custodial punishments (Mears, Cochran, & Cullen, 2015). In these studies, an individual receiving one day in disciplinary custody was grouped together with individuals serving weeks or months, and an average treatment effect was estimated, resulting in possible aggregation bias. In short, these studies largely examined exposure to disciplinary custody and subsequent misconduct while ignoring heterogeneity in the treatment experience—heterogeneity that could be related to differential deterrent effects.

There is reason to believe that treating all disciplinary custody stays as the same could mask deterrent effects. For example, deterrence theory would state that more severe sanctions (i.e. longer stays in disciplinary custody) should result in a greater specific deterrent effect (Beccaria, 1764), either in total cessation of criminal activity or differences in the frequency or seriousness of offending. Studies combining all disciplinary custody effects together would be grouping individuals with a wide range of sanction lengths, resulting in possible aggregation bias in estimating a deterrent effect. Although the certainty of sanctions has found more support in deterrence empirical research (Paternoster, 2010), sanction severity matters theoretically and could have an impact on the effect of disciplinary custody on subsequent misconduct.

The second group of prior studies built on prior research by examining one area of treatment heterogeneity: the length of stay in solitary confinement. This work acknowledged the need to examine differences in the solitary confinement experience,

but limitations in these studies and the focus on a single type of heterogeneity restricts the knowledge gained. Utilizing varying methodologies, this research examined how differential time spent in isolation as a whole (Labrecque & Smith, 2019) or disciplinary custody specifically (Labrecque, 2015; Woo et al., 2019) impacted subsequent misconduct and generally concluded a lack of a deterrent effect. Although this work improved the understanding of the differences in disciplinary custody sanctioning, methodological limitations necessitate further study. For example, one study combined all solitary confinement types into one measure (e.g., administrative custody along with disciplinary custody) and then examined the deterrent impact of length of stay (Labrecque & Smith, 2019). Combining types of solitary confinement in estimating these effects is problematic because only one form – disciplinary custody – is used explicitly as a punishment and would thus be expected to have a deterrent impact. Another study solely examined disciplinary custody as a result of violent infractions and then examined subsequent violence (Woo et al., 2019). However, disciplinary custody is used as a punishment for a wide range of misconduct behaviors (the majority of which are non-violent) and could have a deterrent impact aside from how it relates to violence.

These limitations in prior studies necessitate further research. First, additional work needs to expand on the length of stay question, particularly by focusing on disciplinary custody itself and by expanding the misconduct types leading to and following the disciplinary custody experience to more accurately examine the deterrent impact of disciplinary custody. Second, the field would benefit from research examining more than just one type of heterogeneity in the disciplinary custody experience. There are many sources of unexplored treatment heterogeneity in the disciplinary custody

experience such as behavior within isolation, cells within these units (single-cell vs. double-celling), crowding issues within isolation units, extent of contact with correctional or programming staff, and potential variation in disciplinary custody units across different levels of institutional security.

The current study investigates two types of variation within the disciplinary custody experience that may relate to the deterrent capability of that punishment. First, length of stay is examined for the reasons discussed above, namely deterrence theory's prediction that longer stays in a particular punishment should result in greater deterrent effects. Second, the role of risk updating may also be obscured by aggregation bias in prior studies (Anwar & Loughran, 2011; Lochner, 2007; Matsueda, Kreager, & Huizinga, 2006). Individuals sentenced to disciplinary custody receive a specific sanction length and, at least within the Pennsylvania correctional system, can be released early to the general prison population after serving a certain amount of that time. In light of evidence that individuals update their risk perceptions (Lochner, 2007), deterrence theory would support the hypothesis that getting released earlier than anticipated may prompt prisoners to recalibrate their perception of the sanction as less serious, thus diminishing the specific deterrent effect. On the other hand, defiance theory highlights that procedures and sanctions perceived as procedurally just and fair could increase the likelihood of subsequent deterrent effects (Sherman, 1993). Thus, based on this theory, it is possible that early release alters views of the misconduct sanctioning procedure as more just and fair, increasing the likelihood of a deterrent effect of the punishment (assuming prisoners view serving less than their assigned punishment as "more fair"). Research examining expectation disconfirmation models (discussed further below) would suggest that cases in

which expectations (in this case serving the full sanction length) are disconfirmed in a positive way (i.e. early release) could improve the individual's satisfaction with the experience (Brown, Venkatesh, Kuruzovich, & Massey, 2008; Oliver, 1980; Reisig & Chandek, 2001; Van Ryzin, 2013).

The assumption that heterogeneity in the experienced treatment of disciplinary custody may be related to differences in deterrence is based largely in theory, but there are related empirical reasons to suspect such a relationship. To be specific, scholars have documented the deleterious effects of solitary confinement on individual's mental and physical health (Arrigo & Bullock, 2008; Haney, 2003; Haney & Lynch, 1997; Smith, 2006).² Interestingly, this research suggests that the effect of solitary confinement on mental health outcomes differs by length of stay (Smith, 2006). For example, in a study of Danish prisoners, Sestoft, Anderson, Lillebaek, and Gabrielsen (1998) found the risk for hospitalization for psychiatric morbidity increased with time spent in solitary confinement. These findings related to health highlight the importance of also considering differential experiences of disciplinary custody in understanding the deterrent effect of disciplinary custody.

There are three main reasons beyond theory as to why considering solitary confinement experiences deserves further empirical study. First, housing prisoners in isolation conditions is much more expensive than housing the average individual in the general prison population (Browne et al., 2011; Reiter, 2012). Currently, correctional housing encompasses a large proportion of state budgets (Pew Center on the States, 2009), with a National Research Council report identifying corrections as the "third

² Due to varying definitions across studies, research summarizing the effect of isolation on mental and physical health often groups together various types of isolation, such as supermax prisons and short solitary confinement stays.

highest category of general fund expenditures in most states, ranked behind Medicaid and education” (Travis et al., 2014, p. 314). Due to the increased operating costs associated with solitary confinement, it is in the best interest of state governments and prison administrators to restrict the use of solitary confinement to the fewest number of prisoners necessary to maintain general prison order. As highlighted by Morris (2016), there is “little known about the return on investment from the use of solitary confinement in and of itself” (p. 4).

Second, there is some evidence of a relationship between inmate misconduct and recidivism post-release (Cochran, Mears, Bales, & Stewart, 2014). However, little is known about the mediating or moderating effects of misconduct sanctioning in this relationship. Punishments within prison may not only contribute to and maintain institutional order, but if there is a specific deterrent effect of that punishment, this may also help protect public safety post-release by decreasing misbehavior within prison. Identifying the conditions under which disciplinary custody may serve as a deterrent is important to understand the relationship between inmate misconduct and offending post-release.

Lastly, and perhaps most importantly, there is extensive evidence documenting the negative consequences of solitary confinement on mental and physical health of those subjected to isolation (Arrigo & Bullock, 2008; Haney, 2003; Haney & Lynch, 1997; Smith, 2006). These effects necessitate valid and convincing arguments for the continuing use of solitary confinement as punishment. A finding of a deterrent effect of disciplinary custody could justify the continuation of this practice for specific inmates in the eyes of prison administrators. However, if research continues to find a lack of a

deterrent effect for disciplinary custody, additional avenues and punishments should be explored to lessen the number of individuals subjected to these harsh conditions and potentially deleterious health consequences as a result of a misconduct or rule infraction.

Research Goals

This dissertation examines treatment heterogeneity in disciplinary custody and its effect on specific deterrence, particularly focusing on differences in time spent, both objectively and relative to initial sentence, on subsequent misconducts. The current study utilizes data from the Pennsylvania Department of Corrections (PADOC). As the 7th largest state correctional system in the United States with approximately 50,000 inmates in custody at any given time, Pennsylvania provides a valuable location to examine the role of heterogeneity in solitary confinement as punishment. Full demographic information as well as misconduct and punishment histories were gathered for all individuals housed within PADOC between 2012 and 2014 (N=93,536). This dissertation specifically focuses on those individuals who were admitted to state prison for the first time between 2012 and 2014 with sanctioned misconducts and at least one documented disciplinary custody stay (n=4,425). Some prior studies have compared those receiving disciplinary custody for misconduct to those receiving a different punishment (Medrano et al., 2017; Morris, 2016). To examine treatment heterogeneity in the disciplinary custody experience, it is more appropriate to compare those receiving different levels of the same treatment. Thus, rather than a counterfactual of receiving no treatment (or a different punishment), the counterfactual of interest is receiving a different amount of the treatment, such as a longer stay in disciplinary custody. In other words, among those who are treated, do differences in the treatment predict differences in the outcome of interest?

This study focuses solely on the role of disciplinary custody and the potential deterrent effect of this punishment. To accomplish this goal, other forms of isolation within prison, such as administrative custody or supermax prisons, are not included in the sample. The rationale and justification for each type of isolation is different and thus deserves attention in other studies separate from the current evaluation of deterrent effects. Additionally, as highlighted by Morris (2016), disciplinary custody “is commonly used by corrections administrators in general prison settings and is a practice that is amendable to modification driven by validated empirical evidence” (p. 6).

With these data, this dissertation tests six hypotheses based in deterrence theory on the role of heterogeneity in the disciplinary custody sanction. Although there are competing theoretical arguments regarding the expected result of early release from disciplinary custody, the focus of the current research is the deterrent capabilities of differences in the disciplinary custody sanction. Thus, the following hypotheses are informed by deterrence theory.

Hypothesis 1: Longer stays in disciplinary custody will be associated with a larger specific deterrent effect, measured as a lower likelihood of subsequent misconduct.

Hypothesis 2: Longer stays in disciplinary custody will be associated with a larger specific deterrent effect, measured as a greater number of days until the next misconduct incident.

Hypothesis 3a: Early release from disciplinary custody will be associated with a smaller specific deterrent effect, measured as a higher likelihood of subsequent misconduct.

Hypothesis 3b: Serving a lower proportion of the original disciplinary custody sanction length will be associated with a smaller specific deterrent effect, measured as a higher likelihood of subsequent misconduct.

Hypothesis 4a: Early release from disciplinary custody will be associated with a smaller specific deterrent effect, measured as a fewer number of days to the next misconduct incident.

Hypothesis 4b: Serving a lower proportion of the original disciplinary custody sanction length will be associated with a smaller specific deterrent effect, measured as a fewer number of days to the next misconduct incident.

I evaluate these hypotheses using a propensity score framework to examine the deterrent effect of heterogeneity in the disciplinary custody experience. Using a sample of inmates found guilty of a misconduct offense and sentenced to a disciplinary custody stay, the current study utilizes an extension of traditional propensity score methods to weight each treatment case by the inverse of the propensity of receiving that particular treatment (Hirano, Imbens, & Ridder, 2003), and then uses regression adjustment to account for the selection bias inherent to studying heterogeneity in disciplinary custody stays. This methodology, called “inverse probability weighting with regression adjustment” (IPWRA), is used to examine all three treatments of interest, length of stay, binary early release, and multivalued early release. The propensity score is estimated with an array of theoretically-relevant predictors such as custody level, mental health status, and prior misconduct history. An additional extension for survival outcomes is implemented to examine the restrictive deterrent impact of each treatment condition, modeling the days to a subsequent misconduct following release from disciplinary

custody. In the end, this research contributes to the state of knowledge by examining two distinct types of treatment heterogeneity in disciplinary custody stays, further isolating the potential deterrent capabilities of disciplinary custody as a punishment for prison misconduct.

Chapter 2: Literature Review

History of Solitary Confinement in the United States

Throughout the history of correctional institutions in the United States, solitary confinement had many different purposes such as reformation, punishment, protection, behavior modification, and prisoner management and control (Feeley & Simon, 1992; Haney & Lynch, 1997; Labreque, 2015; Pizarro & Stenius, 2004; Shalev, 2013). The use of solitary confinement dates back to the first formal correctional institutions with the penitentiary system in Pennsylvania in the late 1700s and early 1800s. In this system, all individuals were housed in complete isolation with the aim of penitence and rehabilitation (Cloud, Drucker, Browne, & Parsons, 2015). Advocates for the penitentiary system believed these practices to be more humane than the barbaric public punishments of floggings and public executions of the past (McLennan, 2008; Reiter, 2012). This system's goal was that solitary confinement would allow prisoners to reflect on their wrong doings and seek atonement in silence (Cloud et al., 2015; Reiter, 2012). This penitentiary system competed with the congregate model, originating in Auburn, New York in the early 1800s, in which inmates ate and worked together in silence before returning to isolated cells (Reiter, 2012). These individuals were still technically housed in isolation in singular cells, but the ability to move about the prison and work and interact with other inmates (albeit silently) differentiated this model from that of the penitentiary.

Although first created with the goal of redemption and reformation, psychological and physical damage caused by widespread seclusion was rampant among inmates housed in penitentiaries (Browne et al., 2011; Haney & Lynch, 1997). As highlighted by Reiter (2012), “...so many prisoners in solitary confinement went insane, lost all ability to function, or committed suicide that the practice became unsustainable” (p. 72). The United States Supreme Court documented the deleterious effects of the solitary confinement conditions in these facilities in an 1890 written opinion:

A considerable number of the prisoners fell, after even a short confinement, into a semi-fatuous condition, from which it was next to impossible to arouse them, and others became violently insane; others, still, committed suicide; while those who stood the ordeal better were not generally reformed and in most cases did not recover sufficient mental activity to be of any subsequent service to the community (*In Re Medley*, 1890, p. 168).

These issues in penitentiaries contributed to their demise as wardens and prison administrators began limiting the continuous use of isolation in their facilities (Reiter, 2012). The congregate system was identified as more sustainable and eventually became the standard system with prisoners used as a source of cheap labor (Cloud et al., 2015; McLennan, 2008; Rothman, 1980), but the original congregate model’s use of silence and singular celling eventually faded in practice. Short-term stays in isolation continued in prisons, while long-term solitary became increasingly rare entering the 20th century (Cloud et al., 2015; Haney & Lynch, 1997).

Large-scale segregation within U.S. correctional systems was reintroduced with “supermaximum” facilities (Briggs, Sundt, & Castellano, 2003; Browne et al., 2011; Haney & Lynch, 1997). This new era was preceded by the opening of Alcatraz Prison in 1934 in which the federal system’s most infamous and dangerous prisoners were housed until 1963 (Riveland, 1999) when the facility was closed as the rehabilitation model

gained prominence within corrections. It was not until highly publicized prison unrest and heightened levels of violence within prisons that the first identifiable “supermax” unit was constructed at the United States Penitentiary in Marion, Illinois in 1978 (Riveland, 1999). In late 1983, a week of violence that resulted in the death of two guards and an inmate led to an indefinite lockdown of the facility, in which prisoners were confined to their cells for 23 hours a day (Fellner & Mariner, 1997; Riveland, 1999). In 1986, the Security Management Unit, modeled after the locked-down conditions in Illinois, opened in Florence, Arizona, followed by the infamous Pelican Bay State Prison, built in California in 1989 (Reiter, 2012). By 2004, 40 states had supermax facilities (Cloud et al., 2015). The emergence of these facilities marked a shift in penal policy to a focus on prisoner management and control (Feeley & Simon, 1992), particularly for the incarcerated individuals considered to be exceptionally dangerous. These new supermax prisons were used to house the worst of the worst, with the goal of lessening violence and disruptions in other units and serving as a deterrent for the general prison population (Mears & Bales, 2009; Naday, Freilich, & Mellow, 2008; Pizarro & Stenius, 2004; Reiter, 2012; Riveland, 1999).

Throughout all eras of correctional policy, a form of isolation or solitary confinement has consistently been used as a punishment for disruptive inmates. Although lengths of time spent in isolation as punishment and the conditions of the punishment has changed, regardless of shifts in punishment philosophy, there has consistently been the use of a “jail within prison” (Riveland, 1999). For example, in the post-Civil War decades, among other more medieval punishments such as hanging an individual by his arms from the ceiling of his cell, solitary confinement was given as punishment in which

individuals were housed in completely dark cells and served rations consisting solely of bread and water (Rothman, 1980, p. 20). Additionally, in the following era of penal policy typified by Zebulon Brockway and the introduction of reformatories, in response to the perceived horrors of the penitentiary, offenders received indeterminate sentences and had to demonstrate conformity to prison rules and reform within prison in order to be released (Rothman, 1980). However, even within this system, solitary confinement was given as a punishment for infractions in which the individual was shackled by the hands or feet to their cell door if the system of benefits was not enough to keep an inmate from disrupting the prison order (Rothman, 1980). Solitary confinement as punishment continues today, although prisoner litigation has greatly improved conditions for those housed in these units. This litigation has resulted in requirements within isolation conditions to include access to exercise, access to minimum physical comforts such as consistent lighting, and physical safety (Reiter, 2012). Today, solitary confinement is most commonly used in three capacities: “as a form of punishment for rule violations, as a way to remove prisoners from the general prison population who are thought to pose a risk to security or safety, and as a way to provide safety to prisoners believed to be at risk in the general prison population” (Browne et al., 2011, p. 46).

Consequences of Solitary Confinement

Throughout the history of solitary confinement within prisons, there were consistently documented consequences of the practice. One issue with widespread segregation use is that housing individuals in isolation is much more expensive compared to the general prison population (Browne et al., 2011; Reiter, 2012). This is particularly the case in the operation of supermax facilities (Mears, 2006). These additional costs

largely come from the greater number of correctional staff needed within isolation units as well as the additional technological advancements needed for these units, such as reinforced walls and doors, electronic locks and perimeters, and additional monitoring systems (Browne et al., 2011; Riveland, 1999).

In addition to significant financial costs, the research to date on the effects of solitary confinement on health and mental health repeatedly documents harmful effects. Empirical evidence on “solitary and supermax-like confinement has consistently and unequivocally documented the harmful consequences of living in these kinds of environments” (Haney, 2003, p. 130). There is a wide range of negative effects attributed to time spent in solitary confinement, including: anger, boredom, trouble sleeping, hallucinations, impaired concentration, hypertension, depression, anxiety, and suicidal thoughts and behavior (Haney, 2003, 2018; Haney & Lynch, 1997; Reiter, Ventura, Lovell, Augustine, Barragan, Blair, Chesnut, Dashtgard, Gonzalez, Pifer, & Strong, 2020; Smith, 2006). One summary of the literature noted “between one-third and more than 90 percent experience adverse symptoms in solitary confinement, and a significant amount of this suffering is caused or worsened by solitary confinement” (Smith, 2006, p. 502). There are occasional studies that do not find a negative impact of solitary confinement on mental health (see Zinger, Wichmann, & Andrews, 2001). However, these studies are often intensely criticized for their methodological limitations (Haney, 2003). Other research highlights the relationship between solitary confinement and suicide rates (Patterson & Hughes, 2008), finding that that “the conditions of deprivation in locked units and higher-security housing were a common stressor shared by many of the prisoners who committed suicide” (p. 678) within California Department of Corrections

between 1999 and 2004. This research highlights the long-term and devastating consequences that come from widespread use of solitary confinement within the United States. The National Commission on Correctional Health Care in 2016 concluded that, “...the very nature of prolonged social isolation is antithetical to the goals of rehabilitation and social integration” (NCCHC, 2016, p. 258).

These documented fiscal and mental health concerns with the widespread use of isolation contribute to the controversy surrounding the use of disciplinary custody and supermax prisons. Although the mental and physical health consequences of time spent in solitary confinement are well-documented, eighth amendment challenges to the use of solitary confinement in prisons have generally been unsuccessful. The courts have consistently found constitutional violations in the conditions of solitary confinement in specific instances (for example, the provision of clean bedding and clothing and access to showers and exercise in the 1986 case of *Toussant v. McCarthy*), but have failed to rule solitary confinement as a general practice to be unconstitutional (Reiter, 2012). In the face of mounting evidence of the detrimental impact of solitary confinement on those presenting with mental health diagnoses (Smith, 2006), the courts have created some standards limiting the placement of these individuals in supermax facilities (see *Madrid v. Gomez*, 1995). Other fourteenth amendment challenges have been successful and resulted in courts mandating particular procedures necessary to place individuals in supermax units (Reiter, 2012; see *United States ex. rel. Miller v. Twomey*, 1973).

Definitions and Differences in Types of Isolation

As solitary confinement has served multiple roles throughout correctional history and today, it is necessary to define and distinguish between types of isolation. Today, there are three main types of isolation used within correctional systems: supermax prisons, administrative custody/segregation, and disciplinary custody/segregation. The most commonly discussed and studied use of isolation is supermax prisons (Fellner & Mariner, 1997; Lovell, Johnson, & Cain, 2007; Mears & Bales, 2009; Pizarro & Stenius, 2004; Reiter, 2012; Riveland, 1999). The National Institute of Corrections defined “supermax” housing as:

A free-standing facility, or a distinct unit within a facility that provides for the management and secure control of inmates who have been officially designated as exhibiting violent or serious and disruptive behavior while incarcerated. Such inmates have been determined to be a threat to safety and security in traditional high-security facilities, and their behavior can be controlled only by separation, restricted movement, and limited direct access to staff and other inmates (National Institute of Corrections, 1997, p. 1).

Within these facilities, all prisoners are “held in high levels of confinement, often for long periods of time” (Browne et al., 2011, p. 47). Supermax prisons are built specifically to house the most troublesome or dangerous inmates (Briggs et al., 2003; Naday et al., 2008) within buildings that are architecturally “built to restrict visual and tactile stimulation for prisoners, as well as contact with others” (Browne et al., 2011, p. 47). Supermax facilities generally confine inmates to their cells for 23 hours a day, limiting human contact to a select few, mostly correctional guards, medical staff, counselors, or members of the clergy (Fellner & Mariner, 1997), contributing to consistent sensory deprivation among inmates housed in these facilities (Reiter, 2012). Generally, education and programming are restricted within these prisons as a whole

(Browne et al., 2011). Stated goals of supermax prisons vary (Naday et al., 2008), with a survey of supermax wardens across the country identifying goals such as increasing safety, order, and control in a prison system, incapacitating disruptive prisoners, improving inmate behavior in the full system, and decreasing riots and escapes, among others (Mears, 2006; Mears & Castro, 2006). Supermax prisons have drawn criticism since their inception due to the significant monetary costs associated with the operation of these technologically-advanced facilities, the high levels of deprivation experienced by those housed, and concerns with what constitutes a violation of the eighth amendment's protection against cruel and unusual punishment (Briggs et al., 2003; Mears, 2013).

Correctional institutions also use administrative custody or segregation as a form of isolation. Administrative custody is typically used to “remove prisoners from the general prison population who are thought to pose a threat to safety or security, or for prisoners who are believed to have information about an incident under investigation” (Browne et al., 2011, p. 47). Administrative custody often is used to house those such as gang-affiliated inmates (Pyrooz & Mitchell, 2020) or particular types of offenders who would otherwise be in danger if left in the general prison population. Administrative custody has also been used to house individuals with severe mental health issues or other populations such as death row inmates or those testing HIV-positive (Riveland, 1999). Administrative custody is “not a form of punishment for a specific violation” (Browne et al., 2011, p. 47).

Disciplinary segregation or custody is a “form of punishment for rule violations occurring within the prison setting” (Browne et al., 2011, p. 47). Inmate violence and misconduct can threaten the safety of prisoners and correctional staff, strain correctional

institutions financially, and undermine the order of an institution (Lovell & Jemelka, 1996; Mears, 2013), and prisons use various punishments to isolate disruptive inmates and maintain prison order. For example, within the Pennsylvania Department of Corrections, the possible punishments following a rule misconduct event are: disciplinary custody; administrative custody; cell restriction; confiscation of contraband; loss of privileges; payment for property loss; reprimand, warning, counseling; revoke pre-release status; suspension/removal from job.

An important caveat to disciplinary custody is that although it is a form of solitary confinement, serving a disciplinary custody sanction is not guaranteed to be a solitary experience. Documented prison overcrowding (Carson & Golinelli, 2014; Travis et al., 2014) has contributed to the use of double-celling of individuals placed in isolation in state correctional systems across the country (Browne et al., 2011). This is a practice within restricted housing units in PADOH when overcrowding is a problem and individuals in the current study may have served part, if not all, of their disciplinary custody sanction in a cell with another individual. Even so, other components of a disciplinary custody stay contribute to the perceived severity of this type of punishment. Individuals placed in disciplinary custody spend up to 23 hours in their cell, with little-to-no outside stimuli or natural light (Lovell, Tubiltz, Reiter, Chesnut, & Pfier, 2020). Access to jobs is prohibited and program participation is significantly limited (Browne et al., 2011). Individuals in these units also have limited access to visitation or phone use, cutting off communication to those outside the prison system (Lovell et al., 2020). These factors combine to provide little to break up the monotony of day-to-day existence within these units.

Inmate misconduct is handled similarly to violations of criminal laws. Official reports of misconduct lead to formal charges, pleas, verdicts, and sanctioning. In the case of *Wolff v. McDonnell* (1974), the Supreme Court ruled that basic procedure must be followed when punishing rule infractions within prisons to ensure inmate rights are protected. These rights include making the individual aware of the charges, conducting a fair hearing, and providing the inmate with a written statement of the official decision in the case. State correctional systems vary in the process and steps taken before an individual can be sentenced to disciplinary custody following a rule violation (Riveland, 1999). For example, within Pennsylvania's system there is a process in which a misconduct incident is reported by a correctional officer, and that report is reviewed and signed off by a superior. The superior then refers the case to a hearing in which an examiner determines innocence or guilt in the misconduct and if guilty, assigns a punishment and a length for that punishment. Hearing examiners have multiple punishments to choose from, and disciplinary custody is one of the more severe punishments available for prison infractions. Unlike the criminal court process, in misconduct sanctioning, there is limited external oversight, hearings are not made public, and inmates have fewer due process rights than defendants in a criminal court. Although departments of corrections write formal policies for how misconduct incidents should be handled, hearing examiners overseeing and deciding misconduct cases rarely have the legal training or the experience of criminal court judges, and compliance with formal procedures or consistency in sentencing across examiners is unknown. These factors come together to provide hearing examiners with wide discretion when sanctioning

inmate misconduct incidents. The current study focuses solely on disciplinary custody imposed as a punishment for an infraction.

Theoretical Justification for Disciplinary Custody

Deterrence Theory

Deterrence is the most common justification for the use of solitary confinement as a punishment (Lucas & Jones, 2017; Woo et al., 2019). Additional justifications include incapacitation of disruptive inmates or retribution for harm done or costs imposed on the correctional system (Woo et al., 2019). Research on the goals of disciplinary custody specifically is scarce, but discussion of other forms of isolation, such as supermax prisons, is relevant. A national survey of state prison wardens noted goals of supermax facilities such as improving inmate behavior both throughout the prison system and for inmates in those facilities specifically (Mears, 2006; Mears & Castro, 2006). Mears and Bales (2009) also argued that supermax prisons should serve as a specific deterrent due to the severity of conditions in supermax housing relative to the general prison housing, thus increasing the costs associated with stays in these facilities. As disciplinary custody creates a similar environment with limited mobility and personal contact, these arguments may also be applicable to disciplinary custody.

Deterrence theory is concerned with how sanction threats and the imposition of sanctions inhibit criminal activity. General deterrence is the inhibition of criminal activity among society as a whole, while specific deterrence denotes the inhibition of criminal activity among specific offenders who experience the sanction or threat first-hand

(Stafford & Warr, 1993). Rooted in the work of enlightenment philosophers, deterrence theory states that sanctions are expected to deter future crime to the extent that the punishment is certain, swift, and severe enough to outweigh any potential benefits obtained from committing that crime (Beccaria, 1764; Bentham, 1789). These philosophers viewed humans as rational actors motivated by self-interest who weigh the costs and benefits of a given decision and choose the action in which the benefits outweigh the costs. These views formed what became known as the “classical school” of criminology and challenged the long-held view that criminal behavior was the result of supernatural or demonic forces.

Another school of thought, the positivist school, arose in the mid-19th century and deterrence theory as it related to crime did not regain traction until the 1960s when economists such as Becker (1968) outlined a more detailed, specific equation for the idea of deterrence. Building off Beccaria (1764) and Bentham (1789), Becker (1968) put forth an expected utility model that stated an individual will engage in crime if the expected utility from committing the crime is greater than the expected utility from refraining from committing that crime and engaging in other activities instead. The expected utility approach outlines a “function relating the number of offenses by any person to the probability of his conviction, to his punishment if convicted, and to other variables, such as the income available to him in legal and other illegal activities, the frequency of nuisance arrests, and his willingness to commit an illegal act” (Becker, 1968, p. 177). The expected utility function of committing an offense is represented as³:

$$EU = pU(Y - f) + (1 - p)U(Y)$$

³In this equation, p represents the probability of apprehension, f is the punishment given the offense, and Y represents the benefits stemming from a successful completion of the crime (Becker, 1968, p. 177).

These calculations differ by person and are subject to “bounded rationality” (Simon, 1957) in which individuals are not fully rational but rather weigh the costs and benefits of a given decision with imperfect information, using only the information available to them with restrictions on the individual capacity to analyze or process that data to come to a decision. This work is often cited as the source of rational choice theory in criminology (Cornish & Clarke, 1986), in which formal punishments and the risk perceptions associated with those punishments are just one component of the calculation in the decision to commit crime.

The majority of empirical research on deterrence theory has focused on the roles of certainty and severity of punishment, with celerity receiving minimal empirical attention (Paternoster, 2010). Early work examined objective deterrence, studying the relationship between particular policies, such as the death penalty, and city- or state-wide crime trends. This work largely concluded that the certainty of punishment had a greater deterrent impact than the severity of punishment (Chiricos & Waldo, 1970; Gibbs, 1968; Logan, 1975). However, these studies were criticized for their methodological limitations (Paternoster, 1987; Saltzman, Paternoster, Waldo, & Chiricos, 1982). Of particular concern was the lack of focus on perceptions of formal punishments given that deterrence theory is at its core a social psychology theory of threat communication (Geerken & Gove, 1977; Paternoster, 2010). Thus, empirical research needed to focus not just on objective measures of the certainty and severity of a formal punishment, but rather the subjective interpretations and perceptions of particular punishments for particular offenses. Later work incorporating the role of perceptions in determining deterrent effects (Erickson, Gibbs, & Jensen, 1977; Jensen, Erickson, & Gibbs, 1978; Waldo & Chiricos,

1972), found continued support for the certainty of punishment, although these effects were much weaker than previous research indicated (Paternoster, 1987). This research was also improved with the introduction of panel data, and these new studies found evidence that prior results were largely the product of an experiential effect in which prior behavior was affecting individual perceptions rather than the hypothesized direction of individual perceptions affecting future behavior (Saltzman et al., 1982; Paternoster, Saltzman, Waldo, & Chiricos, 1983). As a whole, this work finds limited support for deterrence theory in its entirety, although certainty is the most supported of the theory's tenets (Paternoster, 2010). These findings have called into question the validity of the theory's components that focus only on formal sanctions without incorporating additional costs or benefits involved in committing crime. These criticisms led to work incorporating other parts of Becker's (1968) calculus, thus improving the ability of the theory to predict crime (Cornish & Clarke, 1986).

More recent updates to the field, namely the examination of risk updating, provided additional context to the deterrence literature. The risk updating research found that risk perceptions were dynamic and updated based on new offending and punishment experiences (Anwar & Loughran, 2011; Horney & Marshall, 1992; Lochner, 2007; Matsueda et al., 2006). This work stemmed from Bayes' probability theorem and centered on the idea that individuals had subjective perceptions of the probability of an event, such as being arrested, based on the information they had gathered up until that point. As individuals encountered new information, such as being arrested or avoiding arrest, they updated their perception of the risk of that event incorporating and integrating the new information into their prior perceptions (Matsueda et al., 2006; Stafford & Warr,

1993). Research consistently found support for the Bayesian learning model and the dynamic nature of risk perceptions in criminology (Anwar & Loughran, 2011; Horney & Marshall, 1992; Lochner, 2007; Matsueda et al., 2006). For example, Anwar and Loughran (2011) used the Pathways to Desistance dataset to examine risk updating among a sample of serious adolescent offenders and found that an individual's posterior risk perception was 6.3 percent higher if that individual was arrested for one crime committed rather than not arrested (p. 687). These updating studies have also found significant differences among groups of offenders (Loughran, Piquero, Fagan, & Mulvey, 2012), with lower perceived risk of punishment among those who are actively involved in crime (Lochner, 2007), consistent with the theory of deterrence. Studies have also shown that vicarious experiences with punishment and punishment avoidance (Stafford & Warr, 1993) have an impact on risk perceptions (Wilson, Paternoster, & Loughran, 2017). These studies reinforce the role that formal criminal justice system operations, particularly arrests, can play in the updating of risk perceptions.

Aside from specific and general deterrence, there is also a distinction between absolute and restrictive deterrence. Absolute deterrence is the idea that the threat of a sanction completely deters an individual from participating in criminal behavior, whereas restrictive deterrence is a reduction of criminal offending, either in frequency of offending or in seriousness of the types of offenses, but not a complete cessation of criminal behavior as the result of the threat of a sanction (Gibbs, 1968; Paternoster, 1989). Within the realm of specific deterrence, restrictive deterrence can manifest as a greater number of days between the sanction and the next criminal event, highlighting that individuals may be deterred by a sanction without completely ceasing criminal

activity. This distinction has led studies to include multiple indicators of recidivism when evaluating the deterrent effects of criminal justice policies, including frequency of reoffending or time to subsequent offending (for example see: DeJong, 1997; Nagin & Paternoster, 1991), as a way to separate possible deterrent effects of a particular punishment. For example, DeJong (1997) highlighted the potential differences in the impact of an experienced sanction by specifying that “after experiencing a sanction, an offender may never return to criminal behavior (desist), return after a brief period of nonoffending (short-term deterrence), or return after an extended period (long-term deterrence)” (p. 561). DeJong (1997) examined the impact of a custodial sentence on time to a recidivism event, demonstrating the utility of varying measures of deterrence, above and beyond an absolute conception of the theory.

Deterrence theory has been applied to multiple criminal justice system policies (Paternoster, 2010) such as focused police practices (e.g., hot spots policing; see Sherman & Weisburd, 1995), sentencing policy changes (e.g., three strikes laws; see Kovandzic, Sloan, & Vieraitis, 2004), and corrections policies (e.g., supermax prisons; see Mears & Bales, 2009). The current state of the support for the criminal justice system’s role in deterrence theory is tenuous. As highlighted by Paternoster (2010), it is difficult to state with precision the deterrent effect of criminal justice system policies and practices, and, due to the delays inherent in the time between a crime occurring and the imposition of a punishment, the system may not be set up to enact a particular deterrent effect. The general conclusion is that the criminal justice system provides a marginal deterrent effect but that the threat of extra-legal, informal sanctions has a stronger effect (Paternoster, 2010). With regard to the deterrent effect of incarceration specifically, Paternoster (2010)

concludes, "...it is probably very safe to say that the threat of imprisonment does indeed act as a general deterrent and probably is responsible for some share of the recent crime drop" (p. 802). With regard to the role of the correctional system in specific deterrence, Nagin (2013) concludes there is "...little evidence of a specific deterrent effect arising from the experience of imprisonment compared with experience of noncustodial sanctions such as probation" (p. 202). Thus, there are differing conclusions regarding the criminal justice system's ability to affect specific and general deterrence. Although there is a general lack of support in a strong role of deterrence in the criminal justice system, there are additional avenues of punishment that can be examined with a foundation in deterrence theory.

Deterrence Theory and Solitary Confinement as Punishment

The current study applies deterrence theory to punishment practices within a state correctional system. The main justification behind the use of solitary confinement as punishment is that disciplinary custody serves as a specific deterrent against future misconduct; those who are exposed to isolation should abstain from committing future misconduct to prevent return to that more severe environment (Lucas & Jones, 2017; Woo et al., 2019). Within correctional settings, isolation is discussed as both a specific deterrent (Mears & Bales, 2009; Morris, 2016) and a general deterrent (Naday et al., 2008; Pizarro & Stenius, 2004). General deterrence is more often discussed as a potential benefit of supermax prisons in that individuals in the general prison population or in a lower security facility will avoid committing violent or severe rule violations to avoid being sent to a supermax facility (Mears, 2006; Mears & Castro, 2006; Pizarro & Stenius, 2004), and specific deterrence is cited as a justification for both supermax prisons

(Mears, 2006) and disciplinary custody specifically (Lucas & Jones, 2017). For example, as part of a project identifying the metrics to evaluate the effectiveness of supermax prisons, Mears (2006) conducted site visits to multiple states. One site visit was to Maryland's former supermax facility (Maryland Correctional Adjustment Center or MCAC) in which multiple individuals were interviewed. Mears (2006) states:

Several respondents relayed that another goal of the prison is to change MCAC prisoners' behavior and to return them to the maximum-security prison. One respondent explained that deprivations (e.g., less freedom, fewer privileges, and hindrance of family involvement) help the prisoner better understand the costs of committing infractions and may reduce the likelihood that he would commit infractions in the future (Mears, 2006, p. 17).

Prior to a discussion of deterrence theory specifically, it is necessary to acknowledge that the context for the use of solitary confinement is larger than just the individual. Prison administrators and management likely also consider the collective of the larger prison environment, prioritizing the use of solitary confinement as punishment to provide general deterrence to others within the prison system or as a method of incapacitation to limit the misconduct opportunities of those placed within isolation (Mears, 2006). However, the current focus is on the most common justification given for placing individuals within disciplinary custody following a misconduct offense (Lucas & Jones, 2017), that of specific deterrence. It is important to acknowledge the larger context and the possibility of other theoretical considerations and justifications for the broader use of solitary confinement.

The three components of deterrence theory (certainty, severity, and celerity) can be applied to the concept of disciplinary custody as a punishment for an infraction within prison. Within the confines of prison, inmate misconduct is likely to be identified and reported, resulting in a high certainty of detection and subsequent punishment (Medrano

et al., 2017). Of course, this could differ if specific infraction types are frequently ignored or enforced only for specific prisoners, resulting in differential levels of the certainty of punishment (Gaes & McGuire, 1985; Haggerty & Bucerius, 2020). Once a correctional officer or staff member has officially reported a misconduct incident, there are multiple potential punishments one can receive as a result, with disciplinary custody serving as one of the most severe options.⁴ Although there are mandated limits to how many days an individual can be placed in disciplinary custody as a result of a single count of an infraction (90 days within PADO), individuals committing multiple infractions in one event can be sentenced to weeks, months, and even years of disciplinary custody. Longer stays in disciplinary custody or longer requirements for other punishments (such as the removal of benefits for a specified number of days) create differences in the severity of the punishment. Additionally, punishments within prisons are typically swift, particularly when compared to sentencing within criminal courts. Within PADO, inmates are served notice of the misconduct charges within 24 hours of the filing of an official report, and hearings and sanctions take place within days or weeks of an infraction.

Punishment practices within prisons could be expected to affect risk perceptions of prisoners just as criminal justice policies affect perceptions of offenders in the real world. Prisoners experience various punishments as a result of committing infractions and likely update their perceptions of risk of receiving particular punishments. These experiences and the experiences of other prisoners around them likely alter future behavior due to these changes in perceptions. Additionally, punishment avoidance likely has an impact on perceptions within prisons (Stafford & Warr, 1993) if individuals are

⁴ Other punishments such as revoking pre-release status are also severe options available to a hearing examiner.

committing minor infractions which go unnoticed or are ignored by correctional officers. The mapping of the components of deterrence theory onto disciplinary custody punishments allows for the testing of variations in those components and the specific deterrent effect of this punishment.

Empirical Evidence on the Deterrent Effect of Disciplinary Custody

Harsher Prison Conditions and Post-Exposure Behavior

Overall, there is no conclusive evidence supporting the argument that solitary confinement or harsher prison conditions improve behavior either within prison or post-release (Bench & Allen, 2003; Chen & Shapiro, 2007; Gaes & Camp, 2009; Labrecque, 2015; Labrecque & Smith, 2019; Lovell et al., 2007; Lucas & Jones, 2017; Mears & Bales, 2009; Medrano et al., 2017; Morris, 2016; Tahamont, 2019; Woo et al., 2019). There are a few studies which explicitly test the deterrent effect of short-term disciplinary custody stays on subsequent prison behaviors, but there is a larger group of studies more broadly examining the deterrent effects of harsher prison conditions on behaviors both prior to release from prison (i.e. misconduct) and recidivism post-release from prison. These studies can help inform the state of knowledge of the deterrent effect of short-term solitary confinement both within and outside of prison. I will first outline the results of this research for recidivism and misconduct and then highlight the studies that directly examined the deterrent impact of disciplinary custody specifically.

The difficulty in isolating the effect of harsher prison conditions on subsequent behavior is that the individuals placed in these conditions (such as higher security level

facilities) likely have exhibited behavior indicative of higher risk and are more likely to commit similar behavior in the future. This leads researchers to leverage various methods to identify the appropriate comparison group in order to isolate the treatment effect of the harsher conditions themselves (Mears & Bales, 2009). The majority of the research examining harsher prison conditions and recidivism has used experiments (Bench & Allen, 2003; Gaes & Camp, 2009) or borderline cases in security classification (e.g., regression discontinuity) (Berk & de Leeuw, 1999; Chen & Shapiro, 2007; Tahamont, 2019) to examine if those housed in higher security environments are deterred from future misconduct or recidivism. The argument is that those individuals just on either side of a cutoff for security placement are substantively similar enough that they should have a shared baseline risk for future offending prior to exposure to differential prison conditions. For example, Chen and Shapiro (2007) used regression discontinuity in security level assignment in a sample of federal inmates and found that those housed in higher security levels were not less likely to recidivate relative to those placed in lower levels. Their models suggested that the higher-placed inmates were actually more likely to recidivate (Chen & Shapiro, 2007). A study conducted by Gaes and Camp (2009) randomly assigned prison security levels to a sample of California inmates and found that Level III inmates assigned to Level III prisons had a higher likelihood of re-admittance to prison than the Level III inmates assigned to Level I facilities.

A particular branch of this work focuses exclusively on supermax facilities and the role these prisons have on the recidivism of individuals released (Lovell et al., 2007; Mears & Bales, 2009). While disciplinary custody is most frequently discussed as a specific deterrent for within prison behavior, stated goals of supermax prisons include

improving behavior both within the system and post-release (Mears, 2006; Mears & Castro, 2006). For example, Lovell et al. (2007) used a retrospective matched control design to match prisoners from supermax and non-supermax facilities in Washington State on factors such as race, age, prior criminal history, and inmate misconduct, and found that supermax prisoners committed felonies at a higher rate post-release than the controls, but the difference was not statistically significant. Overall, this research provides some evidence that those housed in higher security environments are actually more likely to recidivate post-release (Chen & Shapiro, 2007; Gaes & Camp, 2009; Mears & Bales, 2009), the opposite effect than that predicted by deterrence theory.

Other research more pertinent to the current study examines the impact of harsher prison conditions on in-prison behaviors, namely misconduct. For example, Bench and Allen (2003) conducted an experiment in which they randomized assignment to maximum and medium security for a group of Utah inmates originally classified as maximum security and found no significant differences in misconduct between the two groups. The authors suggest that these findings indicate a labeling effect of higher security environments rather than differences in the individuals housed in these environments. A similar experiment conducted by Gaes and Camp (2009) with a California sample also found no differences in institutional misconduct rates between those who were originally classified as Level III (higher security) but were randomly assigned to Level III or Level I prisons. Lastly, Tahamont (2019) exploited the borderline cases in security assignment in California prisons using a fuzzy regression discontinuity design and found that those placed in Level II institutions had higher rates of official misconduct reports compared to individuals in Level III institutions. These higher rates

were largely driven by non-serious misconduct such as bartering (Tahamont, 2019). However, the author did not find any effect at the cutoff between close security (Level III) and maximum security (Level IV). Although this research does not test the role of isolation specifically, higher security environments are more similar to solitary confinement experiences than lower levels of security, particularly with the degree of program availability and the amount of freedom allotted to a prisoner in moving about the facility. These studies speak to the deterrent capabilities of more severe prison environments, but other empirical studies directly testing the deterrent capabilities of short-term disciplinary custody stays are directly applicable to the current study.

Disciplinary Custody and In-Prison Behavior

Early qualitative work examining the role of short-term isolation provides some evidence of the role that disciplinary custody can play in changing inmate behaviors. Suedfeld and Roy (1975) conducted a case study of four inmates who were placed in isolation for participating in the same infraction within a Canadian prison. The authors reported that all four individuals were “better adjusted and posed fewer behavioral problems” (Suedfeld & Roy, 1975, p. 96) after they were released from isolation. One of the four even repeatedly requested to be returned to isolation. Suedfeld and Roy (1975) concluded that isolation can “produce adaptation to the supervised institutional environment” (p. 97). Suedfeld, Ramirez, Deaton, and Baker-Brown (1982) conducted two studies in five prisons in the United States and Canada consisting of interviews and questionnaires from volunteer participants, some of whom had experienced time in solitary confinement units. Although the authors focused largely on the effect of solitary confinement on the mental and physical health of inmates, one of the studies interviewing

12 male inmates from one U.S. institution and two Canadian institutions briefly touched on the use of solitary confinement to produce lasting behavioral changes. The subjects in this study largely felt that solitary confinement “as a punitive technique did not produce many lasting positive changes in behaviors or attitudes, and that as a method for controlling prisoners it is not as effective as withdrawal of privileges might be” (Suedfeld et al., 1982, p. 318).

The quantitative research examining the effect of solitary confinement on subsequent misconduct can be divided into two groups: the studies that examine exposure to solitary confinement and misconduct as a result, and the studies that examine the length of time spent in solitary confinement on subsequent misconduct. The quantitative studies examining exposure to disciplinary custody and subsequent misconduct have focused on punishment practices within a single state,⁵ comparing outcomes for individuals sentenced to disciplinary custody for an infraction to those who did not receive disciplinary custody as a result of their infraction (Labrecque, 2015; Medrano et al., 2017; Morris, 2016). For example, Labrecque (2015) examined longitudinal data on a sample of 14,311 prisoners in Ohio who were incarcerated for at least a year and served some time in disciplinary custody between July 1, 2007 and June 30, 2010. Labrecque (2015) used three-month waves to examine the effects of whether or not an individual had been in solitary confinement in the prior wave on inmate misconduct in the current wave. Utilizing a hybrid random effects model, this study found that solitary confinement in the prior wave did not have an effect on the prevalence or incidence of violent, non-violent, or drug misconducts in the subsequent wave (Labrecque, 2015). Although there

⁵ Due to differences in punishment practices across state systems, limiting study to an individual state system allows for more confidence in the estimation of effects within a single location rather than the average of effects across states with varying practices.

was not an effect overall, the author did find differences by various inmate characteristics (i.e., treatment effect heterogeneity). For instance, female prisoners experiencing disciplinary custody in the prior wave had about a 21% decrease in the probability of violent misconduct whereas individuals with serious mental health issues had higher likelihoods of both nonviolent and drug misconducts following an experience in solitary confinement. There were also offense-specific results, with the experience of solitary confinement for inmates who were incarcerated for a drug offense resulting in a 31.8% decrease in the probability for violent misconduct compared to those sentenced for a property or other nonviolent offense (Labrecque, 2015). The author concludes by stating, “The most important finding in this study is the lack of evidence of any effect of solitary confinement on subsequent inmate misconduct” (Labrecque, 2015, p.112).

The other studies examining this research question reached similar conclusions. Morris (2016) examined the role of short-term solitary confinement for individuals found guilty of a violent infraction within their first two years incarcerated in a large southern state (n=3,808 individuals within 70 prison units). Utilizing a multi-level propensity score matching design, Morris (2016) matched those who received solitary confinement following the first violent infraction to those who did not on individual characteristics, such as educational level and sentence length, and prison characteristics such as average inmate IQ and the average inmate age within the unit. Morris (2016) found that solitary confinement as a punishment for a first-time violent infraction did not increase or decrease the probability or timing of a subsequent violent infraction, and disciplinary custody did not predict membership within any misconduct developmental trajectory classifications (Morris, 2016). The author concluded by stating, “The findings are

relatively robust in suggesting that on average, the initial experience with solitary confinement alone (i.e. as a direct and independent effect) may not play a causal role in subsequent physical violence, its timing, or its downstream effect” (Morris, 2016, p. 17). Less rigorous studies using subsets of incarcerated individuals (Medrano et al. (2017) examined the effect of solitary confinement among capital inmates) or severely restricted samples (due to missing data, Lucas and Jones (2017) were only able to examine a sample of 228 inmates) reached similar conclusions on the lack of a deterrent effect for the experience of disciplinary custody as a whole.

Additional quantitative studies of interest examine the length of time spent in a solitary confinement environment and the effect of that length of time on subsequent misconduct. This work began to address the issues of examining treatment heterogeneity in the solitary confinement experience and how that heterogeneity can impact subsequent infractions.⁶ First, Labrecque (2015), in addition to examining the role of exposure to disciplinary custody on misconduct (see above), also examined the role of the number of days spent in disciplinary custody in the previous three-month wave on the prevalence and incidence of misconduct in the current wave. As with his findings regarding exposure, Labrecque (2015) found that the length of time spent in isolation did not have any effect on misconducts in the subsequent wave, regardless of the type of misconduct examined.

⁶ Prior studies have examined differences in treatment effect heterogeneity, an important avenue to explore. For example, Labrecque (2015) examined how the experience of solitary confinement differed by gender, mental health status, and incarceration offense. Medrano et al. (2017) took an important step in examining the effect of disciplinary custody among capital inmates; however, without the comparison to other types of inmates, the contribution of this study to understanding treatment effect heterogeneity is limited. Although there are additional areas of treatment effect heterogeneity to examine, the current study focuses solely on differences in treatment heterogeneity. Different levels of treatment can alter the outcomes of that treatment among the same groups of people and this should be examined prior to additional avenues of effect heterogeneity.

In another study, Labrecque and Smith (2019) examined a cohort of male inmates who spent at least one day in solitary confinement during their first year incarcerated and examined subsequent violent and nonviolent misconduct. Utilizing a multivariate logistic regression, the study found there was no influence of time spent segregated on later infractions in the year follow-up period. However, this study did find that the length of time spent in isolation had a negative (albeit marginal) effect on subsequent placement in restrictive housing. This demonstrates the importance of examining multiple indicators of follow-up behavior. Despite this marginal finding, the author concluded, “placing men in restrictive housing confinement for longer durations does not lead to great improvements in their institutional adjustment” (Labrecque & Smith, 2019; p. 9). Importantly, however, this study did not parcel out different types of segregation, instead combining all types of restricted housing into one indicator. In other words, it examined the deterrent effect of types of housing that *are not meant to deter future rule-breaking*. For example, individuals may be placed in administrative segregation/custody for suicide watch or protection from other inmates; in such circumstances, there is no expectation of a deterrent effect on future misconduct. Grouping all types of restrictive housing together in this way could cloud deterrent effects as different types of restrictive housing would be expected to have varying effects on later behavior.

Lastly, Woo and colleagues (2019) examined the impact of time spent in disciplinary segregation on behavior both within prison and post-release among prisoners in Washington State. The first sample examining the deterrent capabilities of disciplinary segregation within prison included all individuals incarcerated at any point between August 2008 and March 2016 who had a violent infraction at some point during their stay

(n=3,144).⁷ Six-month time intervals were applied for each individual from March 31, 2013 to September 30, 2015. This study found no significant differences in violent infractions in the four six-month follow-up intervals. The number of days spent in disciplinary segregation also did not have a statistically significant impact in subsequent periods. Woo et al. (2019) also examined the role of disciplinary segregation on recidivism post-release with a sample of 838 offenders who had experienced disciplinary custody during the study period who were then matched using three-to-one nearest neighbor matching to the rest of the sample. Similarly to the findings as it related to in-prison behavior, there were no statistically significant differences in conviction rates between those who had experienced disciplinary segregation and those that had not in the three-year follow-up period (Woo et al., 2019).

Together, the first group of studies concluded that exposure to disciplinary custody, without accounting for heterogeneity in that treatment, does not have a deterrent effect on ensuing misconduct (Lucas & Jones, 2017; Medrano et al., 2017; Morris, 2016). The second group of studies examined one component of heterogeneity in solitary confinement—length of stay—and concluded a lack of a deterrent capability as well (Labrecque, 2015; Labrecque & Smith, 2019; Woo et al., 2019). There are important differences in the samples and outcomes examined in these studies. For example, Morris (2016) and Woo and colleagues (2019) only examined receiving solitary confinement as a punishment for the first reported act of violence and subsequent violence. Moreover, although other studies focused specifically on disciplinary custody, Labrecque and Smith (2019) grouped all restrictive housing together.

⁷ Woo et al. (2019) excluded all prisoners identified as having a mental health disorder due to the potential decreased ability of these individuals to make rational choices in line with the tenets of deterrence theory.

Early work examining disciplinary custody as a monolithic event could suffer from aggregation bias, clouding the true deterrent effect, and limiting our understanding of the utility of this punishment. The combination of all disciplinary custody experiences into one dummy classification could lead to the lack of a deterrent finding due to the grouping of those for whom there was a deterrent effect of the punishment and those for whom there was not. For example, if only longer stays within disciplinary custody produce noticeable deterrent effects, then inclusion of those with shorter stays of only a few days would produce a null finding. Subsequent work examining one aspect of treatment heterogeneity, length of stay, was an important addition to the literature. Woo et al.'s (2019) study of length of stay and misconduct is the most significant contribution to examining the role of heterogeneity in the disciplinary custody sanction. However, important limitations and avenues for further study necessitate additional research building off this study. First, Woo and colleagues (2019) only examined disciplinary custody as a result of a violent misconduct and subsequent violence, ignoring other types of misconduct. Disciplinary custody is used as a punishment for all forms of misconduct and would be expected to deter all types of misconduct, not just violence. It is possible that the more common misconduct types such as property or prison order offenses are more likely to be deterred by differences in a disciplinary custody stay than the less frequent violent offenses. Pennsylvania also has differing policies from Washington State, which could impact results, namely that disciplinary custody stays are capped at 90 days per charge in Pennsylvania compared to 30 days in Washington. It is possible that this lack of variation in length of disciplinary custody stays in the study site contributed to their null findings (Woo et al., 2019). Lastly, Woo et al. (2019) excluded individuals

with mental illnesses due to the impact these issues could have on rational decision making. However, disciplinary custody is meant to deter offending among all inmates, not just those without mental health issues, and excluding these individuals could impact results. Without a more inclusive examination of the effect of disciplinary custody on future behavior, there cannot be a complete conclusion on the presence or absence of a specific deterrent effect of this punishment.

Present Study

The current study addresses the limitations of prior work by focusing on treatment heterogeneity in the disciplinary custody experience. In particular, I examine the impact of two aspects of heterogeneity in disciplinary custody: the length of time spent in disciplinary custody and early release from disciplinary custody. For the length of time spent in disciplinary custody, deterrence theory specifies that more severe sanctions will deter future crime (Beccaria, 1764). The premise is that longer punishments are perceived as more severe, thus increasing the costs associated with committing a subsequent offense that could result in that sanction (Meade, Steiner, Makarios, & Travis, 2012). Longer lengths of stay in disciplinary custody contribute to the severity of that punishment. The lack of personal contact with others, limited physical mobility, and suspension of programming or job duties contribute to a monotonous routine devoid of some of the daily distractions available to the general prison population (Reiter, 2012). Additional days or even weeks within that environment increase the costs associated with committing an offense. Theoretically, these increased perceived costs should result in a greater desire to avoid the types of activities that led to the original punishment.

Alternatively, defiance theory would hypothesize that longer sanctions could result in a defiant reaction if the longer stays are perceived as unjust or excessive (Sherman, 1993). According to Sherman's (1993) defiance theory, punishments perceived as fair or legitimate are more likely to lead to deterrent effects. Sherman (1993) highlights that one way a sanction can be perceived as unfair is if it is "substantially arbitrary, discriminatory, excessive, undeserved, or otherwise objectively unjust" (p. 461). Long stays in disciplinary custody, particularly among those individuals who receive more than 90 days due to multiple charges within the same misconduct event, may be perceived as overly harsh or undeserved. Examining length of stay is a particularly policy-relevant research question; for example, if it can be shown that short stays within disciplinary custody can provide the same behavioral outcomes as longer, more expensive and potentially detrimental stays, steps can be taken to further limit the maximum number of days permissible per misconduct charge.

As with the first group of studies examining disciplinary custody and deterrence discussed previously, originating work on incarceration and recidivism focused solely on the experience of incarceration as a whole, without due attention to the many sources of heterogeneity in the incarceration experience (Mears et al., 2015; Travis et al., 2014). This reasoning has led to multiple studies attempting to identify the deterrent effect of longer incarceration sentences (Loughran, Mulvey, Schubert, Fagan, Piquero, & Losoya, 2009; Meade et al., 2012; Rydberg & Clark, 2016; Snodgrass, Blokland, Haviland, Nieuwebeerta, & Nagin, 2011). For example, Snodgrass et al. (2011) utilized a sample of Netherlands offenders sentenced to incarceration and utilized optimal nonbipartite matching to match across offenders receiving different levels of incarceration on

characteristics such as sex, age, and trajectory group membership. This allowed for comparison between groups with differing levels of treatment, with the most common matches coming from one individual with a sentence of 6 to 12 months compared to an individual sentenced for 12 or more months. This study failed to find an effect for varying sentence lengths on 3-year recidivism (Snodgrass et al., 2011).

Similarly, a disciplinary custody sanction can result in a range of punishment lengths, which could in turn, affect risk perceptions. There are restrictions to the length of a disciplinary custody sanction, and one count of an infraction can be subjected to between 1 and 90 days in disciplinary custody. Additional counts for the same misconduct event can add additional variation to a length of stay. Previous research finding a null effect of disciplinary custody on misconduct aggregated all sanction lengths by focusing solely on whether or not an individual experienced solitary confinement regardless of the number of days spent in isolation (Medrano et al., 2017; Morris, 2016), thus potentially masking a deterrent effect for longer time spent in disciplinary custody. Subsequent research examining length of stay has fundamental limitations, such as the grouping of all types of isolation together (Labrecque & Smith, 2019) or deserves elaboration, such as extending research beyond just violence (Woo et al., 2019).

Another source of heterogeneity with potential recidivism consequences is the possibility of early release. Within the PADO system, once an individual has served part (most often half) of their disciplinary custody sanction for the majority of offenses,⁸ they are eligible for release back to the general prison population. Individuals are made

⁸ Fourteen particularly serious offenses such as assault, rape, or arson are not subject to early release from disciplinary custody, and these offense types are excluded from analyses examining early release.

aware of the possibility of early release at the time of the sanctioning hearing. Similar to a parole hearing, a committee within PADOH known as the Program Review Committee (PRC), meets, evaluates the misconduct case and the individual's behavior while in disciplinary custody, and determines whether that individual should serve their full disciplinary custody term or return to the general population prior to the completion of their sanction length.

This early release mechanism could have an impact on individual risk perceptions (Anwar & Loughran, 2011; Lochner, 2007; Matsueda et al., 2006). Research examining criminal and punishment experiences has found consistent support for individuals updating their perceived risk of committing criminal acts based off their personal experiences (Anwar & Loughran, 2011; Lochner, 2007; Matsueda et al., 2006). This is particularly related to Stafford and Warr's (1993) conception of punishment avoidance as a factor contributing to individual perceptions. An individual released earlier than their original sentence is aware of exactly how many days of additional punishment they have avoided. Serving less time than originally expected could shift the perception of disciplinary custody to not as detrimental as expected, thus lessening the perceived severity and risk involved with that punishment. This could lead to a smaller deterrent effect among individuals released from disciplinary custody early.

Alternatively, individuals released from disciplinary custody prior to completion of their initial sentence length may look more favorably on the punishment system as a whole, perceiving it to be more fair and just. According to Sherman's (1993) defiance theory, punishments perceived as fair or legitimate are more likely to lead to deterrent effects. Specifically, Sherman (1993) states, "Sanctions produce future deterrence of law-

breaking (desistance, less frequent or less serious violations) to the extent that offenders experience sanctioning conduct as legitimate...” (p. 448). This could particularly be the case given the sensory and social deprivations associated with an extended stay in disciplinary custody. Sherman (1993) highlights that one way a sanction can be perceived as unfair is if it is “substantially arbitrary, discriminatory, excessive, undeserved, or otherwise objectively unjust” (p. 461). Recent work has also applied the work of legitimacy and procedural justice to corrections systems, highlighting the opportunities for promoting legitimacy within prison (Brunton-Smith & McCarthy, 2016; Tyler, 2010). For example, using data on prisoners in England and Wales, Brunton-Smith and McCarthy (2016) found that opinions on the degree to which fair procedures were in place for expressing views, challenging wrongful decisions, and assigning/removing privileges contributed to individual perceptions of staff legitimacy. This research highlights the potential for formal procedures, such as the early release mechanism, to contribute to perceptions of the fairness or legitimacy of the system. Early release from disciplinary custody could result in perceptions of the sanction as more fair or less excessive, altering how individuals react to such punishment and could result in a greater deterrent effect for that individual. Prior research has not differentiated between those who serve their full disciplinary sanction length and those who are released early, thus potentially masking differences in a deterrent effect between these populations.

An area of research that also informs this discussion is the expectancy disconfirmation model that originated in consumer satisfaction research (Brown et al., 2008; Oliver, 1980; Van Ryzin, 2013). The expectancy disconfirmation model is rooted in the premise that individuals have expectations about products or services that are then

confirmed or disconfirmed by the actual performance or the service itself (Oliver, 1980). Individuals can experience negative disconfirmation in which his/her expectations were not met by the service, decreasing the satisfaction associated with that service. On the other hand, individuals can experience positive disconfirmation in which expectations are exceeded, increasing satisfaction with the service. Reisig and Chandek (2001) applied this model to criminal justice operations in the realm of police-citizen encounters, examining the role of expectations using random probability samples of individuals who had recent contact with police. The authors found the expectancy disconfirmation model was applicable in understanding satisfaction for both voluntary and involuntary encounters (Reisig & Chandek, 2001). Within the realm of early release from disciplinary custody, the expectancy disconfirmation model would support the predictions of defiance theory in that prisoners whose expectations are to serve the full sanction length and are then released early have those expectations disconfirmed in a positive direction. Although individuals are unlikely to be satisfied with the experience of disciplinary custody, it is possible that those released earlier would have a better outlook on the misconduct sanctioning system as a whole.

Specific deterrence can manifest itself in multiple ways such as a complete cessation of criminal activities or a delayed or more infrequent return to criminal activities (DeJong, 1997; Paternoster, 1989), also known as the difference between absolute versus restrictive deterrence. If disciplinary custody has a specific deterrent effect, there should be a longer time period between the end of the sanction and another misconduct incident (DeJong, 1997). It is also possible that the experience of a sanction such as disciplinary custody has an immediate deterrent effect that then fades as

individuals commit additional misconducts without getting caught or as the memories of the time spent in disciplinary custody fades. Examining the time to subsequent misconduct is an important way to examine the deterrent capabilities of a punishment, as it is possible that both restrictive and absolute deterrence are at work and that short-term deterrent effects are masked in prior research with long follow-up periods that do not account for time to subsequent misconduct. Other areas of deterrence research, namely work in the field of terrorism, have relied on hazard models to examine this aspect of deterrence, namely the deterrent effects of various government interventions and how an intervention can impact the hazard rate of subsequent terrorist acts (Dugan, LaFree, & Piquero, 2005; LaFree, Dugan, Korte, 2009).

It is important to caveat the limitations of this study in the examination of the components of deterrence theory. Deterrence theory is at its root a perceptual theory (Paternoster, 2010). As stated by Nagin (2013), “Deterrence is the behavioral response to the perception of sanction threats” (p. 204). However, in the absence of data on perceptions, research has relied extensively on the examination of formal sanctions and changes in policy in sentencing and corrections (Kovandzic et al., 2014; Mears & Bales, 2009). This is an issue for the field at large as Nagin (2013) states, “Establishing the link between risk perceptions and sanction regimes is imperative; the conclusion that crime decisions are affected by sanction risk perceptions is not sufficient to conclude that policy can deter crime” (p. 204). Due to the lack of data on individual perceptions of disciplinary custody and on how treatment heterogeneity in disciplinary custody could shape such perceptions, I must rely on official sanctioning data to examine the research questions posited here. As this is the case, any results found in support of deterrence

theory must be taken with caution, and subsequent research examining the role of perceptions must be undertaken to further examine the mechanism behind any findings. However, regardless of perceptions, correctional staff use official data to make policy decisions, and analyses examining the relationship between formal sanctioning within prisons and subsequent behavior are of interest and important to practitioners.

These questions lead to six main hypotheses drawn from deterrence theory. As discussed above, there are competing theories that would posit opposite results of differences in the severity of disciplinary custody stays, both for length of stay or the proportion of the original sanction served. However, the hypotheses for the current study are constructed under the guide of specific deterrence. The focus of this paper is on examining the specific deterrent capabilities of disciplinary custody and thus, this perspective informs and shapes these hypotheses.

Hypothesis 1: Longer lengths of stay in disciplinary custody will be associated with a larger specific deterrent effect, measured as a lower likelihood of subsequent misconduct.

Hypothesis 2: Longer lengths of stay in disciplinary custody will be associated with a larger specific deterrent effect, measured as a greater number of days to the next misconduct incident.

Hypothesis 3a: Early release from disciplinary custody will be associated with a smaller specific deterrent effect, measured as a higher likelihood of subsequent misconduct.

Hypothesis 3b: Serving a lower proportion of the original disciplinary custody sanction length will be associated with a smaller specific deterrent effect, measured as a higher likelihood of subsequent misconduct.

Hypothesis 4a: Early release from disciplinary custody will be associated with a smaller specific deterrent effect, measured as a fewer number of days to the next misconduct incident.

Hypothesis 4b: Serving a lower proportion of the original disciplinary custody sanction length will be associated with a smaller specific deterrent effect, measured as a fewer number of days to the next misconduct incident.

Chapter 3: Methods

Data

The current study examines the deterrent effect of disciplinary custody sanctioning using data from a large state prison system. The data were gathered directly from the Pennsylvania Department of Corrections (PADOC) and contain information on the full misconduct histories of all individuals present in Pennsylvania state prisons between January 2012 and December 2014. Pennsylvania is the seventh largest state prison system in the country with approximately 50,000 inmates in custody at any given time. The system houses two Diagnostic and Classification centers, one for each gender, where each inmate brought into PADOC undergoes programmatic and needs assessment, is given a custody level, and assigned to a permanent institution. Of the 27 institutions open during the three-year study period, two housed female offenders and 25 housed male offenders.⁹ Misconduct hearings and sanctioning practices are decentralized at the facility level, with each institution holding separate misconduct hearings overseen by one or more hearing examiners. Hearing examiners act as the judge in misconduct proceedings and are solely responsible for assessing the facts of the misconduct, determining guilt, and delivering sanctions. The legal threshold to issue a finding of guilty within a misconduct hearing is a preponderance of evidence (PADOC, 2015). All facilities have a separate disciplinary custody unit in which individuals sentenced to this

⁹ During the study period, two facilities closed and another facility opened so only 24 institutions were open throughout the entire study period.

punishment serve their term before returning to the general population within that facility. When a misconduct incident is recorded it is given a unique misconduct number. Each misconduct number has an associated inmate and a series of charges and sanctions that describe and accompany the incident. One hearing occurs for each misconduct incident in which there may be multiple charges and sanctions imposed. For disciplinary custody sanctions an individual can receive up to 90 days in isolation per each misconduct charge. However, multiple misconduct charges within the same misconduct event can result in disciplinary custody sanction lengths of months and even years. Within disciplinary custody stays, there is the opportunity for early release back into the general population. Other than a few specific offense types, discussed further below, all individuals sentenced to disciplinary custody are eligible for release from the disciplinary custody unit prior to serving the full length assigned by the hearing examiners.

Full misconduct histories and demographic information were obtained for all inmates housed within PADOCC at any point between January 2012 and December 2014 (N=93,536). Misconduct history data included all official reports of misconduct and the case processing outcomes for each infraction. Data for each misconduct incident contained the charges, plea, verdict, sanction imposed, and sanction length. The data were restricted to a much smaller sample due to the parameters of the current study. Figure 1 outlines the steps taken in restricting the original sample pulled from PADOCC. First the sample of all individuals present in PADOCC facilities between 2012 and 2014 (N=93,536) was restricted to only those individuals admitted to PADOCC for a new court admission for the first time between 2012 and 2014 (N=25,690).¹⁰ Data were restricted to

¹⁰ Individuals were identified as incarcerated in prison for the first time within the state of Pennsylvania if they were identified as a “court commitment,” “county transfer,” “detentioner,” or “federal commitment.”

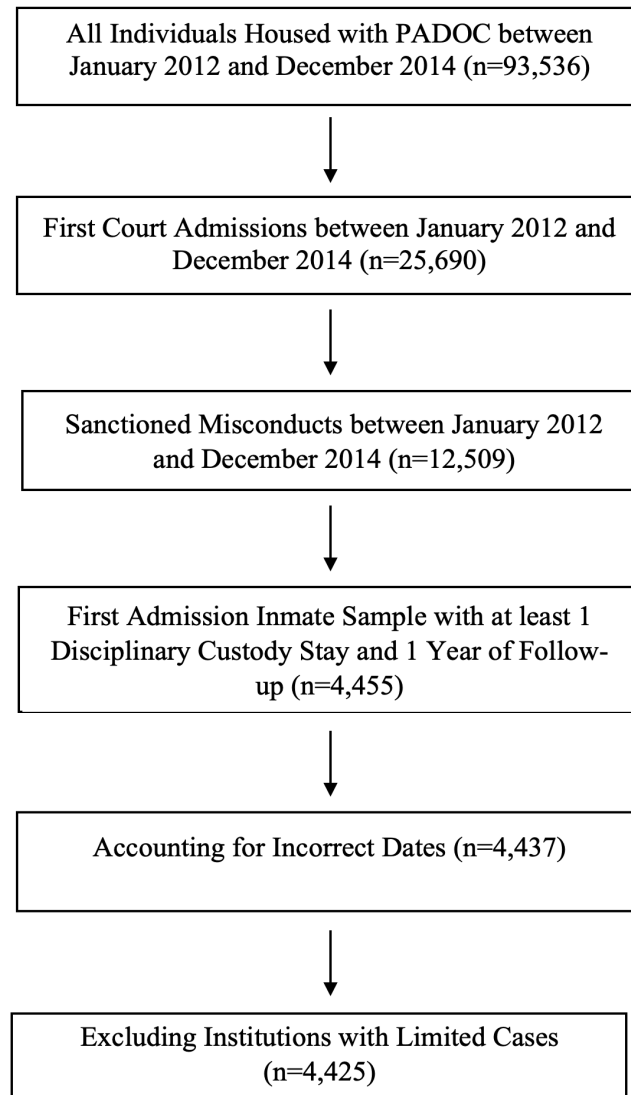
those admitted to PADOCC for the first time to account for the possibility of prior incarceration and misconduct sanctioning experiences that could impact within-prison behavior. This decision was made to isolate the deterrent impact of a first disciplinary custody stay among those incarcerated for the first time to eliminate potential confounding factors stemming from prior experiences. Next, the sample was restricted to only those who had sanctioned misconducts (N=12,509). Finally, the sample was restricted to only those who had at least one disciplinary custody sanction to allow for the examination of heterogeneity in that sanction and was restricted to only those with one full year of follow-up after release from disciplinary custody (N=4,455). After the sample restrictions were accounted for, one issue of incorrect dates led to the dropping of an additional 18 individuals. For these rows, the individual had a sanction completion date that came prior to the date the individual was admitted to disciplinary custody (N=4,437). Lastly, initial analyses attempting to include individuals housed in all PADOCC institutions were unsuccessful, due to the few number of cases from three specific institutions. Two institutions, Cresson and Greensburg, closed during the study period (both in June of 2013), and very few individuals in the dataset were housed in these institutions (3 in Cresson and 2 in Greensburg). Due to the limited number of cases from these institutions and the perfect predictability of the early release treatment (of those eligible for early release in these institutions, no individuals were released prior to serving 100% of their disciplinary custody sanction), these cases were removed from

Those whose first incarceration was due to a violation of a prior probation sentence were excluded from this definition due to the possible confounding effects of an individual entering a state facility as a result of a type of recidivism or failure to meet the requirements of his/her probation sentence. Because only Pennsylvania state data was used for the current project, it is possible that individuals within this sample had been incarcerated in another state or in the federal system prior to the current incarceration. Additionally, county jail data was not available and individuals could have a wide array of prior experiences in jail not accounted for here.

analyses. Additionally, 7 cases were housed in the Quehanna institution, PADOc's only boot camp facility in which inmates undergo a six-month disciplinary and training boot camp program. Due to the differences between this institution and the remaining PADOc facilities, and the low number of individuals in the sample, this institution was also removed from analyses. After these final sample restrictions, the final disciplinary custody analysis file contained 4,425 individuals.¹¹ This sample contains misconduct data through August 2017 to allow for an extended follow-up period (i.e., up to 5.5 years for those released from their first disciplinary custody stay at the beginning of 2012).

¹¹ A subset of the sample was released from their first disciplinary custody stay directly to administrative custody, another restricted housing unit. Although administrative custody would provide a more restrictive environment than release back to the general prison population, it is still possible to commit additional misconducts within that environment as well as once that individual is eventually released back to the general population. It is also possible for individuals to move in and out of restrictive or therapeutic housing throughout the study period. However, none of these more restrictive environments is exempt from misconduct opportunities. These individuals were included in all analyses.

Figure 1. Sample Restrictions



Exclusion Criteria and Implications

The descriptive statistics for the final sample are provided in Table 1 and described in depth below. However, it is important to discuss the descriptive differences between the original data pull of 93,536 incarcerated individuals and the final sample of 4,425 individuals. Due to the exclusion criteria described above, there are several notable

differences between the two samples, particularly in race, marital status, custody level, and incarceration offense. First, the original sample had a higher proportion of prisoners identified as White (42.6% compared to 32.1% of the final sample) and a lower proportion identified as Black (45.6% compared to 56.1% of the final sample). Additionally, the proportion of individuals identified with a single marital status was smaller in the original sample (74.7% compared to 86.0% in the final sample). There is also a noticeable difference in custody level in that the original data pull had 27.9% of prisoners classified as Level 1 or 2 (the lowest possible classifications) compared to only 5.6% in the disciplinary custody restricted sample. Lastly, 21.3% of individuals in the original data pull had been incarcerated for a drug offense relative to 12.7% for the restricted sample. Some of these differences, particularly in custody level and incarceration offense are to be expected because individuals committing misconducts and sanctioned to disciplinary custody would be expected to be a more risky sample relative to all individuals present within PADOX facilities at a given time.

The differences between these two samples highlight the changes created by the exclusion criteria. Although the decisions made in creating the sample of the current study were necessary to isolate deterrent effects and minimize other influences (such as prior incarceration or disciplinary custody stays), these decisions have a large impact on who is included in this study and to whom the findings can be generalized. The current study only concerns first-time incarcerated persons who break prison rules and are subjected to one of the harshest sanction options as a result. Additionally, the current results pertain only to the outcome of an individual's first disciplinary custody stay within their first incarceration. Thus, the exclusion criteria create a sample of incarcerated

persons who may have greater issues adjusting to prison life, following institutional rules, or who commit more severe misconducts relative to the general prison population as a whole. This study does not generalize to all individuals housed in state prisons or even to just those experiencing their first incarceration. Rather, this study examines a specific sample of first-time incarcerated individuals who commit at least one prison rule infraction serious enough to warrant placement in the harshest punishment available within correctional institutions, and who remain within prison for at least one year after they have been released from disciplinary custody. The requirement of one year of follow-up is necessary to allow for enough follow-up time to examine the impact of the disciplinary custody stay. However, this exclusion criterion further restricts the sample to those serving longer sentences, likely for harsher or more violent originating offenses. The exclusion of those who are released from the institution prior to serving a full year following a disciplinary custody stay is the main decision that results in a riskier sample. To be clear, in studies of heterogeneity disciplinary custody sanctioning, the sample will be “riskier” than the general prison population due to the examination of only those who commit a misconduct and are placed in disciplinary custody in the first place.

Table 1. Descriptive Statistics for Full Length of Stay Sample

Variable	Obs.	Mean	Std. Dev.	Min	Max
Treatment Indicators					
Length of Stay	4,425	45.031	50.474	0	1320
Early Release	3,275	0.647	-	0	1
Outcome Variables					
Subsequent Misconduct	4,425	0.617	-	0	1
Days to Subsequent Misconduct	3,299	196.965	215.798	0	1758
Covariates					
Demographics					

Age at First DC Stay	4,425	28.565	9.237	16.110	73.155
White	4,425	0.321	-	0	1
Black	4,425	0.561	-	0	1
Hispanic	4,425	0.111	-	0	1
Other Race	4,425	0.006	-	0	1
Single	4,425	0.860	-	0	1
Married	4,425	0.084	-	0	1
Divorced, Separated, Widowed	4,425	0.049	-	0	1
Unknown Marital Status	4,425	0.007	-	0	1
Incarceration Status					
Custody Level 1 and 2	4,425	0.056	-	0	1
Custody Level 3	4,425	0.405	-	0	1
Custody Level 4	4,425	0.231	-	0	1
Custody Level 5	4,425	0.063	-	0	1
Unknown Custody Level	4,425	0.244	-	0	1
Length of Incarceration (days)	4,425	326.865	286.324	0	1605
Incarceration Offense Type					
Violent	4,425	0.450	-	0	1
Property	4,425	0.121	-	0	1
Drug	4,425	0.127	-	0	1
Other	4,425	0.184	-	0	1
Missing	4,425	0.118	-	0	1
Mental Health					
Highest Prior MH C	4,425	0.184	-	0	1
Highest Prior MH D	4,425	0.062	-	0	1
Work and Programming					
Total Prior Violent Program	4,425	0.147	0.447	0	4
Total Prior Alcohol/Drug Program	4,425	0.115	0.368	0	4
Total Prior Other Program	4,425	0.093	0.351	0	4
Current Violent Program	4,425	0.034	-	0	1
Current Alcohol/Drug Program	4,425	0.034	-	0	1
Current Other Program	4,425	0.018	-	0	1
Worked at Time of First DC Stay	4,425	0.739	-	0	1
Current and Prior Misconducts					
Current Violent Misconduct	4,425	0.313	-	0	1
Current Property Misconduct	4,425	0.108	-	0	1
Current Drug Misconduct	4,425	0.037	-	0	1
Current Prison Rule Misconduct	4,425	0.408	-	0	1
Current Other Misconduct	4,425	0.135	-	0	1
Prior Misconduct Count	4,425	0.680	1.136	0	10
Institutional Indicators					
Albion	4,425	0.020	-	0	1
Benner Township	4,425	0.046	-	0	1
Cambridge Springs	4,425	0.018	-	0	1

Chester	4,425	0.034	-	0	1
Coal Township	4,425	0.033	-	0	1
Dallas	4,425	0.035	-	0	1
Fayette	4,425	0.038	-	0	1
Forest	4,425	0.038	-	0	1
Frackville	4,425	0.019	-	0	1
Graterford	4,425	0.081	-	0	1
Greene	4,425	0.020	-	0	1
Houtzdale	4,425	0.047	-	0	1
Huntingdon	4,425	0.033	-	0	1
Laurel Highlands	4,425	0.022	-	0	1
Mahanoy	4,425	0.022	-	0	1
Muncy	4,425	0.047	-	0	1
Pine Grove	4,425	0.053	-	0	1
Pittsburgh	4,425	0.038	-	0	1
Retreat	4,425	0.012	-	0	1
Rockview	4,425	0.037	-	0	1
Smithfield	4,425	0.019	-	0	1
Somerset	4,425	0.057	-	0	1
Waymart	4,425	0.031	-	0	1

Measures

Treatment Indicator Variables

Length of Stay

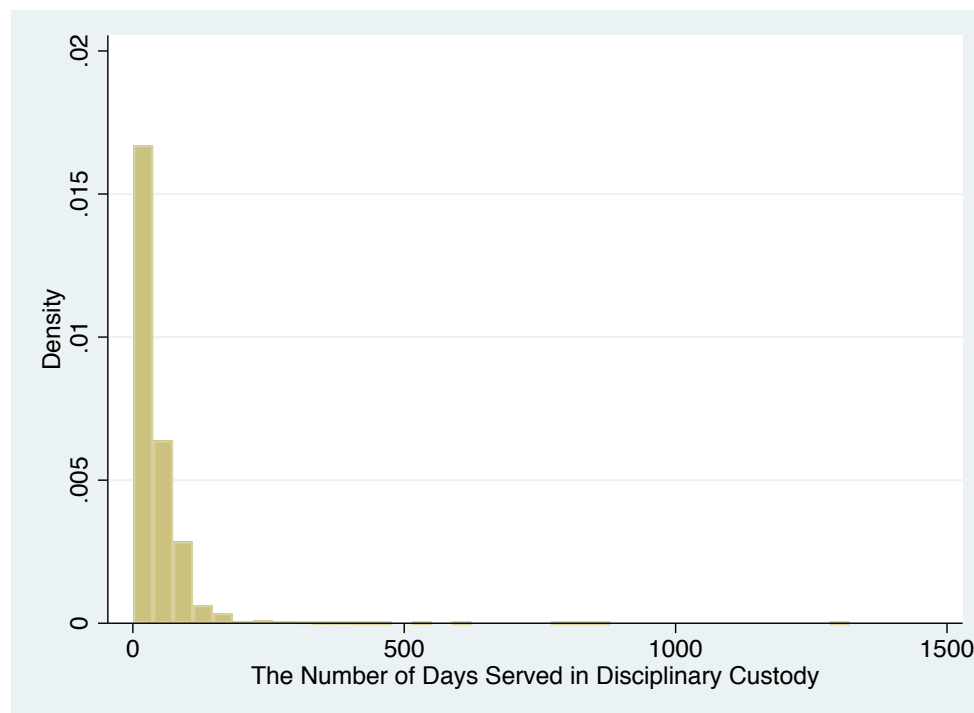
The first treatment examined in the current study is length of stay. In the sanctioning process, the hearing examiner assigns a sanction (or multiple sanctions) and a length for each sanction. The data collected for the current study contained the original sanction length assigned for each sanctioned misconduct. However, this original sanction may not be an accurate indicator of how long an individual actually spends within disciplinary custody. First, individuals can be released early from disciplinary custody (a process described in more detail below). Secondly, individuals may commit additional infractions within the disciplinary custody unit and receive additional days to serve in

disciplinary custody as punishment. Thus, it is more accurate to examine the number of actual days served rather than the original number of days in the sentence given. To create this variable, two dates within PADOCC were examined as potential sources to create the treatment indicator. First, the Completion Date is the expected date of release given the sanction length set forth by the hearing examiner. Second, the Actual Completion Date is the actual date the individual is released from disciplinary custody to the general inmate population. To create the variable of the number of days an individual actually served in disciplinary custody, the Actual Completion Date was used rather than the original sanction length.

The distribution of actual number of days served (*Length of Stay*) is shown in Figure 2. As can be seen in that figure, there are several outliers at the upper end of the distribution. The average number of days served in the first disciplinary custody is 45.03 days and the median is 30 days. Although the current study examines length of stay across the whole correctional system, it is likely that punishment practices and norms vary across institutions. For length of stay, institutional average number of days served in the current sample ranges between 28.8 days in Cambridge Springs to a high of 62.0 days in Pine Grove. This research question will be examined utilizing inverse probability weighing with regression adjustment modified for multivalued treatments. This method (described in greater detail below) calculates the average treatment effect for receiving one dose of the treatment relative to a higher or lower dose. In practice, quartiles (Loughran et al., 2009) or quintiles (Bucklen, 2014) are typically used as convenient cutoff points to create roughly equal-sized dosage groups. In the current study, the

quartiles of the length of stay distribution will be used.¹² The use of quartiles is largely a statistical one, as using quartiles provides roughly equivalent sized groups in order to compare across different categories of length of stay.¹³ The quartiles for this treatment are as follows: 1-21, 22-30, 31-55, and 56-1320¹⁴ days and each quartile contains roughly 1,100 individuals.

Figure 2. Distribution of Actual Days Served in Disciplinary Custody (n=4,425)



¹² Originally, quintiles were explored as potential dose-response categories to provide additional variation within the length of stay and early release dose-response models. However, the distribution of the early release variable limited the ability to use quintiles. The large number of individuals serving exactly 100% of their sanction meant the smallest possible delineation of that data was the quartile rather than the quintile. For consistency, quartiles will be used in the length of stay analysis as well.

¹³ Additional analyses were undertaken to examine the potential consequences of altering the operationalization of length of stay. Within the length of stay distribution, there are spikes at 15-day increments, indicating that hearing examiners often place disciplinary custody sanctions around 15, 30, and 45 days. Due to the frequency of these sanction lengths, I also examined dividing the length of stay distribution by 15-day increments. Although this designation did not create roughly equivalent group sizes as the length of stay quartiles used in the main analyses, these designations are likely of more interest to hearing examiners and practitioners. However, the results for the length of stay analyses were substantively similar to those of the main analyses using quartiles.

¹⁴ The 99th percentile is 228 days.

Early Release

The other treatment examined in the current study is the early release mechanism. In PADOc's sanctioning process, an individual is sentenced to a particular sanction and then a length for that sanction. For disciplinary custody sanctions specifically, there is a mechanism for early release from that punishment back into the general prison population. The Program Review Committee (PRC) is a group of three staff members¹⁵ working within Pennsylvania's correctional system that deals with multiple aspects of the sanctioning process such as conducting administrative custody hearings and overseeing the first level of appeals for misconduct hearings. Another function of the PRC is making decisions regarding continued confinement within disciplinary custody. At regular intervals throughout the month, the PRC meets, evaluates each misconduct case and the individual's behavior while in disciplinary custody, and determines whether that individual should serve their full disciplinary custody term or return to the general population prior to the completion of their sanction length. As stated in the Inmate Discipline Procedures Manual, "...the PRC must consider a release to general population upon completion of half of the sanction imposed" (PADOc, 2015). This process is generally known to inmates prior to receiving disciplinary custody time.¹⁶ Within

¹⁵ The PADOc Inmate Discipline Procedures Manual (DC-ADM 801) specifies that "The committee shall consist of one staff member from each of the following classifications: Deputy Superintendent (who shall serve as the chairperson), Corrections Classification and Program Manager (CCPM), Unit Manager, School Principal, Drug and Alcohol Treatment Specialist (DATS), Supervisor or Inmate Records Office Supervisor, and a Commissioned Officer. The Facility Manager may designate other staff as committee members, however, if such designations are made, they shall be in writing and the Facility Manager shall maintain a list of all designees. Whenever a PRC is convened, at least one member of the committee shall be a staff member who is directly involved in the administration of the RHU/SMU in which the inmate is currently housed" (PADOc, 2015, Glossary of Terms).

¹⁶ Although the early release process is not described in the Inmate Handbook, the handbook does specify that individuals in the misconduct hearing appeals process have access to the DOC Inmate Discipline

PADOC, there are fourteen listed misconduct offenses that are not eligible for early release due to the severity of the misconduct types. These offenses are as follows: aggravated assault, arson, assault, burglary, escape, extortion by threat of violence, involuntary deviate sexual intercourse, kidnapping, murder, rape, riot, robbery, unlawful restraint, and voluntary manslaughter. These offenses are thus excluded from analyses examining the early release mechanism.

Of all offenses eligible for early release (n=4,014), there is significant variation across institutions in the early release decision. On one end of the distribution, Fayette (which has 165 first disciplinary custody stays in the current study) only had 4.85% of eligible cases released prior to serving the full sanction length. On the other end of the distribution, individuals in Forest (which had 154 eligible disciplinary custody stays) received early release 81.17% of eligible stays. The institution contributing the largest number of cases to the current study, Camp Hill (one of the Diagnostic and Classification Centers), awarded early release to 77.84% of its eligible first disciplinary custody stays. These differences highlight the need to account for institutional factors when examining treatment heterogeneity in disciplinary custody stays.

For the first examination of the early release mechanism, the treatment condition is defined as serving any amount of time less than 100% of the original sanction length. The data obtained from PADOC contain two variables which allow for the examination of this research question; the first is the Completion Date which is the expected date of release given the original sanction length, the second, the Actual Completion Date, is the

Policy. In a personal correspondence with a member of the PRC within PADOC, the PRC member specified that individuals within the system are generally aware of the PRC process, and “Even if they don’t know about it through policy, most inmates will see PRC within the first 7 days. If they are not seen individually within the first 7 days, PRC also makes weekly rounds to the cells. It is also common place to automatically schedule inmates to see PRC when they reach their ½ time and are eligible for early release.”

date the individual was actually released from disciplinary custody. Thus, the binary measure of early release was set equal to one if the Actual Completion Date came prior to the Completion Date (*Early Release*). This is the most conservative approach to examining early release because it includes all individuals who were released prior to serving their full sanction length. The comparison condition for this treatment is defined as those who serve exactly 100% of their original sanction length (i.e. the Completion Date and the Actual Completion Date are the same). Any individual who served more than 100% of their original sanction likely did so because of additional misconducts committed while in disciplinary custody, resulting in additional charges and sanctions to disciplinary custody. As these individuals would have a very low probability of being released prior to serving their original sanction length, they are excluded from the early release analyses.¹⁷

The exclusion of those serving more than 100% of their original sanction length from the comparison group reduces the original disciplinary custody sample by 720 individuals and excluding those offenses that are not eligible for early release further reduces the sample by 411 individuals. The method used in the current analyses requires each individual to have a non-zero probability of being placed in each potential treatment category. For the non-binary early release analyses (described below), 19 individuals did not meet this threshold and were thus excluded from analyses. Thus, the final analysis sample for the early release mechanism is 3,275 individuals; 1,162 of whom served exactly 100% of their original sanction length and 2,132 of whom were released prior to

¹⁷ Supplemental analyses including all individuals who served more than 100% of their original sanction were conducted to further examine this decision. These results are discussed in detail in the results section below.

serving 100% of the sanction length. The distribution of percent sanction served for this sample is shown in Figure 3 and the descriptive statistics are shown in Table 2.

Figure 3. Distribution of Early Release from Disciplinary Custody (n=3,275)

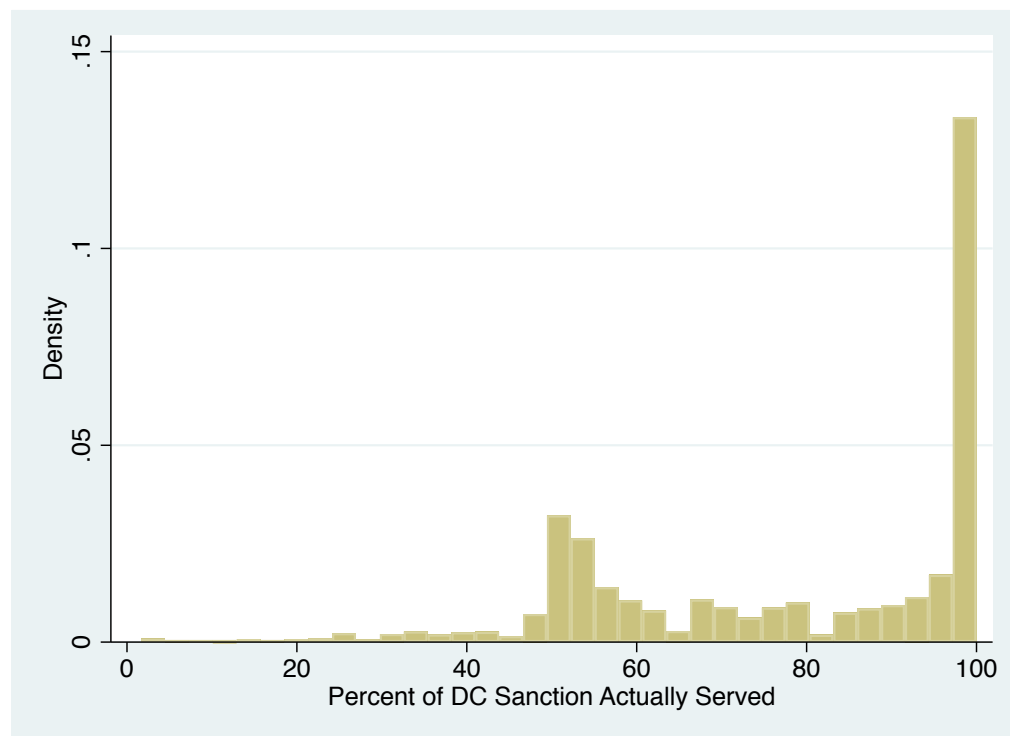


Table 2. Descriptive Statistics for Early Release Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
Treatment Indicators					
Length of Stay	3,275	34.846	24.041	0	259
Early Release	3,275	0.647	-	0	1
Outcome Variables					
Subsequent Misconduct	3,275	0.609	-	0	1
Days to Subsequent Misconduct	2,440	205.297	222.069	1	1758
Covariates					
Demographics					
Age at First DC Stay	3,275	28.422	9.090	16.517	73.155
White	3,275	0.315	-	0	1
Black	3,275	0.578	-	0	1

Hispanic	3,275	0.107	-	0	1
Other Race	3,275	0	-	0	0
Single	3,275	0.860	-	0	1
Married	3,275	0.087	-	0	1
Divorced, Separated, Widowed	3,275	0.047	-	0	1
Unknown Marital Status	3,275	0.007	-	0	1
Incarceration Status					
Custody Level 1 and 2	3,275	0.049	-	0	1
Custody Level 3	3,275	0.415	-	0	1
Custody Level 4	3,275	0.225	-	0	1
Custody Level 5	3,275	0.064	-	0	1
Unknown Custody Level	3,275	0.246	-	0	1
Length of Incarceration (days)	3,275	326.619	284.134	0	1605
Incarceration Offense Type					
Violent	3,275	0.450	-	0	1
Property	3,275	0.116	-	0	1
Drug	3,275	0.129	-	0	1
Other	3,275	0.189	-	0	1
Missing	3,275	0.115	-	0	1
Mental Health					
Highest Prior MH C	3,275	0.172	-	0	1
Highest Prior MH D	3,275	0.057	-	0	1
Work and Programming					
Total Prior Violent Program	3,275	0.144	0.437	0	4
Total Prior Alcohol/Drug Program	3,275	0.115	0.370	0	3
Total Prior Other Program	3,275	0.088	0.341	0	4
Current Violent Program	3,275	0.031	-	0	1
Current Alcohol/Drug Program	3,275	0.034	-	0	1
Current Other Program	3,275	0.017	-	0	1
Worked at Time of First DC Stay	3,275	0.729	-	0	1
Current and Prior Misconducts					
Current Violent Misconduct	3,275	0.253	-	0	1
Current Property Misconduct	3,275	0.113	-	0	1
Current Drug Misconduct	3,275	0.041	-	0	1
Current Prison Rule Misconduct	3,275	0.451	-	0	1
Current Other Misconduct	3,275	0.143	-	0	1
Prior Misconduct Count	3,275	0.658	1.121	0	10
Institutional Indicators					
Albion	3,275	0.021	-	0	1
Benner Township	3,275	0.056	-	0	1
Cambridge Springs	3,275	0.012	-	0	1
Chester	3,275	0.028	-	0	1

Coal Township	3,275	0.031	-	0	1
Dallas	3,275	0.034	-	0	1
Fayette	3,275	0.043	-	0	1
Forest	3,275	0.041	-	0	1
Frackville	3,275	0.021	-	0	1
Graterford	3,275	0.085	-	0	1
Greene	3,275	0.019	-	0	1
Houtzdale	3,275	0.053	-	0	1
Huntingdon	3,275	0.035	-	0	1
Laurel Highlands	3,275	0.026	-	0	1
Mahanoy	3,275	0.018	-	0	1
Muncy	3,275	0.038	-	0	1
Pine Grove	3,275	0.055	-	0	1
Pittsburgh	3,275	0.026	-	0	1
Retreat	3,275	0.013	-	0	1
Rockview	3,275	0.042	-	0	1
Smithfield	3,275	0.020	-	0	1
Somerset	3,275	0.064	-	0	1
Waymart	3,275	0.029	-	0	1

In comparing the early release sample in Table 2 to the full sample in Table 1, the samples are rather similar along demographic lines. The main difference between the two samples is in the current misconduct offense type. As would be expected given the misconduct type constraints placed on early release eligibility, the early release sample is made up of less violent misconduct relative to the length of stay sample (25.3% compared to 31.3%). Additionally, a greater proportion of misconducts in the early release sample are due to a prison rule violation compared to the full sample (45.1% compared to 40.8%).

Following the binary treatment of early release, the next set of analyses will examine the extent to which an individual is released early. This analysis examines the 3,275 individuals who were released early or served the full 100% of their sanction length. It is likely that various thresholds of early release, such as 50% or 75% of the

original sanction length could have differing impacts on deterrent outcomes and the examination of early release as a binary treatment could mask these differences. These two methods examining early release (binary and multivalued) allow for the study of both the experience of early release and the degree of early release. As with the length of stay analysis described above, the multivalued analysis of early release will use the rough adherence to the quartiles of that distribution to create dosage categories. The quartiles are as follows: 0%-55.56%; 55.57%-86.66%; 86.67%-99.39%; 99.44%-100%. Due to the large number of individuals who served exactly 100% of their sanction, the rough adherence to the quartiles created slightly dissimilar group sizes. The first two quartiles contain roughly 850 individuals, the third 428 individuals, and the last category contains 1,164 individuals.

Outcome Variables

The current study utilizes two main outcomes of misconduct following completion of a disciplinary custody sanction. First, I examine a dummy indicator of whether or not the individual committed an additional misconduct (of any offense type) within one year of release from disciplinary custody (*Subsequent Misconduct*). Individuals are followed for one year after release from disciplinary custody to the general prison population to allow for adequate time for the individual to adjust to post-isolation conditions. Additionally, behavior more than a year after a relatively short punishment would be less likely to be related to that punishment compared to other more recent prison experiences. This variable was created using the date released from disciplinary custody and was coded as 1 if any misconduct committed by that individual

was between the Actual Completion Date and 365 days after that date.¹⁸ As seen in Table 1, 61.7% of the sample had a subsequent misconduct within one year of release from disciplinary custody.

For the 2nd and 4th hypotheses, to address potential differences in restrictive deterrence rather than absolute deterrence, I also examine the number of days to the first subsequent misconduct following release from disciplinary custody (*Days to Subsequent Misconduct*). As seen in Table 1, the average number of days to the first misconduct following release from disciplinary custody among those with a follow-up misconduct is 197.0 days with a median of 123 days.

Covariates

Propensity score methods assess balance only on variables that are observed and measured. Thus, it is important to include as many theoretically-relevant variables as possible to fully maximize the propensity score methodology. The dataset collected from PADOX provides a rich selection of covariates that would be expected to be related to both the likelihood of receiving the treatment (both length of stay and early release) and the likelihood of and timing to subsequent misconduct. This full list of covariates will be included in the estimation of the propensity score.¹⁹ The sample of individuals studied differs by the two treatment conditions examined due to the restraints on the comparison

¹⁸ All analyses examine only misconduct after release from disciplinary custody back to the general prison population. Additional analyses were run to include misconduct that occurred within disciplinary custody as part of the recidivated within one year of release outcome. Although 641 individuals (~14.5% of the sample) had misconduct within disciplinary custody, including these misconducts in the recidivism outcome only increased those who recidivated within 1 year by 289 individuals. The inclusion of this group did not substantively change the results of the binary recidivism main results.

¹⁹ One benefit to the use of propensity score methods is the ability to use missing data as its own covariate (Rosenbaum & Rubin, 1984). For example, those with an unknown custody level are likely more similar (e.g. they are likely housed in a Diagnostic center at the time of their first disciplinary custody stay and have not been classified and assigned a permanent institution) relative to other custody levels. Thus, multiple indicators of missing data are included in the vector of covariates.

condition in the early release mechanism (i.e. only those who were eligible for early release and served exactly 100% of the original sanction length). Thus, the descriptive statistics shown in Table 1 and described in detail below are for the full sample of 4,425 individuals prior to the restrictions imposed by the second type of treatment heterogeneity examined. The descriptive statistics for the early release sample are shown in Table 2.

First, demographic variables are included, namely age, race/ethnicity, and marital status.²⁰ Hearing examiners may sanction individuals differently based on their baseline characteristics. For example, younger prisoners may be seen as more of a threat to general prison order due to the well-known relationship between age and behavior. Age (*Age at First DC Stay*) is included due to the well-documented relationship between age and misconduct within prison (Steiner et al., 2014). This continuous variable was calculated from the individual's date of birth and the date of their first admission to disciplinary custody. There is a wide range of ages present in the dataset with an average age at first disciplinary custody stay of about 28 and a half years old. Individual race and ethnicity (*White, Black, Hispanic, Other Race*) captures the potential for racial differences in treatment as well as in the deterrent capabilities of disciplinary custody. The majority of the sample is Black (56.1%), 32% is White, and 11% is identified as Hispanic. The Other Race category combines the categories of Asian, American Indian, and Other as designated by PADOH and comprises less than 1% of the sample. Lastly, marital status is included in the analyses (*Single; Married; Divorced, Separated, Widowed; Unknown Marital Status*). Due to small sample sizes, the categories of

²⁰ Originally, a dummy variable for sex (*Male*) was included in analyses. However, the addition of institutional dummy variables caused the variable Male to drop out due to multicollinearity. The institution variables are perfect predictors of sex as each institution exclusively houses either male or female prisoners.

divorced, separated, and widowed were combined into one dummy variable. The majority of the sample is single (86.0%), with only 8.4% of the sample identified as married at the time of first admission to PADO.

Next, variables identifying incarceration status and an individual's time in prison prior to their first disciplinary custody stay are included. Factors such as custody level are likely to be at the forefront when hearing examiners are making their sanctioning decisions. Other indications of risk such as the incarceration offense can provide additional information to the hearing examiners on the type of offender that individual is. First, the individual's custody level as identified by PADO is included in analyses. Custody level ranges from 1 to 5, with a level of 5 indicating those in need of the highest level of security. Although Level 1 is a potential category of custody, within this sample, the lowest rated custody level was Level 2 (*Custody Level 2, Custody Level 3, Custody Level 4; Custody Level 5; and Unknown Custody Level*). The most populated category for custody level is Level 3 with 40.5% of the sample, followed by the unknown custody level (24.4%) and Level 4 with 23.1% of the sample. Another incarceration covariate is the type of offense for which the individual was incarcerated. Some prior research has found a relationship between the incarceration offense and behavior within prison (Steiner et al., 2014). These offense types are classified into five categories (*Violent, Property, Drug, Other, Missing*).²¹ The most common incarceration offense type is violent (45.0%), followed by other (18.4%), drug (12.7%), property (12.1%), and missing (11.8%). In addition to incarceration offense type, a measure of the number of days incarcerated prior to the first disciplinary custody stay is included (*Length of*

²¹ The most common offenses by type are as follows: for violent, Robbery with Serious Bodily Injury; for property, Burglary; for drug, Manufacture/Sale/Deliver or Possess with Intent; and for other, Persons Not to Possess, Use, etc. Firearms.

Incarceration). Length of incarceration could be a proxy for adjustment to life in prison. Individuals committing a misconduct early on in their incarceration stay may be treated more leniently by hearing examiners due to a perception of simple adjustment issues rather than an indicator of greater risk to other prisoners or to prison staff. The average individual is incarcerated for just under a year (326.87 days) prior to their first disciplinary custody stay. This measure captures time at risk in that those who have fewer days incarcerated prior to their first serious sanction are likely at a higher risk for subsequent problematic behaviors relative to those who are able to avoid serious sanctioning for a longer period of time.

Another set of variables included are those for mental health status. Mental health status is likely an indicator considered by hearing examiners and members of the Program Review Committee when making their sanctioning decisions, particularly due to perceptions of individuals presenting with severe mental health issues as more dangerous or risky to other inmates or prison staff. Additionally, prior research has found evidence of a relationship between mental health status and misconduct within prison (James & Glaze, 2006). Within PADO, an individual is evaluated for mental health status at a Diagnostic Center when first admitted and subsequent evaluations take place throughout the time the individual is incarcerated. Mental health status is rated on a four-point scale: “A” Roster designates those who have no identified mental health needs or a history of such needs; “B” Roster designates those who have a history of mental health issues but do not present as currently in need of treatment; “C” Roster designates those who are currently receiving treatment but have not been diagnosed with a serious mental illness;

and “D” Roster designates those who have been diagnosed with a serious mental illness.²² For the current study, two measures of severe mental health issues are included, and both are dummy variables identifying if the highest prior mental health designation was as “C” or “D” Rosters (*Highest Prior MH C, Highest Prior MH D*).²³

Additional incarceration variables of work and programming within PADOc are included. Working and programs within prison provide structure to daily activities and could be a factor impacting the length of time spent within disciplinary custody (i.e. if a hearing examiner takes into account the work or programs an individual is involved with when making sanctioning decisions). First, a work variable was created and (*Worked at Time of First DC Stay*) is a dummy variable indicating if the individual was on work assignment at the time of the first disciplinary custody stay. The majority of the sample (73.9%) was on a work assignment at the time of the first disciplinary custody stay. Specific job type data were not gathered for the current study. Second, for programs, multiple variables were created based on the type of program and the timing of the program prior to the first disciplinary custody stay. Programs were categorized by type based on the title of the program as well as additional information provided on the PADOc website.²⁴ Very few individuals were in a program at the time of their first entry

²² A serious mental illness is defined by PADOc in Policy 13.8.1 (Access to Mental Health Care) as “a substantial disorder of thought or mood, which significantly impairs judgment, behavior, capacity or recognize reality, or cope with the ordinary demands of life.”

²³ Original models included several more mental health indicators, included average prior mental health scores. However, due to issues of multicollinearity in the models, these variables were pared down to just two indicators.

²⁴ Originally, programming was distributed into six categories: Violent, Sex Offender, Parenting, Alcohol and Drug, Therapeutic, and Other Programs. However, due to small sample sizes and issues of multicollinearity, these were pared down into three categories: Violent, Alcohol and Drug, and Other Programs. The Other category now includes all sex offender, parenting, and therapeutic programs. Violent programs included programs aimed at violence reduction such as “Batterers Group” and “Violence Prevention High Intensity.” Sex offender programs were those focused solely on individuals convicted of sex-based offenses and included programs such as “Sex Offender Program Mod-High Intensity” and “Sex Offender Orientation.” Parenting programs focused on helping individuals learn better parenting skills to

into disciplinary custody (*Current Violent Program, Current Alcohol/Drug Program, and Current Other Program*). Additionally, the majority of individuals in the sample were not in any programs prior to the first DC stay (*Total Prior Violent Program, Total Prior Alcohol/Drug Program, and Total Prior Other Program*).

Additionally, indicators of the current misconduct offense and misconducts prior to the first disciplinary custody stay are included due to the potential impact of prior and current misconduct on the treatment conditions and on future behavior. Hearing examiners likely take into account the misconduct history of an individual when making sanctioning decisions and those with a greater number of prior misconducts would be expected to receive a harsher sanction relative to someone appearing for their first infraction. The type of misconduct also would be expected to have an impact on sanctioning decisions (i.e. it is more likely that an individual sanctioned for a violent misconduct will serve longer within disciplinary custody compared to someone with a more minor infraction). Prior studies have also found consistent support for the relationship between prior misconduct behavior and future misconduct (Steiner et al., 2014). First, the misconduct offense type for the misconduct resulting in the first disciplinary custody stay is included in five dummy indicators (*Current Violent Misconduct, Current Property Misconduct, Current Drug Misconduct, Current Prison*

improve parenting upon release from prison and included programs such as “Positive Parenting” and “Parenting Teens.” Alcohol and Drug programs included classes such as “Co-Occurring Therapeutic Community” and “Relapse Prevention.” Therapeutic programming included programs aimed at helping prisoners cope as well as counseling and included programs such as “Character Development” and “Thinking for a Change.” Lastly, other programming contains programs that did not fit into other categories and included programs such as “PV Group” and “Seeking Safety (Females).” PADOC also offers programs focused on education and work apprenticeships but the final sample in the current study did not participate in any of those programs.

Rule Misconduct, and *Current Other Misconduct*).²⁵ The majority of misconducts resulting in the first disciplinary custody stay are for a prison rule violation (40.8%) followed by a violent misconduct (31.3%). This demonstrates the importance of examining all misconduct offense types leading to a disciplinary custody stay rather than a focus on just violent incidents.

An indicator for the total number of prior reported misconducts is included (*Prior Misconduct Count*). Prior infractions likely impact the treatment conditions and are also a predictor of future misconducts. For the current sample, the number of prior misconducts ranges from 0 to 10 with the average individual having less than one (.68) prior misconduct. Attempts were also made to include guilty and not guilty prior misconduct charges broken down by offense type but small n's and issues of multicollinearity led to the sole inclusion of a prior misconduct count.

Finally, dummy indicators for each PADOc institution were included in the analyses. In a system such as PADOc, it is necessary to account for institutions in both examining the treatment and the outcomes due to the potential for differing punishment practices/norms and differing environments. As discussed above, there is variation in both length of stay and early release practices by institution. Including these institutional dummies in the propensity score models is thus necessary to accurately estimate the probability of receiving a particular treatment or treatment level. Additionally, institutions can have an impact on follow-up misconduct. Misconduct rates vary by institution and it is likely that different environments or tolerance for particular types of

²⁵ The most common misconduct offense by type is as follows: for violent misconduct, Fighting; for property misconduct, Possess Contraband Including Money, Implements of Escape, Drugs, Etc.; for drug misconduct, Possession or Use of Dangerous or Controlled Substance; for prison rule misconduct, Refusing to Obey an Order; and for other misconduct, Threaten an Employee or Their Family with Bodily Harm.

misconduct in one institution over another can impact the likelihood of and days to a subsequent misconduct.

Importantly, propensity score methods are only capable of matching on observed and measured covariates. Thus, any limitations in the covariates available for study limit claims of accounting for selection bias with this methodology. In the current study, additional covariates not available would bolster the propensity score matching technique. For example, the current study does not have indicators for visitation. It is possible that frequent visitation could affect a hearing examiner's sanctioning decisions based on perceived ties to the outside world and could also provide an indicator of risk for future misbehavior. Additionally, the current study does not include risk assessment scores. Risk assessment test scores were part of the original data collection, however, extensive missingness within that variable (over 70% of the disciplinary custody sample) resulted in the dropping of that variable. Other variables, namely custody level and mental health status are components of the risk assessment evaluation, and are highly collinear to the risk assessment score and are included in the current analyses.²⁶

Additionally, the risk assessment tool is largely used to determine placement in treatment programs and the inclusion of these variables in this analysis captures that aspect of the risk assessment tool. Although other similar variables are included in analyses, such as custody level, it is important to recognize that the exclusion of this variable could result in unobserved heterogeneity and imbalance among unobserved variables. Although steps

²⁶ Among the sample with risk assessment data (n=983), the two risk assessment variables, the average prior RST score and the highest prior RST score, were significantly correlated (at .05) with the highest prior mental health variables and custody level. Due to the high correlation between the risk assessment score and these other variables included in analyses, these variables are expected to serve as an adequate proxy for risk assessment scores. Additional analyses including an indicator of highest prior risk assessment score resulted in issues of multicollinearity and the model failing to converge, further providing evidence that the variables included in the model are capturing similar information to that provided by the risk assessment score.

were taken to include all potential, available confounders, the lack of these theoretically important variables prohibits causal interpretations in the current study. However, the benefits of propensity score methods, namely the identification of issues of common support and the nonparametric matching of individuals, provide advantages for this methodology above regression techniques (Apel & Sweeten, 2010).

Analytic Plan

Research examining all types of solitary confinement necessitates non-experimental studies due to the ethical issues of conducting an experiment that randomizes solitary confinement or the length of stay in solitary confinement. Any non-experimental study that attempts to isolate the effects of solitary confinement on later behaviors must rely on other methods that deal with the issue of selection. In the case of disciplinary custody, individuals who are sanctioned to disciplinary custody as a result of a misconduct and those sanctioned to a different punishment are likely fundamentally different on multiple characteristics indicative of risk. In the current study of treatment heterogeneity in disciplinary custody stays, those sentenced to longer stays within disciplinary custody likely differ from those sentenced to brief stays, or those who are released from disciplinary custody early may already be at less risk for subsequent rule-breaking relative to those who serve their full stays. Thus, the comparison of individuals receiving different levels of the treatment involves comparing dissimilar groups (Loughran et al., 2009). In the sections to follow, I first outline the use of inverse probability weighting with regression adjustment to create comparable groups. First, I discuss the use of inverse probability weighting for both the binary early release treatment as well as the multivalued treatments of length of stay and early release. Next, I

briefly introduce the use of inverse probability weighting with regression adjustment modified for survival outcomes to evaluate the 2nd and 4th hypotheses investigating the time to subsequent misconduct. Lastly, I discuss how these methods will be incorporated into the current study and outline the methodological choices made in this dissertation.

Propensity Score Modeling and the Average Treatment Effect

To examine heterogeneity in solitary confinement as punishment, a randomized controlled experiment would be the “gold standard” to randomly assign varying lengths of stay or early release from disciplinary custody among a sample of inmates. Random assignment thus controls both observed and unobserved differences between the treatment and control groups. However, due to the practical and ethical implications of randomly assigning particular individuals to spend longer sentences in harsh conditions, other methodological avenues must be examined.

One method that is used to address the selection issues inherent in research questions such as these is propensity score matching (Rosenbaum & Rubin, 1983). In this methodology, a score is created that is indicative of the propensity to receive a given treatment based on a set of observed covariates. This score is then used to create balance across the treatment and control group on the observed predictors, indicating that the treatment assignment is conditionally independent of the covariates. The goal with propensity score matching is to create balance over as many observed variables as possible with the aim of creating balance over all unobserved factors as well. Using this propensity score to then match (or stratify) across the control and treatment group creates a simulated counterfactual for the treatment group (i.e. what the outcome would have been if an individual in the treatment group had been placed in the control group). Thus,

any estimates are expected to be unbiased under the assumption that all factors which could impact the treatment assignment and the outcome are included in the set of observed covariates (Rosenbaum and Rubin, 1984).

Rosenbaum and Rubin (1983) define the propensity score as “the conditional probability of assignment to a particular treatment given a vector of observed covariates” (p. 41). The traditional formula for the propensity score is:

$$e(x) = pr(Z=I|x)$$

Z represents the binary treatment and x is the vector of observed covariates. The estimated propensity score ranges from 0 to 1, with scores closer to 1 representing a greater likelihood of receiving the given treatment.

Within propensity score modeling, the outcome of interest is the average treatment effect (ATE), defined as “ $E(T_1) - E(T_0)$ where $E(.)$ denotes expectation in the population” (Rosenbaum & Rubin, 1983, p. 42). In other words the ATE is the “expected effect of treatment on a randomly drawn person from the population” (Wooldridge, 2010, p. 905). In layman’s term, the average treatment effect “... of treatment 1 relative to treatment 2 is the comparison of mean outcomes had the entire population been observed under one treatment, versus had the entire population been observed under another treatment” (McCaffrey, Griffin, Almirall, Slaughter, Ramchand, & Burgette, 2013, p. 3390). Another outcome of interest is known as the average treatment effect on the treated (ATET), which is “the main effect of those who actually participated in the program” (Wooldridge, 2010, p. 906). Generally, in a particular study, researchers estimate either the ATE or the ATET. A disadvantage of ATET is that it does “...not support inferences about the relative effect of programs if they are expanded...”

(McCaffrey et al., 2013, p. 3391) beyond those that specifically received that treatment. For this reason, the current study estimated the average treatment effect in all main and supplemental analyses.

When examining a single treatment, the ATE is the comparison of mean outcomes of if the entire population received the treatment versus if the entire population had received the control. However, within a study examining a multivalued treatment, there are multiple pairwise ATEs. In the current examination of both length of stay and multivalued early release, there are 6 pairwise ATEs estimated due to the quartile designation of the independent variable (i.e. first quartile relative to the fourth quartile, the second quartile relative to the fourth quartile, and so on).

There are two assumptions required to estimate the average treatment effect. The first, known as the ignorability assumption (Rosenbaum & Rubin, 1983) (also called unconfoundedness or conditional independence (Wooldridge, 2010)), is defined as, “Conditional on x , w and (y_0, y_1) are independent” (Wooldridge, 2010, p. 908). Simply put, this assumption holds that there is no unmeasured confounding in the model (Cole & Hernan, 2008). The second assumption, known as the overlap assumption,²⁷ specifies there is a requirement to be “...able to observe both control and treated units for every outcome on x ” (Wooldridge, 2010, p. 910). “Overlap means that, for any setting of the covariates in the assumed population, there is a chance of seeing units in both the control and treatment groups” (Wooldridge, 2010, p. 910). The overlap assumption specifies that the propensity score for a specific set of covariates is never zero or one, meaning there is a statistical probability that that individual could receive the specific treatment (Cole &

²⁷ The overlap assumption has also been referred to in the literature as the positivity assumption (Cole & Hernan, 2008; McCaffrey et al., 2013).

Hernan, 2008; Wooldridge, 2010). Rosenbaum and Rubin (1983) state when both of these assumptions are met then “treatment assignment is strongly ignorable given the observed covariates x ” (p. 43).

Inverse Probability Weighting

One method of analysis using the basics of propensity score methods is inverse probability weighting (Rosenbaum, 1987). Inverse probability weighting uses “weights based on the propensity score to create a synthetic sample in which the distribution of measured baseline covariates is independent of treatment assignment” (Austin, 2011, p. 408). Each individual in the sample is assigned a weight that is the inverse of the probability of having received their particular treatment (Austin, 2011; Cole & Hernan, 2008; Hirano et al., 2003; Thoemmes & Ong, 2016). Thus, individuals who received the treatment are given a weight of $1/P(Z=1|X)$ and those receiving the control receive $1/1-P(Z=1|X)$ (Thoemmes & Ong, 2016).

Inverse probability weighting “creates a pseudo-population in which the exposure is independent of the measured confounders” (Cole & Hernan, 2008). Inverse probability weighting with regression adjustment “is conceptually identical to running an unweighted, regular regression model in the pseudo-population in which confounders and treatment are independent of each other” (Thoemmes & Ong, 2016, p. 42). In practice, inverse probability weighting with regression adjustment (IPWRA) uses two models, one to predict assignment to treatment, and the other to predict the specified outcome. Due to the setup of two different models, a different set of covariates can be used to predict each model. In practice, this is often the case as the variables expected to predict treatment assignment could be different from those expected to predict the outcome. The

combination of inverse probability weighting and regression adjustment can “achieve some robustness to misspecification of the parametric models” (Wooldridge, 2010, p. 930). The results of the IPWRA models are “doubly robust” as only one of the models, either the propensity score model or the outcome model, must be correctly specified to produce unbiased estimates (Imbens & Wooldridge, 2008; Wooldridge, 2010).

Inverse Probability Weighting with Survival Outcomes

There are specific challenges when an outcome is survival time or time to a specified event. Within criminology, survival analyses have been used in the area of terrorism research to examine the impact of interventions on time to subsequent terrorist events such as hijackings (Dugan et al., 2005). As highlighted by Rodriguez (2010), these survival models are used for data that have three main components: “(1) the dependent variable or response is the waiting *time* until the occurrence of a well-defined event, (2) observations are *censored*, in the sense that for some units the event of interest has not occurred at the time the data are analyzed, and (3) there are predictors or *explanatory* variables whose effect on the waiting time we wish to assess or control” (p. 1).

In the case of the current study, there are two potential sources of censoring. The first is that the data is censored because misconduct data were only gathered through August 2017. Thus, any individual who is released from disciplinary custody and did not commit an additional misconduct prior to August 2017 is right censored. The other source of censoring is the potential for an individual to be released from PADOX prior to committing an additional misconduct while incarcerated. Of the full disciplinary custody sample (n=4,425), 1,126 (25.45%) do not have a misconduct following release from their

first disciplinary custody stay. Logistic regression does not adequately address the issues of these censored data.

In survival analysis, the ATE is “the mean difference in survival time because of the treatment” (Austin, 2014, p. 1244). Within IPWRA models, there are two potential methods for estimating a model with a survival outcome, likelihood-adjusted censoring (LAC-IPWRA) vs. weighted-adjusted censoring (WAC-IPWRA). The main difference between the two is that the weighted-adjusted censoring model requires a separate model predicting the time-to-censoring whereas the likelihood-adjusted censoring model includes a log-likelihood function for the outcome in order to adjust for censoring in the outcome (Pope, 2015). As weighted-adjusted censoring models require the separate modeling of the censoring process, these models are generally less robust than the likelihood-adjusted censoring models. Additionally, weight-adjusted censoring imposes a more restrictive assumption that the censoring process is random. Because the LAC-IPWRA estimators use a log-likelihood term instead of weights to adjust for censoring, these models do not require this stricter assumption (Pope, 2015). For this reason, the current study utilizes the likelihood-adjusted censoring model to examine the models estimating days to follow-up misconduct.

Current Study

The current study focuses on two aspects of heterogeneity in disciplinary custody and examines outcomes with two measures of deterrence. All models use inverse probability weighting with regression adjustment, with modifications made for multivalued treatments and survival outcomes as needed. Table 3 outlines all the analyses, both for the main and supplemental results, as well as the sample sizes and

models used for each analysis. First, I examine the relationship between length of stay within a disciplinary custody stay and a binary outcome of misconduct within one year of release from disciplinary custody, followed by examining the role between length of stay and the days to subsequent misconduct. Next, I examine the effect of a binary indicator of early release on both the binary outcome of misconduct and the days to subsequent misconduct measure. Lastly, I examine early release further by breaking down early release into a multivalued treatment, examining the impact of differences in proportion of time served in disciplinary custody on both the binary recidivism outcome and the days to subsequent misconduct. Together, these six analyses examine each of the six research hypotheses.

Table 3. Analysis Summary

	Sample Size	Model
Main Analyses		
Length of Stay and Binary Misconduct	4,425	IPWRA
Length of Stay and Days to Misconduct	4,425	IPWRA with survival outcomes
Binary Early Release and Binary Misconduct	3,275	IPWRA
Binary Early Release and Days to Misconduct	3,275	IPWRA with survival outcomes
Multivalued Early Release and Binary Misconduct	3,275	IPWRA
Multivalued Early Release and Days to Misconduct	3,275	IPWRA with survival outcomes
Supplemental Analyses		
Change in Severity		
Length of Stay	2,792	IPWRA
Binary Early Release	2,045	IPWRA
Multivalued Early Release	2,045	Model did not converge
Full Sample Early Release		
Binary Early Release and Binary Misconduct	4,014	IPWRA
Binary Early Release and Days to Misconduct	4,014	IPWRA with survival outcomes
Multivalued Early Release and Binary	4,014	IPWRA

Misconduct			
Multivalued Early Release and Days to Misconduct	4,014	IPWRA with survival outcomes	
Early Release as a Proportion of Median Time Served			
Binary Early Release and Binary Misconduct	4,014	IPWRA	
Binary Early Release and Days to Misconduct	4,014	IPWRA with survival outcomes	
Multivalued Early Release and Binary Misconduct	4,014	IPWRA	
Multivalued Early Release and Days to Misconduct	4,014	IPWRA with survival outcomes	

For the first type of heterogeneity examined, the focus is on the impact, conditional on an individual receiving a disciplinary custody stay, of the marginal effect of length of stay on future misconduct. First, I examined the baseline comparisons between the covariates by treatment category. Next, I ran the inverse probability weighting with regression adjustment models using the “teffects ipwra” code in Stata. This model first estimates the propensity for receiving a specific treatment (i.e., being in a specific quartile of the length of stay variable). It then calculates the inverse probability of being placed in each treatment and uses this probability to weight and re-estimate the outcome model. These models are then run to examine all six pairwise ATEs to examine the relative ATE between each possible category of the treatment. Prior to a discussion of the results of these models, I examined the balance of the covariates after weighting, paying particular attention to the covariates that were out of balance prior to weighting. The binary early release models only have one ATE of interest as it measures a binary treatment, but the non-binary early release model also designated by quartiles will have the same six pairwise ATEs as the length of stay analyses described above.

The multivalued treatment models (length of stay and multivalued early release) are estimated using a multinomial logit model, and the binary treatment model (binary

early release) is estimated using a logit model. For the outcomes, the binary outcome model (subsequent misconduct) is estimated using a logit model and the survival outcome of days to misconduct is estimated using a Weibull model.

For the IPWRA models, and for propensity score models as a whole, the treatment is considered independent of the covariates included in the model (Rosenbaum & Rubin, 1983; 1984). In the current study, all covariates are measured prior to the disciplinary custody stay. Thus, both the length of stay and early release treatments are only independent from the covariates observed prior to their first disciplinary custody stay. It is possible, and even likely given what is considered in the early release decision, that behavior within disciplinary custody is evaluated and factored into the early release decision. This is a limitation of the current study, and the implications of this issue will be discussed further in the limitations section below. Additionally, this method is not able to account for change over the treatment. Given the intolerable nature of stays in solitary confinement, and the documented profound physical and mental health deterioration that comes with these environments (Haney, 2018), it is important to acknowledge that this modeling approach cannot account for any differences in the individual between entering and exiting disciplinary custody. Scholars often highlight change in incarceration research in terms of age or maturity (Snodgrass et al., 2011); although the vast majority of disciplinary custody stays are in days or weeks, there are some who spend years within isolation, and come back to the general population older and at a lower risk of subsequent misbehavior. It is arguably more possible that subjects may experience psychological changes during their stay, which should be considered when drawing inferences from the results.

Chapter 4: Results

Multivalued Length of Stay

Likelihood of Subsequent Misconduct

The first model of interest examines heterogeneity in length of stay and the likelihood of committing an additional misconduct within one year of release from disciplinary custody. The length of stay treatment was broken down by the quartiles of the distribution: 1-21, 22-30, 31-55, and 56-1320 days. Table 4 shows the standardized differences and variance ratios for all covariates in the length of stay model both before and after weighting using the inverse probability of receiving each treatment. This table shows the covariate balance summary for each quartile relative to the first length of stay quartile. A covariate is identified as balanced when the standardized difference is close to zero and the variance ratio is close to 1. Prior to weighting, it is clear that several covariates are out of balance. Particularly, variables such as *Unknown Custody Level*, current misconduct offense type indicators, and a few of the institution dummy variables are out of balance prior to weighting. Together, the differences across the dosage levels indicate the potential for selection bias and the importance of accounting for such differences prior to making a conclusion of the deterrent capabilities of differential lengths of stay.

Table 4. Raw and Weighted Covariate Balance Summary for Length of Stay (n=4,425)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
2nd Quartile				
Age at First DC Stay	-0.0488	-0.0068	0.9590	1.0527
Black	0.0339	0.0330	0.9928	0.9926
Hispanic	0.0364	0.0127	1.0975	1.0316
Other Race	-0.0508	-0.0255	0.4440	0.7298
Married	0.0044	-0.0113	1.0123	0.9667
Divorced, Separated, Widowed	-0.0352	0.0224	0.8677	1.0934
Unknown Marital Status	-0.0515	-0.0346	0.5185	0.6849
Custody Level Three	-0.0704	0.0319	0.9741	1.0124
Custody Level Four	0.0424	0.0028	1.0532	1.0038
Custody Level Five	-0.0330	-0.0876	0.8820	0.7518
Unknown Custody Level	0.1055	0.0134	1.1382	1.0167
Incarcerated Days	0.0654	0.0302	1.0741	0.9657
Property Incarceration Offense	-0.0605	-0.0074	0.8671	0.9833
Drug Incarceration Offense	-0.0117	-0.0014	0.9737	0.9969
Other Incarceration Offense	0.0354	0.0100	1.0570	1.0164
Missing Incarceration Offense	0.0608	0.0185	1.1608	1.0448
Highest Prior MH C	0.0420	-0.0007	1.0663	0.9989
Highest Prior MH D	-0.0601	-0.0158	0.8399	0.9452
Total Prior Violent Program	-0.0845	0.0056	0.6775	0.9481
Total Prior Alcohol/Drug Program	-0.0691	-0.0150	0.6908	0.9382
Total Prior Other Program	-0.0094	0.0047	1.0026	0.9104
Current Violent Program	0.0564	0.0279	1.3168	1.1617
Current Alcohol/Drug Program	0.0767	0.0559	1.4802	1.3531
Current Other Program	0.0371	0.0328	1.3129	1.2684
Worked at Time of First DC Stay	0.0145	0.0551	0.9843	0.9449
Current Property Misconduct	-0.0119	-0.0043	0.9694	0.9892
Current Drug Misconduct	0.1475	-0.0028	2.8751	0.9848
Current Prison Rule Misconduct	-0.2373	0.0194	1.0156	1.0071
Current Other Misconduct	-0.0164	0.0034	0.9675	1.0073
Prior Misconduct Count	-0.0476	0.0450	0.8255	1.0446
Albion	0.0265	0.0138	1.1651	1.1006
Benner Township	0.0546	0.0344	1.2505	1.1698
Cambridge Springs	-0.0463	-0.0129	0.7195	0.9047
Chester	0.1124	0.0601	1.9787	1.4033
Coal Township	0.0261	0.0492	1.1564	1.3155
Dallas	0.0809	-0.0384	1.5631	0.8332
Fayette	0.0484	0.0039	1.2346	1.0189
Forest	-0.1343	0.0000	0.5469	1.0002

Frackville	0.1526	-0.0202	4.0528	0.8705
Graterford	-0.0766	-0.0629	0.7595	0.8434
Greene	0.0194	0.0048	1.1889	1.0352
Houtzdale	-0.1411	0.0028	0.5663	1.0124
Huntingdon	0.0465	0.0212	1.2475	1.1181
Laurel Highlands	-0.0047	0.0083	0.9744	1.0564
Mahanoy	-0.0261	0.0113	0.8372	1.0798
Mercer	-0.1884	0.0112	0.1603	1.0787
Muncy	0.1011	-0.0269	1.5497	0.8955
Pine Grove	0.0382	-0.0061	1.2042	0.9741
Pittsburgh	-0.0479	0.0310	0.7752	1.1663
Retreat	-0.0259	-0.0198	0.8056	0.8462
Rockview	-0.0960	0.0035	0.6691	1.0176
Smithfield	0.0737	0.0217	1.6043	1.1801
Somerset	0.0289	0.0155	1.0982	1.0618
Waymart	0.2386	0.0233	4.7850	1.1392
3rd Quartile				
Age at First DC Stay	-0.1655	-0.0172	0.7556	1.0253
Black	0.0123	0.0282	0.9978	0.9938
Hispanic	0.0448	0.0207	1.1204	1.0515
Other Race	0.0217	-0.0288	1.2982	0.6978
Married	-0.1076	-0.0134	0.7110	0.9603
Divorced, Separated, Widowed	-0.0042	0.0059	0.9838	1.0241
Unknown Marital Status	-0.0199	-0.0429	0.7980	0.6174
Custody Level Three	-0.0148	0.0097	0.9955	1.0040
Custody Level Four	-0.0571	0.0440	0.9253	1.0576
Custody Level Five	0.0380	-0.0863	1.1422	0.7554
Unknown Custody Level	0.0686	0.0103	1.0917	1.0129
Incarcerated Days	-0.1663	0.0318	0.9642	0.9616
Property Incarceration Offense	0.0152	-0.0173	1.0339	0.9608
Drug Incarceration Offense	0.0396	-0.0211	1.0901	0.9524
Other Incarceration Offense	-0.0347	-0.0030	0.9434	0.9950
Missing Incarceration Offense	0.0923	0.0112	1.2457	1.0271
Highest Prior MH C	-0.0669	-0.0243	0.8922	0.9597
Highest Prior MH D	-0.2149	-0.0104	0.4658	0.9639
Total Prior Violent Program	-0.0832	0.0257	0.6548	0.9744
Total Prior Alcohol/Drug Program	-0.0654	-0.0039	0.8071	1.1159
Total Prior Other Program	-0.0258	-0.0012	1.0460	0.9546
Current Violent Program	-0.0684	0.0301	0.6664	1.1752
Current Alcohol/Drug Program	0.0172	0.0310	1.1002	1.1900
Current Prior Other Program	-0.0233	0.0292	0.8242	1.2378
Worked at Time of First DC Stay	-0.0010	0.0425	1.0012	0.9579

Current Property Misconduct	-0.0196	0.0115	0.9499	1.0289
Current Drug Misconduct	0.1595	0.0224	3.0753	1.1235
Current Prison Rule Misconduct	-0.4282	-0.0311	0.9497	0.9872
Current Other Misconduct	-0.0307	-0.0150	0.9392	0.9681
Prior Misconduct Count	-0.1972	0.0013	0.6494	0.9956
Albion	-0.1119	0.0142	0.4237	1.1035
Benner Township	-0.1145	0.0309	0.5486	1.1522
Cambridge Springs	0.0439	0.0016	1.3038	1.0119
Chester	0.1262	0.0548	2.1214	1.3653
Coal Township	0.0544	0.0574	1.3381	1.3720
Dallas	0.0209	-0.0385	1.1342	0.8328
Fayette	-0.0159	-0.0040	0.9276	0.9810
Forest	-0.2015	-0.0112	0.3557	0.9456
Frackville	0.1446	-0.0195	3.8337	0.8745
Graterford	0.1169	-0.0821	1.4031	0.7969
Greene	0.0940	0.0144	2.0750	1.1063
Houtzdale	-0.1921	-0.0007	0.4298	0.9970
Huntingdon	-0.0632	0.0268	0.7016	1.1503
Laurel Highlands	-0.0744	-0.0023	0.6250	0.9849
Mahanoy	0.0322	0.0109	1.2193	1.0769
Mercer	0.0172	0.0188	1.1002	1.1343
Muncy	0.1445	-0.0094	1.8161	0.9629
Pine Grove	-0.0403	0.0287	0.8023	1.1247
Pittsburgh	0.0542	0.0263	1.2821	1.1408
Retreat	-0.0148	-0.0189	0.8867	0.8529
Rockview	-0.1692	-0.0090	0.4486	0.9559
Smithfield	-0.0432	0.0158	0.7083	1.1301
Somerset	-0.2556	-0.0071	0.2752	0.9723
Waymart	0.1138	0.0293	2.4283	1.1763
4th Quartile				
Age at First DC Stay	-0.2415	0.1012	0.7691	1.1311
Black	0.0904	0.0203	0.9760	0.9956
Hispanic	0.0601	-0.0274	1.1620	0.9325
Other Race	0.0273	-0.0266	1.3819	0.7194
Married	-0.0397	0.0145	0.8905	1.0431
Divorced, Separated, Widowed	-0.1026	0.0137	0.6343	1.0567
Unknown Marital Status	0.0164	-0.0449	1.1831	0.6013
Custody Level Three	-0.0810	0.0424	0.9695	1.0159
Custody Level Four	0.0160	0.0023	1.0204	1.0030
Custody Level Five	0.0267	-0.0969	1.0994	0.7267
Unknown Custody Level	0.0872	0.0162	1.1154	1.0201
Incarcerated Days	-0.0408	0.0402	0.9191	0.8818

Property Incarceration Offense	-0.0455	-0.0628	0.8998	0.8589
Drug Incarceration Offense	-0.0069	0.1096	0.9846	1.2500
Other Incarceration Offense	-0.0177	-0.0454	0.9712	0.9247
Missing Incarceration Offense	0.0316	0.0347	1.0831	1.0842
Highest Prior MH C	-0.0289	-0.0329	0.9537	0.9454
Highest Prior MH D	-0.3111	0.2017	0.2657	1.7735
Total Prior Violent Program	-0.0411	0.0911	0.7283	1.0201
Total Prior Alcohol/Drug Program	-0.0943	-0.0289	0.6335	0.8568
Total Prior Other Program	0.0743	0.0039	1.0913	0.7712
Current Violent Program	0.0237	-0.0184	1.1289	0.8990
Current Alcohol/Drug Program	0.0437	0.0306	1.2631	1.1871
Current Other Program	0.0616	0.0282	1.5426	1.2288
Worked at Time of First DC Stay	0.0122	0.0480	0.9868	0.9523
Current Property Misconduct	0.0656	-0.0385	1.1718	0.9043
Current Drug Misconduct	0.3045	-0.0062	5.9957	0.9670
Current Prison Rule Misconduct	-0.7867	0.1105	0.6952	1.0303
Current Other Misconduct	-0.1085	-0.0041	0.7856	0.9911
Prior Misconduct Count	-0.0772	0.0201	0.7877	0.9674
Albion	-0.0850	0.0878	0.5452	1.7187
Benner Township	0.0414	-0.0080	1.1879	0.9617
Cambridge Springs	-0.1643	0.1451	0.1689	2.3936
Chester	0.1515	0.0431	2.3967	1.2825
Coal Township	0.0488	0.0250	1.3014	1.1553
Dallas	0.1411	-0.0636	2.0583	0.7301
Fayette	-0.1144	-0.0012	0.5269	0.9943
Forest	-0.1073	-0.0148	0.6306	0.9283
Frackville	0.1371	-0.0343	3.6337	0.7841
Graterford	0.0736	-0.0940	1.2496	0.7683
Greene	0.1594	-0.0076	3.0731	0.9461
Houtzdale	-0.1018	0.0005	0.6790	1.0023
Huntingdon	-0.0426	0.0648	0.7943	1.3790
Laurel Highlands	-0.1414	0.0078	0.3455	1.0529
Mahanoy	-0.0136	-0.0194	0.9134	0.8700
Mercer	-0.0149	-0.0034	0.9163	0.9764
Muncy	-0.0517	0.0552	0.7587	1.2272
Pine Grove	0.2946	-0.0117	2.9552	0.9509
Pittsburgh	0.0195	-0.0252	1.0980	0.8726
Retreat	-0.0656	0.0142	0.5425	1.1181
Rockview	-0.2119	0.0056	0.3326	1.0280
Smithfield	-0.0331	-0.0161	0.7727	0.8754
Somerset	-0.0645	-0.0153	0.7919	0.9403
Waymart	0.1888	0.0091	3.7460	1.0530

To address the issues of selection bias inherent in the current study, I utilized inverse probability weighing with regression adjustment (IPWRA). For the first model, length of stay, the treatment was broken down into the quartiles of the length of stay distribution, and the outcomes was a binary indicator of whether the individual committed a misconduct within one year of release from disciplinary custody. The IPWRA requires the modeling of both the treatment and the outcome in separate models. This model was estimated and the results of the six pairwise ATEs are shown in Table 5.

Prior to a discussion of the results, it is important to first examine and check the assumptions made by these models. First, to trust the results of IPWRA, one must examine the balance of the covariates after weighting. Table 4 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates, namely the majority of the standardized differences are closer to zero and the variance ratios are closer to 1. However, there are a few notable exceptions. For example, the variables *Other Race* and *Custody Level 5* are generally out of balance after the model has been weighted. While this is not an ideal scenario, some covariate imbalance is to be expected, particularly due to the large number of covariates used in the current analyses. Even if length of stay were randomly assigned, at an $\alpha=.05$, one would expect about 2 or 3 of the 54 covariates to remain out of balance.

One of the main concerns with using inverse probability weighting is the possibility for extreme weights that can result in large and unstable estimates (Lopez & Gutman, 2017). The `teffects` code in Stata has an automatic overlap violator error that identifies all individuals with a small probability (it flags those with propensity scores

less than $1.00e-.05$) of receiving the treatment. No such small propensity scores were identified in the length of stay analyses. Another check to the overlap assumption is the examination of overlap plots. Figure 4 displays the estimated densities of the probability of receiving each treatment level for all the length of stay analyses. As described above, one of the assumptions inherent to IPWRA is the requirement of a non-zero probability of receiving each level of the treatment. As can be seen in Figure 4, the overlap assumption is satisfied in that there are observations in all four treatment categories at each combination of covariate values.

Figure 4. Length of Stay Overlap Graph (n=4,425)

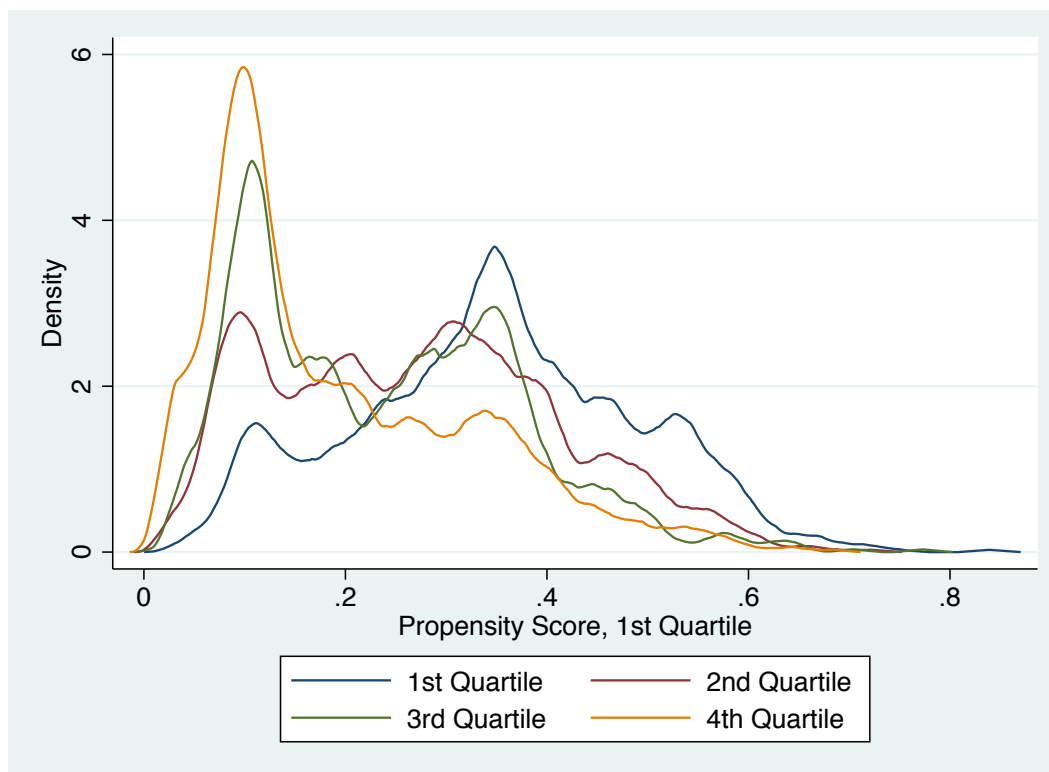


Table 5. Length of Stay and Binary Misconduct Pairwise ATEs (n=4,425)

	ATE	Robust SE	Z	P> z	95% CI	
2 vs. 1	-.0368	.0217	-1.70	0.089	-.0792	.0057
3 vs. 1	-.0218	.0221	-0.98	0.325	-.0652	.0216
4 vs. 1	.0497	.0224	2.22	0.026	.0059	.0936
POmean 1	.6293	.0160	39.45	0.000	.5980	.6606
3 vs. 2	.0150	.0214	0.70	0.484	-.0270	.0569
4 vs. 2	.0865	.0217	3.99	0.000	.0441	.1290
POmean 2	.5925	.0150	36.63	0.000	.5632	.6218
4 vs. 3	.0715	.0221	3.24	0.001	.0282	.1149
POmean 3	.6075	.0156	38.93	0.000	.5769	.6381
POmean 4	.6790	.0160	42.53	0.000	.6477	.7103

After these checks have satisfied the assumptions of inverse probability weighting, the estimated pairwise ATEs can be examined and discussed. IPWRA results display two types of parameters of interest: ATEs (discussed above) and the POM. The POM for each treatment level is the average of each potential outcome. For example, in Table 5, the POM of the first quartile is 62.9%, meaning on average, those in the first quartile have a likelihood of committing a subsequent misconduct post-release from disciplinary custody of 62.9%. As shown in Table 5, there are statistically significant differences in the length of stay quartiles. The likelihood of a follow-up misconduct within a year of release from the first disciplinary custody stay is .05 higher for those in the fourth quartile of length of stay relative to those in the first. The probability of recidivism in the 1st quartile is 62.9%. The change in probability is additive, so the estimated probability of recidivism within a year of release from the first DC stay for those in the 4th quartile is 67.9%. Another result of significance comes from the pairwise ATE of the 2nd and 4th quartiles. The likelihood of a follow-up misconduct within a year

of release from the first DC stay is .09 higher for those in the fourth quartile (67.9%) of LOS relative to those in the second quartile (59.3%). Lastly, the likelihood of a follow-up misconduct is .07 higher for those in the fourth quartile of length of stay relative to those in the third quartile (60.8%). The remaining pairwise ATEs do not show statistically significant mean differences between the first, second, or third quartiles. These results show that those in the fourth quartile of length of stay, meaning they served between 56 and 1,320 days in their first DC stay, were significantly more likely to commit a misconduct within a year of their release than those who served fewer days. These results do not support Hypothesis 1. Rather than longer lengths of stay in disciplinary custody being associated with larger deterrent effects, those serving the longest sanctions have higher likelihoods of committing a subsequent misconduct within a year of release from disciplinary custody.

Number of Days to Subsequent Misconduct

In examining the second outcome of interest, days to subsequent misconduct, it is necessary to account for the censoring inherent in this outcome. In the current study, there are two sources of potential right censoring: those who were released from PADOX prior to committing a subsequent misconduct following their return to the general inmate population from disciplinary custody, and those who did not commit a subsequent misconduct prior to August 2017 when data collection ceased. Among the sample of 4,425, 3,299 (74.6%) committed at least one misconduct following release from their first disciplinary custody stay.

Table 4 shows the standardized differences and variance ratios for all covariates in the length of stay models both before and after weighting using the inverse probability

of receiving each treatment. This table shows the covariate balance summary for each quartile relative to the first length of stay quartile. A covariate is identified as balanced when the standardized difference is close to zero and the variance ratio is close to 1. Prior to weighting, many covariates are out of balance. For example *Current Prison Rule Misconduct* consistently has a large standardized difference across the quartiles relative to the first quartile.

To address the issues of selection bias inherent in the current study, I utilized IPWRA for survival outcomes. The treatment indicator was multivalued length of study and the outcome was a survival outcome of days to subsequent misconduct following release from disciplinary custody. IPWRA requires the modeling of both the treatment and the outcome in separate models. These models were estimated and the results of the six pairwise ATEs are shown in Table 6.

Table 6. Length of Stay and Days to Misconduct Pairwise ATEs (n=4,425)

	ATE	Robust SE	z	P> z	95% CI	
2 vs. 1	291.4568	313.3193	0.93	0.352	-322.6378	905.5514
3 vs. 1	176.9597	103.4585	1.71	0.087	-25.8153	397.7347
4 vs. 1	-112.5362	79.2257	-1.42	0.155	-267.8158	42.7434
POmean 1	627.313	56.8489	11.03	0.000	515.8911	738.7348
3 vs. 2	-114.4973	319.2875	-0.36	0.720	-740.2893	511.2948
4 vs. 2	-403.9928	313.2314	-1.29	0.197	-1017.915	209.9295
POmean 2	918.7698	308.3975	2.98	0.003	314.3218	1523.218
4 vs. 3	-289.4959	103.1417	-2.81	0.005	-491.6499	-87.3418
POmean 3	804.2727	87.0071	9.24	0.000	633.7544	974.7909
POmean 4	514.7768	56.2128	9.16	0.000	404.6017	624.9519

Prior to a discussion of the results, I first checked the assumptions required by IPWRA. First, I examined the balance of the covariates after weighting. Table 4 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates,. However, there are a few exceptions. For example, the variables *Custody Level 5* and *Unknown Marital Status* are consistently out of balance after the model has been weighted. Some covariate imbalance is to be expected, particularly due to the large number of covariates used in the current analyses. As mentioned previously, if length of stay were randomly assigned one would still expect about 2 or 3 of the 54 covariates to remain out of balance. As mentioned above, there were no overlap violators identified in the length of stay analyses (i.e. no individual had a propensity score less than $1.00e-.05$ for placement in any of the quartiles of length of stay). Also as discussed previously, Figure 4 shows there is overlap between the four treatment categories.

After these checks have satisfied the assumptions of inverse probability weighting, the estimated pairwise ATEs can be examined and discussed. As shown in Table 6, there is only one statistically significant relationship between the 3rd and 4th quartiles. The average time to follow-up misconduct is 289 days less for those in the fourth quartile relative to those in the third quartile (i.e., those individuals who served 56 or more days versus between 31 and 55 days, respectively). The remaining pairwise ATEs are not statistically significant, although there is a marginally significant ($\alpha < 0.1$) mean difference between the third and first quartiles. These findings demonstrate there is something unique about the group serving between 31 and 55 days

in their first disciplinary custody stay relative to some of the other quartiles. These results do not support Hypothesis 2. Rather than longer disciplinary custody stays being associated with restrictive deterrent effects, those in the 4th quartile have fewer days until their first subsequent misconduct relative to those serving shorter disciplinary custody stays, although only the difference between the 3rd and 4th quartiles is statistically significant. These results are substantively consistent with the prior findings of the relationship between length of stay and the probability of a follow-up misconduct.

Binary Early Release

The next set of analyses focus on the binary early release treatment. As discussed above, the current operationalization of early release is anyone who served less than 100% of their assigned disciplinary custody sanction length. Thus, everyone who served between .01% and 99.99% of their original sanction are grouped as the treatment category and the comparison group are those who served exactly 100% of their original sanction length. This measurement decision results in a decrease in the sample of the remaining main analyses. The exclusion of those serving more than 100% of their original sanction length from the comparison group reduces the original disciplinary custody sample by 720 individuals and excluding those offenses that are not eligible for early release (discussed above in the methodology section) further reduces the sample by 411 individuals. The method used in the current analyses requires each individual to have a non-zero probability of being placed in each potential treatment category. For the multivalued early release analyses (described below), 19 individuals did not meet this threshold and were thus excluded from analyses for both the binary and non-binary early release analyses. Thus, the final analysis sample for the early release mechanism is 3,275

individuals; 1,162 of whom served exactly 100% of their original sanction length and 2,132 of whom were released prior to serving 100% of the sanction length.

Likelihood of Subsequent Misconduct

Prior to estimating the model, I examined the balance for all covariates across the treatment and comparison groups and these results are shown in Table 7. Because these analyses only have one treatment, binary early release, the comparison is between those who were released prior to serving their full sanction length and those serving exactly 100% of their original sanction length. This table shows that there are covariate differences that exist between those who are released early and those who serve the full 100% of their original sanction length. These include the number of days incarcerated prior to the first disciplinary custody stay, both mental health indicators, and several institution dummy variables. Regardless of which indicator of imbalance is used, there are more covariates out of balance than what one would expect if early release were randomly assigned.

Table 7. Raw and Weighted Covariate Balance Summary for Binary Early Release
(n=3,275)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
Age at First DC Stay	-0.0598	-0.0124	0.8707	0.9305
Black	-0.0454	0.0142	1.0146	0.9961
Hispanic	0.0441	-0.0279	1.1199	0.9354
Married	0.0359	-0.0773	1.1120	0.8173
Divorced, Separated, Widowed	-0.0192	-0.0177	0.9211	0.9291
Unknown Marital Status	0.0296	0.0241	1.4488	1.3784
Custody Level Three	0.0588	-0.0234	1.0211	0.9920
Custody Level Four	0.0158	0.0670	1.0207	1.0991

Custody Level Five	-0.0767	0.0311	0.7643	1.1151
Unknown Custody Level	-0.0537	0.0025	0.9394	1.0030
Incarcerated Days	-0.2349	-0.0225	0.9016	0.9534
Property Incarceration Offense	0.0777	0.0310	1.2101	1.0811
Drug Incarceration Offense	-0.0206	0.0244	0.9553	1.0564
Other Incarceration Offense	-0.0194	-0.0077	0.9694	0.9878
Missing Incarceration Offense	0.0308	-0.0095	1.0772	0.9777
Highest Prior MH C	-0.1101	0.0171	0.8299	1.0316
Highest Prior MH D	0.1086	-0.0518	1.5478	0.8325
Total Prior Violent Program	-0.0017	0.0579	1.0370	1.1408
Total Prior Alcohol/Drug Program	-0.0278	0.0264	1.0225	1.1988
Total Prior Other Program	0.0061	0.0096	0.8624	0.8604
Current Violent Program	-0.0750	0.0063	0.6722	1.0356
Current Alcohol/Drug Program	-0.0959	-0.0122	0.6177	0.9388
Current Other Program	0.0218	0.0191	1.1766	1.1604
Worked at Time of First DC Stay	0.1961	0.0293	0.8272	0.9685
Current Property Misconduct	0.0175	-0.0733	1.0438	0.8483
Current Drug Misconduct	-0.0798	-0.0448	0.6947	0.8042
Current Prison Rule Misconduct	0.0607	0.0161	1.0128	1.0028
Current Other Misconduct	0.0078	0.0315	1.0158	1.0664
Prior Misconduct Count	-0.0748	0.0023	1.0089	1.1274
Albion	-0.1034	0.0066	0.5073	1.0440
Benner Township	-0.3536	0.0074	0.2423	1.0290
Cambridge Springs	0.0136	-0.0052	1.1294	0.9566
Chester	-0.0566	0.0050	0.7288	1.0291
Coal Township	0.1896	0.0177	3.5737	1.1031
Dallas	0.0487	0.0065	1.2921	1.0341
Fayette	-0.4821	-0.0410	0.0371	0.8199
Forest	0.2793	-0.0029	6.4135	0.9870
Frackville	0.0092	-0.0033	1.0641	0.9783
Graterford	-0.1470	0.0087	0.6539	1.0268
Greene	-0.0146	0.0100	0.9016	1.0754
Houtzdale	0.2968	-0.0011	5.0176	0.9956
Huntingdon	0.0512	0.0002	1.3078	1.0008
Laurel Highlands	-0.0446	0.0039	0.7681	1.0235
Mahanoy	0.1509	-0.0066	3.9704	0.9538
Mercer	0.0179	0.0065	1.2234	1.0756
Muncy	-0.1032	0.0046	0.6129	1.0224
Pine Grove	0.0185	0.0133	1.0754	1.0542
Pittsburgh	0.0398	0.0114	1.2771	1.0721
Retreat	0.1378	-0.0682	4.9668	0.5948
Rockview	0.1259	0.0142	1.8768	1.0686
Smithfield	-0.1277	0.0015	0.4239	1.0108

Somerset	-0.3997	0.0033	0.2263	1.0118
Waymart	-0.1355	0.0049	0.4706	1.0278

With regard to the examined outcomes, 61.54% of the early release treatment group committed a misconduct within one year of release from disciplinary custody compared to 59.38% of the comparison group (i.e., those who served exactly 100% of their determined sanction length). Thus, prior to matching, there is a difference between the two groups and those who are released early from disciplinary custody are more likely to commit a subsequent misconduct within a year. Based on the pre-existing differences between these two groups, and the pre-existing imbalance in multiple covariates of interest, prior to weighting the comparison and treatment groups are not directly comparable.

I ran a model examining the relationship between binary early release and binary misconduct within one year of release from disciplinary custody and these results are displayed in Table 8. Prior to a discussion of these results, it is first important to examine the assumptions made by the model. First, for binary treatments, there is an overidentification test for covariate balance within Stata.²⁸ This test performs a chi-squared test to examine the null hypothesis that the covariates are balanced. I performed this test using the Stata code “tebalance overid” after running the IPWRA model and the results are shown in Table 9. This test failed to reject the null hypothesis, indicating that the weighted model achieved balance across the covariates. In addition to this check, balance can be assessed by looking at the weighted columns of Table 7, which shows that the covariates are more balanced after the weighted model was run compared to the raw

²⁸ This test is not available for multivalued treatments.

comparisons. Lastly, the overlap between the comparison and treatment groups can be examined in Figure 5. Although densities vary, there is overlap across the distribution.

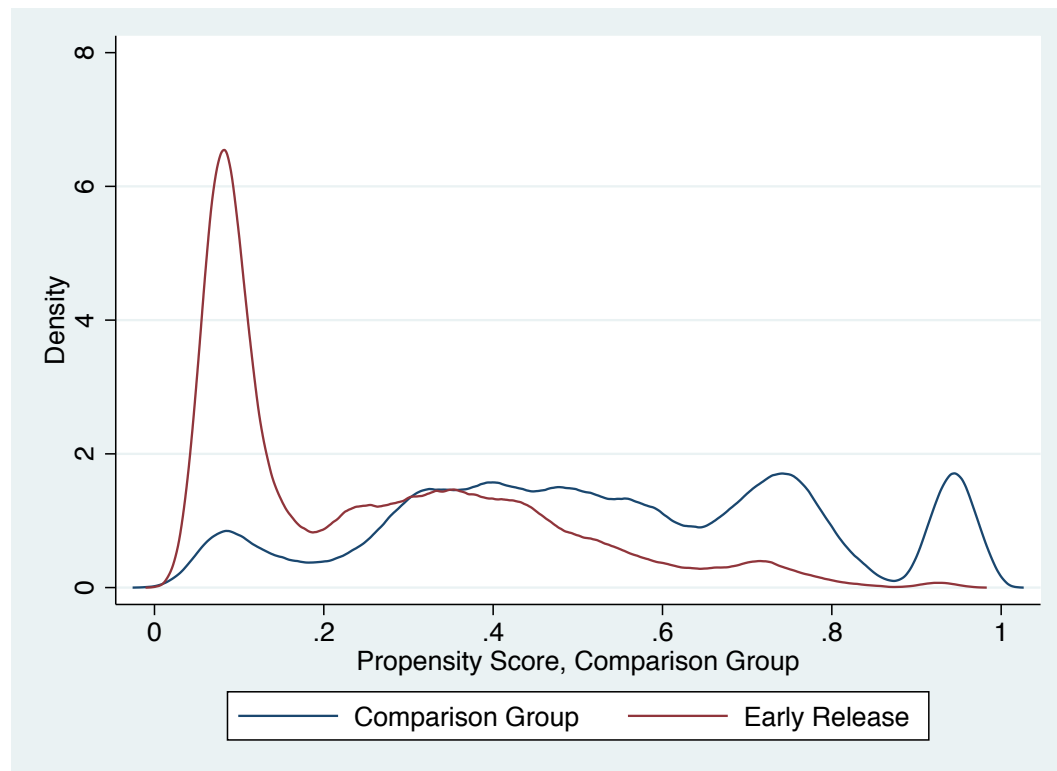
Table 8. Binary Early Release and Binary Misconduct Pairwise ATE (n=3,275)

	ATE	Robust SE	Z	P> z	95%CI	
Early Release (1 vs 0)	.0118	.0212	0.55	0.579	-.0298	.0533
POmean	.5993	.0174	34.37	0.000	.5652	.6335

Table 9. Binary Early Release and Binary Misconduct Overidentification Test (n=3,275)

chi2(54)	65.0508
Prob > chi2	0.1442

Figure 5. Binary Early Release Overlap Graph (n=3,275)



As can be seen in Table 8, there is no significant relationship between being released prior to serving 100% of the original sanction length relative to serving the full sanction length (exactly 100%) in likelihood of a follow-up misconduct within a year of release from disciplinary custody. This result does not provide support for Hypothesis 3a. In other words, there does not appear to be a deterrent effect of being released prior to the original completion date on the likelihood of committing a subsequent misconduct.

Number of Days to Subsequent Misconduct

Due to the censored nature of the days to subsequent misconduct outcome, basic linear regression does not adequately address the issues of right-censored data. Among the 3,275 individuals in the early release sample, 2,440 (74.5%) commit a subsequent misconduct prior to release from PADOH or prior to the end of the study period in August 2017. Prior to estimating the model using inverse probability weighting with regression adjustment for survival outcomes, I examined the balance for all covariates across the treatment and comparison groups and these results are shown in Table 7. As discussed above, this table shows that there are covariate differences that exist between those who are released early and those who serve the full 100% of their original sanction length. There are clear pre-existing differences in the early release decision between institutions, as evidenced by the imbalance among several of the institution dummy variables.

I ran a model examining the relationship between binary early release and days to subsequent misconduct within one year of release from disciplinary custody and these results are displayed in Table 10. Prior to discussing these results, I performed the chi-

squared overidentification test for covariate balance within Stata examining the null hypothesis that the covariates are balanced, and the results are shown in Table 11. This test failed to reject the null hypothesis, indicating that the weighted model achieved balance across the covariates. In addition to this check, balance can be assessed by looking at the weighted columns of Table 7, which shows that the covariates are more balanced after the weighted model was run compared to the raw comparisons. Lastly, the overlap between the comparison and treatment groups can be examined in Figure 5. Although densities vary, there is overlap across the distribution.

Table 10. Binary Early Release and Binary Misconduct Pairwise ATE (n=3,275)

	ATE	Robust SE	z	P> z	95%CI	
Early Release (1 vs 0)	-150.4292	112.6581	-1.34	0.182	-371.2351	70.3766
POmean	816.057	103.5468	7.88	0.000	613.1091	1019.005

Table 11. Binary Early Release and Days to Misconduct Overidentification Test (n=3,275)

chi2(54)	65.0460
Prob > chi2	0.1443

As can be seen in Table 10, there is no significant relationship between being released prior to serving 100% of the original sanction length relative to serving the full sanction length (exactly 100%) in days to a follow-up misconduct. This result does not provide support for Hypothesis 4a. As with examining the likelihood of a subsequent

misconduct, there is no evidence of a deterrent effect of a binary indicator of early release and time to follow-up misconduct.

Multivalued Early Release

One issue with the binary early release analyses described above is the large amount of variation that is captured in the binary treatment category. It is possible that differences in the percent of the original sanction served could have varying deterrent results. The next set of analyses examines this possibility by breaking down the binary treatment into a multivalued treatment using the quartiles of the early release distribution. Those quartile breakdowns are as follows: 0%-55.56%, 55.57%-86.66%, 86.67%-99.39%, and 99.44% to 100%.

Likelihood of Subsequent Misconduct

The first dependent variable of interest is a binary indicator of subsequent misconduct within one year of release from the first disciplinary custody stay. Table 12 shows the standardized differences and variance ratios for all covariates in the multivalued early release model both before and after weighting using the inverse probability of receiving each treatment. This table shows the covariate balance summary for each quartile relative to the first early release quartile. A covariate is identified as balanced when the standardized difference is close to zero and the variance ratio is close to 1. Prior to weighting, there are several covariates that are out of balance. Particularly, variables such as *Incarcerated Days*, *Highest Prior MH C*, *Highest Prior MH D*, and a few of the institution dummy variables are out of balance prior to weighting. Together, the differences across the different dosage levels indicate the potential for selection bias

and the importance of accounting for such differences prior to making a conclusion of the deterrent capabilities of differential early release.

Table 12. Raw and Weighted Covariate Balance Summary for Multivalued Early Release
(n=3,275)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
2nd Quartile				
Age at First DC Stay	0.0760	-0.0275	0.9249	0.8282
Black	-0.0187	-0.0454	1.0053	1.0128
Hispanic	0.0157	-0.0119	1.0402	0.9720
Married	0.0240	0.0181	1.0725	1.0516
Divorced, Separated, Widowed	0.0217	-0.0345	1.0991	0.8587
Unknown Marital Status	-0.0162	-0.0132	0.8323	0.8503
Custody Level Three	0.0785	-0.0377	1.0227	0.9875
Custody Level Four	-0.0251	0.0122	0.9675	1.0167
Custody Level Five	-0.0328	0.0574	0.8836	1.2118
Unknown Custody Level	-0.0450	0.0190	0.9454	1.0231
Incarcerated Days	0.2558	-0.0103	1.2224	0.8502
Property Incarceration Offense	0.0125	0.0194	1.0275	1.0475
Drug Incarceration Offense	0.0871	0.0390	1.2141	1.0884
Other Incarceration Offense	0.0514	-0.0548	1.0845	0.9166
Missing Incarceration Offense	-0.0039	-0.0166	0.9907	0.9602
Highest Prior MH C	0.1379	0.0008	1.3198	1.0015
Highest Prior MH D	-0.0587	-0.0162	0.8239	0.9428
Total Prior Violent Program	0.0730	-0.0096	1.0675	0.9332
Total Prior Alcohol/Drug Program	0.0453	-0.0186	0.8268	0.8611
Total Prior Other Program	-0.0568	-0.0258	0.6445	0.8830
Current Violent Program	-0.0808	0.0231	0.6045	1.1373
Current Alcohol/Drug Program	0.0993	-0.0405	1.8446	0.8145
Current Other Program	0.0733	0.0092	1.8149	1.0703
Worked at Time of First DC Stay	-0.1425	0.1053	1.1968	0.8940
Current Property Misconduct	0.1016	0.0292	1.2860	1.0743
Current Drug Misconduct	0.1116	-0.0302	1.9670	0.8593
Current Prison Rule Misconduct	-0.0743	-0.0475	0.9923	0.9950
Current Other Misconduct	-0.0626	0.0095	0.8841	1.0180
Prior Misconduct Count	-0.0012	-0.0145	0.8089	0.8286
Albion	0.0433	-0.0437	1.4027	0.7701

Benner Township	-0.0525	-0.0216	0.7109	0.9209
Cambridge Springs	0.0430	0.0118	1.5170	1.1143
Chester	0.0505	-0.0143	1.3831	0.9232
Coal Township	0.0271	0.0000	1.1246	0.9999
Dallas	0.0907	-0.0448	1.6572	0.8021
Fayette	0.0380	0.0921	1.9349	1.6380
Forest	-0.0661	-0.0007	0.7892	0.9966
Frackville	0.1707	0.0050	4.1041	1.0339
Graterford	-0.1493	0.0086	0.6225	1.0257
Greene	0.0603	0.0316	1.9283	1.2822
Houtzdale	-0.0946	0.0118	0.7331	1.0477
Huntingdon	0.1560	0.0048	3.0519	1.0257
Laurel Highlands	0.1398	-0.0143	2.6584	0.9220
Mahanoy	0.0971	0.0098	1.9996	1.0744
Mercer	-0.0274	-0.0043	0.7562	0.9517
Muncy	0.1247	-0.0126	1.9443	0.9424
Pine Grove	0.0284	-0.0016	1.1657	0.9936
Pittsburgh	0.0556	-0.0062	1.3527	0.9640
Retreat	0.1419	-0.0135	2.9705	0.8906
Rockview	0.0034	0.0048	1.0139	1.0221
Smithfield	-0.0835	-0.0160	0.4897	0.9027
Somerset	0.0531	0.0328	1.3658	1.1293
Waymart	0.1265	0.0048	2.8619	1.0280
3rd Quartile				
Age at First DC Stay	-0.0505	-0.0864	0.7728	0.6886
Black	-0.0137	0.0269	1.0051	0.9905
Hispanic	0.0527	-0.0126	1.1371	0.9704
Married	0.0641	-0.0115	1.1984	0.9675
Divorced, Separated, Widowed	0.0055	-0.0433	1.0259	0.8242
Unknown Marital Status	-0.0160	0.0021	0.8352	1.0253
Custody Level Three	0.0028	-0.0388	1.0021	0.9872
Custody Level Four	0.0013	0.0366	1.0028	1.0497
Custody Level Five	-0.0479	0.0831	0.8331	1.3105
Unknown Custody Level	0.0111	-0.0505	1.0143	0.9366
Incarcerated Days	0.3620	-0.0550	1.1120	0.8239
Property Incarceration Offense	-0.0997	-0.0731	0.7844	0.8244
Drug Incarceration Offense	0.0083	0.0084	1.0212	1.0189
Other Incarceration Offense	-0.0318	-0.0186	0.9479	0.9719
Missing Incarceration Offense	0.0005	0.0232	1.0024	1.0557
Highest Prior MH C	0.2021	-0.0449	1.4681	0.9198
Highest Prior MH D	-0.1722	-0.0505	0.5157	0.8262
Total Prior Violent Program	0.2201	0.0105	1.5540	0.9049

Total Prior Alcohol/Drug Program	0.0690	-0.0276	0.9548	0.9364
Total Prior Other Program	0.1566	-0.0004	1.3549	0.9141
Current Violent Program	0.0083	0.0231	1.0463	1.1376
Current Alcohol/Drug Program	0.1059	-0.0589	1.9124	0.7352
Current Other Program	0.1379	0.0002	2.7555	1.0013
Worked at Time of First DC Stay	-0.1359	0.1011	1.1897	0.8985
Current Property Misconduct	0.0885	0.0284	1.2498	1.0722
Current Drug Misconduct	0.2384	-0.0538	3.4575	0.7553
Current Prison Rule Misconduct	-0.1595	-0.0014	0.9714	0.9999
Current Other Misconduct	-0.1208	-0.1074	0.7777	0.7962
Prior Misconduct Count	0.0393	-0.0139	0.9684	1.1857
Albion	0.0069	-0.0484	1.0607	0.7471
Benner Township	0.0498	-0.0296	1.3152	0.8925
Cambridge Springs	0.1048	0.0210	2.4703	1.2073
Chester	0.0847	-0.0123	1.6795	0.9339
Coal Township	-0.1110	-0.0176	0.5518	0.9079
Dallas	0.1993	-0.0312	2.6517	0.8604
Fayette	-0.0014	0.0527	0.9731	1.3475
Forest	-0.1530	-0.0167	0.5384	0.9223
Frackville	0.1710	0.0226	4.1183	1.1574
Graterford	-0.2601	-0.0090	0.3770	0.9731
Greene	0.2828	0.0376	8.5224	1.3398
Houtzdale	-0.0432	0.0105	0.8761	1.0423
Huntingdon	0.3916	0.0150	8.2803	1.0805
Laurel Highlands	0.1006	-0.0278	2.1111	0.8512
Mahanoy	0.1764	0.0123	3.0915	1.0940
Mercer	-0.0701	0.0095	0.4352	1.1112
Muncy	-0.0467	-0.0383	0.7218	0.8291
Pine Grove	0.4389	0.0116	4.6031	1.0462
Pittsburgh	-0.0883	-0.0233	0.5375	0.8677
Retreat	-0.0028	-0.0530	0.9733	0.6015
Rockview	-0.0455	0.0542	0.8229	1.2639
Smithfield	-0.0612	-0.0125	0.6128	0.9238
Somerset	0.0186	0.0382	1.1234	1.1513
Waymart	0.1832	0.0115	4.0583	1.0677
4th Quartile				
Age at First DC Stay	0.0769	0.0066	1.0615	0.9559
Black	0.0373	-0.0325	0.9871	1.0095
Hispanic	-0.0278	0.0081	0.9295	1.0190
Married	-0.0137	0.0831	0.9590	1.2418
Divorced, Separated, Widowed	0.0285	0.0082	1.1311	1.0347
Unknown Marital Status	-0.0393	-0.0321	0.6182	0.6559

Custody Level Three	-0.0285	-0.0059	0.9884	0.9982
Custody Level Four	-0.0206	-0.0513	0.9730	0.9281
Custody Level Five	0.0534	-0.0103	1.1989	0.9636
Unknown Custody Level	0.0364	0.0096	1.0419	1.0116
Incarcerated Days	0.4143	0.0182	1.2569	0.9634
Property Incarceration Offense	-0.0927	-0.0276	0.7983	0.9328
Drug Incarceration Offense	0.0575	-0.0087	1.1404	0.9803
Other Incarceration Offense	0.0363	-0.0233	1.0596	0.9647
Missing Incarceration Offense	-0.0290	0.0016	0.9318	1.0039
Highest Prior MH C	0.2085	-0.0209	1.4802	0.9627
Highest Prior MH D	-0.1636	0.0329	0.5369	1.1192
Total Prior Violent Program	0.0822	-0.0596	1.1084	0.8194
Total Prior Alcohol/Drug Program	0.0586	-0.0400	0.8981	0.8225
Total Prior Other Program	0.0066	-0.0177	1.0797	1.1053
Current Violent Program	0.0460	0.0070	1.2607	1.0405
Current Alcohol/Drug Program	0.1585	-0.0216	2.4681	0.8993
Current Other Program	0.0415	-0.0184	1.4308	0.8660
Worked at Time of First DC Stay	-0.2815	0.0473	1.3544	0.9542
Current Property Misconduct	0.0420	0.0917	1.1162	1.2354
Current Drug Misconduct	0.1812	0.0079	2.7326	1.0382
Current Prison Rule Misconduct	-0.1253	-0.0567	0.9805	0.9935
Current Other Misconduct	-0.0539	-0.0530	0.9000	0.8991
Prior Misconduct Count	0.0803	-0.0212	0.9056	0.7885
Albion	0.1217	-0.0509	2.3100	0.7345
Benner Township	0.3454	-0.0210	3.8989	0.9232
Cambridge Springs	0.0286	0.0231	1.3312	1.2292
Chester	0.0949	-0.0155	1.7690	0.9166
Coal Township	-0.1976	-0.0190	0.2683	0.9006
Dallas	0.0359	-0.0423	1.2413	0.8127
Fayette	0.4930	0.0966	42.4108	1.6729
Forest	-0.3265	0.0162	0.1280	1.0776
Frackville	0.1097	0.0059	2.7205	1.0397
Graterford	0.0409	-0.0086	1.1102	0.9744
Greene	0.1229	0.0326	3.2606	1.2922
Houtzdale	-0.3376	0.0024	0.1725	1.0095
Huntingdon	0.1277	0.0093	2.5886	1.0494
Laurel Highlands	0.1269	-0.0229	2.4696	0.8769
Mahanoy	-0.0734	0.0108	0.4608	1.0822
Mercer	-0.0416	-0.0019	0.6415	0.9783
Muncy	0.1497	-0.0144	2.1679	0.9342
Pine Grove	0.1209	-0.0083	1.7822	0.9674
Pittsburgh	-0.0316	-0.0174	0.8209	0.9006
Retreat	-0.0764	0.0556	0.3617	1.5170

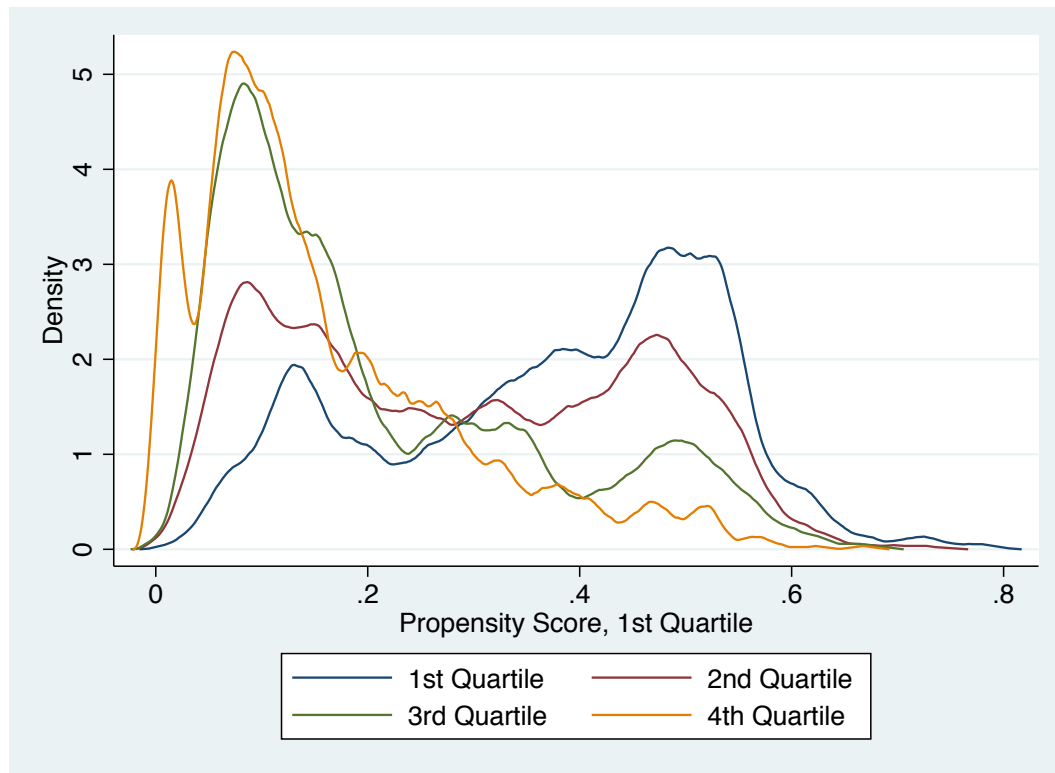
Rockview	-0.1336	-0.0111	0.5151	0.9492
Smithfield	0.0855	-0.0327	1.6836	0.8055
Somerset	0.4200	0.0293	5.1690	1.1154
Waymart	0.2244	0.0061	5.0490	1.0358

To address the issues of selection bias inherent in the current study, I utilized IPWRA. First, prior to examining the results of IPWRA, one must examine the balance of the covariates after weighting. Table 12 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates; namely the majority of covariates have standardized differences that are closer to zero and the variance ratios are closer to 1. However, there are a few exceptions. For example, *Age at First DC Stay* remains out of balance after the model has been weighted between both the 2nd and 3rd quartiles relative to the first quartile. Some covariate imbalance is to be expected, particularly due to the large number of covariates used in the current analyses.

Violations of the overlap assumption can result in large, unstable weights (Lopez & Gutman, 2017). The `teffects` code in Stata has an automatic overlap violator error that identifies all individuals with a small probability (it flags those with propensity scores less than 1.00e-.05) of receiving the treatment. As described above, there were 19 overlap violators identified in the multivalued early release analyses and these individuals were removed from all early release analyses to allow for symmetry in sample sizes between the various analyses. Another check to the overlap assumption is the examination of overlap plots. Figure 6 displays the estimated densities of the probability of receiving each treatment level for the multivalued binary release. These results satisfy the overlap

assumption in that there are observations in all four treatment categories at each combination of covariate values.

Figure 6. Multivalued Early Release Overlap Graph (n=3,275)



The estimated pairwise ATEs for this model are shown in Table 13. These results indicate there are no statistically significant differences between the various quartile comparisons in the likelihood of a subsequent misconduct within one year of release from disciplinary custody. These results do not provide support for Hypothesis 3b. Taken in conjunction with the binary early release model reported above, there does not appear to be a deterrent relationship between early release, measured as either a binary or a multivalued treatment, and the likelihood of a subsequent misconduct within one year of release from disciplinary custody.

Table 13. Multivalued Early Release and Binary Misconduct Pairwise ATEs (n=3,275)

	ATE	Robust SE	Z	P> z	95% CI	
2 vs. 1	-.0056	.0284	-0.20	0.843	-.0612	.0500
3 vs. 1	-.0438	.0337	-1.30	0.193	-.1098	.0222
4 vs. 1	-.0227	.0273	-0.83	0.406	-.0763	.0308
POmean 1	.6236	.0213	29.26	0.000	.588	.6654
3 vs. 2	-.0382	.0322	-1.19	0.235	-.1012	.0249
4 vs. 2	-.0171	.0255	-0.67	0.503	-.0671	.0329
POmean 2	.6180	.0190	32.51	0.000	.5807	.6552
4 vs. 3	.0211	.0312	0.68	0.499	-.0401	.0822
POmean 3	.5798	.0262	22.15	0.000	.5285	.6311
POmean 4	.6009	.0173	34.64	0.000	.5669	.6349

Number of Days to Subsequent Misconduct

In examining the second outcome, days to subsequent misconduct, it is necessary to account for censoring. In the current study, there are two sources of potential right censoring: those who were released from PADOc prior to committing a subsequent misconduct following their release from disciplinary custody, and those who did not commit a subsequent misconduct prior to August 2017 when data collection ceased. Among the early release sample of 3,275, 2,440 committed at least one misconduct following release from their first disciplinary custody stay.

Table 12 shows the standardized differences and variance ratios for all covariates in the multivalued early release and days to subsequent misconduct model both before and after weighting using the inverse probability of receiving each treatment. This table shows the covariate balance summary for each quartile relative to the first length of stay quartile. As described above, *Incarcerated Days* and several of the institution dummy variables consistently have large standardized differences across the quartiles relative to the first quartile.

To address the issues of selection bias inherent in the current study, I utilized IPWRA for survival outcomes. For the multivalued early release model, the treatment was broken down into the quartiles of the early release distribution, and the outcome was a survival outcome of days to subsequent misconduct following release from disciplinary custody. IPWRA requires the modeling of both the treatment and the outcome in separate models. These models were estimated and the results of the six pairwise ATEs are shown in Table 14.

Prior to a discussion of the results, I first checked the assumptions required by IPWRA. First, I examined the balance of the covariates after weighting. Table 12 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates, namely the majority of covariates standardized differences are closer to zero and the variance ratios are closer to 1. However, there are a few exceptions. For example, *Unknown Marital Status* remained out of balance between the 4th quartile relative to the 1st quartile. The overlap assumption specifies that all individuals must have a non-zero probability of receiving each treatment (Cole & Hernan, 2008; Wooldridge, 2010). As described above, there were 19 overlap violators identified in the multivalued early release analyses with a particularly small probability of receiving the treatment, and these individuals were removed from these analyses. Figure 6 displays the estimated densities of the probability of receiving each treatment level. As can be seen in Figure 6, there is overlap between the four treatment categories, satisfying this assumption.

As shown in Table 14, there are no statistically significant comparisons between the pairwise ATEs in the multivalued early release analyses. These results do not provide

support for Hypothesis 4b. Taken in conjunction with the binary early release results reported above, there does not appear to be a deterrent effect of early release, either measured as a binary or multivalued treatment, on time to subsequent misconduct.

Table 14. Multivalued Early Release and Days to Misconduct Pairwise ATEs (n=3,275)

	ATE	Robust SE	z	P> z	95% CI	
2 vs. 1	-48.1573	166.3057	-0.29	0.772	-374.1106	277.7959
3 vs. 1	-124.3849	153.6877	-0.81	0.418	-425.6072	176.8374
4 vs. 1	22.7592	154.5890	0.15	0.883	-280.2296	325.7481
POmean 1	786.6384	117.1311	6.72	0.000	557.0656	1016.2110
3 vs. 2	-76.2276	154.7654	-0.49	0.622	-379.5622	227.1071
4 vs. 2	70.9166	156.0753	0.45	0.650	-234.9853	376.8184
POmean 2	738.4810	119.3635	6.19	0.000	504.5328	972.4292
4 vs. 3	147.1441	141.3061	1.04	0.298	-129.8108	424.0990
POmean 3	662.2535	99.1242	6.68	0.000	467.9736	856.5334
POmean 4	809.3976	101.0211	8.01	0.000	611.3998	1007.3950

Supplemental Analyses

The results described above represent the main findings of the current project. However, there are areas of further exploration necessary to fully examine the research questions put forth in this dissertation. The following supplemental analyses fall into two main categories. The first examines an additional outcome: the change in severity of a subsequent misconduct relative to the misconduct that preceded the first disciplinary custody stay. This analysis represents another examination of the potential for disciplinary custody to play a role in *restrictive* deterrence rather than *absolute*

deterrence. The second set of supplemental analyses provides two additional examinations of the early release treatment. First, I examine the same early release analyses described above but using the full original sample ($n=4,425$) rather than the early release sample ($n=3,275$). These analyses thus include all individuals serving more than 100% of their original sanction length. This group includes those who were originally eligible for early release but ended up serving longer than their original completion date, thus providing estimates for all individuals eligible for early release. Then, I conducted an additional examination of early release by modifying the definition of what qualifies as an “early release.” In the original analyses reported above, early release was defined as serving anything less than 100% of the original sanction length assigned by the hearing examiner, but another way of operationalizing this treatment is relative to institutional norms. Below, I examine whether early release as defined as the proportion of median time served by the institution rather than as a proportion of the original sanction assigned alters the findings reported above. This operationalization of early release places more focus on the institutional practices and norms that could impact how much time an individual expected to serve apart from the original sanction length assigned by a hearing examiner.

Restricted Deterrence

The first set of supplemental analyses provides a further test of restrictive deterrence by examining the change in severity between the misconduct resulting in the first disciplinary custody stay and the first subsequent misconduct. One way that a deterrent effect could manifest is through a decrease in the severity of offending after a particular punishment (Gibbs, 1968; Paternoster, 1989). This possibility has contributed

to many efforts to operationalize offense severity (Burton, Finn, Livingston, Scully, Bales, & Padgett, 2004). In the current study, this relationship would take shape as a less serious subsequent misconduct than the one the individual was sanctioned for originally. PADOc assigns a classification code to each misconduct type with Class A designating the most serious offenses such as assault, Class B designating less serious offenses such as failure to stand count, and Class C designating offenses such as taking unauthorized food from the dining room. These are the classification codes that are used internally to assess the severity of an infraction and can influence the types of sanctions eligible after a guilty verdict. For the current study, a change in the severity of the offense is measured by a dummy variable which is assigned a 1 if the subsequent misconduct was a lower classification than the original misconduct offense (i.e., an originating offense of Class A and a subsequent offense of Class B, an originating offense of Class A and a subsequent offense of Class C, or an originating offense of Class B and a subsequent offense of Class B) and a 0 identifies those who committed a subsequent misconduct of the same or higher severity (*Decreased Severity*).

One issue that arises with this outcome is the need to have charging and classification information on both the misconduct resulting in the first disciplinary custody stay and the first subsequent misconduct. However, data issues within PADOc create a problem for a select group of recidivists, those whose first subsequent misconduct infraction following release from the first disciplinary custody stay results in a stay in administrative custody, most likely for protective reasons. In Pennsylvania's data, any misconduct that results in an administrative custody stay does not have the related charging information, so it is not possible to discern the misconduct offense or the

severity of that subsequent offense. Thus, any individual who received a stay in administrative custody as a result of their first subsequent misconduct is not included in this set of supplemental analyses. As described in the main set of analyses examining days to first subsequent misconduct, 3,299 individuals of the full sample (n=4,425) committed at least one misconduct after release from disciplinary custody. However, of this group, 507 received an administrative custody stay as a result of that misconduct event. Thus, there is no data on the misconduct offenses of this group, and they are excluded from these analyses, resulting in a sample of n=2,792.

Length of Stay

Within the change in severity sample (n=2,792), 917 (32.84%) had a subsequent misconduct that was of lesser severity than the misconduct that led to their first disciplinary custody stay. Although the sample differs, the same quartile cutoffs were used between the main analyses and the supplemental analyses for ease of comparison to the main results (for both length of stay and multivalued early release analyses).²⁹ The breakdown of the sample by length of stay quartiles and the raw standardized differences and variance ratios both before and after running the weighted model can be seen in Table 15. This table shows the covariate balance summary for each quartile relative to the first length of stay quartile. A covariate is identified as balanced when the standardized difference is close to zero and the variance ratio is close to 1. Prior to weighting, there are not as many unbalanced covariates as there are in some of the main models. However, a

²⁹ Additional analyses were undertaken to examine if changing the length of stay and multivalued early release cutoffs would change the results for the change of severity analyses. Although the quartiles for length of stay did shift with the smaller sample size, the results were substantively similar to those using the original length of stay quartile cutoffs. Additionally, although the sample is smaller for the non-binary early release analyses, the quartile cutoffs remained the same between the full sample and the change in severity specific sample. Thus, the results described here reflect the same quartiles as those in the main set of analyses.

few of the institutional dummy indicators, namely *Frackville* and *Waymart*, and *Current Drug Misconduct* have larger standardized differences.

Table 15. Raw and Weighted Covariate Balance Summary for Length of Stay and Change in Severity Analyses (n=2,792)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
2nd Quartile				
Age at First DC Stay	-0.0919	-0.0175	0.8412	0.9803
Black	0.0255	0.0389	0.9903	0.9847
Hispanic	0.0236	0.0283	1.0648	1.0780
Other Race	0.0263	0.0242	1.5627	1.5212
Married	-0.0524	-0.0495	0.8437	0.8537
Divorced, Separated, Widowed	-0.0879	-0.0207	0.6715	0.9147
Unknown Marital Status	0.0028	-0.0435	1.0432	0.6152
Custody Level Three	-0.0851	0.0116	0.9702	1.0043
Custody Level Four	0.0505	0.0042	1.0614	1.0053
Custody Level Five	-0.0141	-0.1038	0.9473	0.7004
Unknown Custody Level	0.0901	0.0392	1.1167	1.0490
Incarcerated Days	0.0741	0.0016	1.0717	0.9122
Property Incarceration Offense	-0.0494	-0.0248	0.8913	0.9438
Drug Incarceration Offense	0.0241	-0.0466	1.0602	0.8949
Other Incarceration Offense	0.0272	0.0009	1.0466	1.0015
Missing Incarceration Offense	0.0508	0.0516	1.1310	1.1309
Highest Prior MH C	0.0141	0.0147	1.0215	1.0241
Highest Prior MH D	-0.0182	-0.0321	0.9459	0.8925
Total Prior Violent Program	-0.0175	-0.0121	0.8657	0.8757
Total Prior Alcohol/Drug Program	-0.0941	-0.0126	0.5513	0.8757
Total Prior Other Program	0.0104	0.0012	1.1775	0.9807
Current Violent Program	0.0693	0.0477	1.4219	1.2898
Current Alcohol/Drug Program	0.0612	0.0354	1.3592	1.2018
Current Other Program	0.0164	0.0663	1.1279	1.6090
Worked at Time of First DC Stay	0.1035	0.0679	0.8858	0.9272
Current Property Misconduct	-0.0304	0.0038	0.9181	1.0100
Current Drug Misconduct	0.1477	-0.0173	3.0650	0.9171
Current Prison Rule Misconduct	-0.1850	-0.0048	1.0249	0.9984
Current Other Misconduct	-0.0148	-0.0001	0.9705	0.9997

Prior Misconduct Count	-0.0262	0.0384	0.8267	0.9704
Albion	0.0427	0.0072	1.2953	1.0523
Benner Township	0.0853	0.0206	1.4058	1.0946
Cambridge Springs	-0.0226	-0.0167	0.8622	0.8829
Chester	0.0594	0.0371	1.4206	1.2298
Coal Township	0.0413	0.0499	1.2663	1.3299
Dallas	0.1263	-0.0593	1.8742	0.7816
Fayette	-0.0316	-0.0278	0.8705	0.8816
Forest	-0.1569	0.0057	0.5317	1.0259
Frackville	0.1875	0.0425	6.4537	1.3775
Graterford	-0.1254	-0.0466	0.6400	0.8762
Greene	-0.0244	-0.0241	0.7845	0.8316
Houtzdale	-0.1469	0.0276	0.5142	1.1425
Huntingdon	0.0379	0.0341	1.2012	1.2000
Laurel Highlands	-0.0455	0.0092	0.7646	1.0677
Mahanoy	-0.0507	0.0291	0.7194	1.2116
Mercer	-0.1490	0.0121	0.1630	1.1070
Muncy	0.1118	-0.0159	1.6550	0.9358
Pine Grove	0.0919	-0.0046	1.5141	0.9812
Pittsburgh	-0.0833	0.0384	0.6275	1.2261
Retreat	0.0045	-0.0166	1.0429	0.8579
Rockview	-0.1175	-0.0034	0.6161	0.9838
Smithfield	0.1013	0.0276	1.9614	1.2433
Somerset	0.0589	0.0006	1.1959	1.0023
Waymart	0.2229	0.0072	4.5939	1.0410
3rd Quartile				
Age at First DC Stay	-0.1100	-0.0312	0.8019	1.0105
Black	-0.0322	0.0179	1.0107	0.9933
Hispanic	0.0399	0.0267	1.1102	1.0737
Other Race	0.0048	0.0383	1.0938	1.8807
Married	-0.0514	-0.0338	0.8468	0.8992
Divorced, Separated, Widowed	-0.0480	-0.0308	0.8149	0.8744
Unknown Marital Status	0.0448	-0.0590	1.8172	0.4968
Custody Level Three	-0.0279	-0.0210	0.9919	0.9915
Custody Level Four	-0.0746	0.0433	0.9040	1.0543
Custody Level Five	0.0351	-0.0910	1.1358	0.7356
Unknown Custody Level	0.0737	0.0412	1.0964	1.0513
Incarcerated Days	-0.1366	-0.0078	1.0873	0.9798
Property Incarceration Offense	0.0134	-0.0134	1.0299	0.9695
Drug Incarceration Offense	0.0554	-0.0525	1.1390	0.8816
Other Incarceration Offense	-0.0209	-0.0159	0.9642	0.9734
Missing Incarceration Offense	0.0777	0.0426	1.2013	1.1077

Highest Prior MH C	-0.0697	-0.0219	0.8919	0.9639
Highest Prior MH D	-0.1588	-0.0180	0.5610	0.9393
Total Prior Violent Program	-0.0217	-0.0051	0.9009	0.9701
Total Prior Alcohol/Drug Program	-0.0620	0.0027	0.7603	1.1297
Total Prior Other Program	0.0133	0.0018	1.2937	1.0229
Current Violent Program	-0.0400	0.0256	0.7866	1.1514
Current Alcohol/Drug Program	-0.0099	0.0088	0.9465	1.0485
Current Other Program	-0.0519	0.0295	0.6421	1.2532
Worked at Time of First DC Stay	0.0528	0.0335	0.9435	0.9648
Current Property Misconduct	0.0202	0.0235	1.0557	1.0619
Current Drug Misconduct	0.1996	-0.0042	4.0870	0.9794
Current Prison Rule Misconduct	-0.4283	-0.0317	0.9560	0.9884
Current Other Misconduct	-0.0168	-0.0257	0.9665	0.9460
Prior Misconduct Count	-0.2061	-0.0006	0.6659	1.0085
Albion	-0.0911	0.0050	0.4843	1.0365
Benner Township	-0.1010	0.0318	0.5925	1.1475
Cambridge Springs	0.0237	-0.0084	1.1540	0.9399
Chester	0.0680	0.0385	1.4872	1.2391
Coal Township	0.0415	0.0526	1.2674	1.3492
Dallas	0.0327	-0.0500	1.2024	0.8147
Fayette	-0.0366	-0.0337	0.8507	0.8572
Forest	-0.2384	-0.0024	0.3266	0.9892
Frackville	0.1617	0.0367	5.3684	1.3223
Graterford	0.0483	-0.0671	1.1508	0.8230
Greene	0.1000	0.0060	2.1590	1.0444
Houtzdale	-0.1733	0.0081	0.4386	1.0412
Huntingdon	-0.0713	0.0403	0.6645	1.2386
Laurel Highlands	-0.1147	-0.0014	0.4554	0.9902
Mahanoy	0.0151	0.0191	1.0913	1.1365
Mercer	0.0530	0.0320	1.4194	1.2955
Muncy	0.1880	0.0019	2.1791	1.0078
Pine Grove	-0.0244	0.0288	0.8792	1.1216
Pittsburgh	0.0181	0.0307	1.0901	1.1794
Retreat	0.0097	-0.0222	1.0929	0.8119
Rockview	-0.1808	-0.0144	0.4360	0.9323
Smithfield	-0.0134	0.0258	0.8966	1.2273
Somerset	-0.2434	-0.0072	0.3210	0.9743
Waymart	0.0831	0.0084	2.0113	1.0483
4th Quartile				
Age at First DC Stay	-0.2051	0.0642	0.7632	1.0429
Black	0.0436	0.0024	0.9825	0.9990
Hispanic	0.0340	-0.0089	1.0935	0.9757

Other Race	0.0602	0.0098	2.5034	1.1965
Married	-0.0567	-0.0250	0.8313	0.9252
Divorced, Separated, Widowed	-0.0871	-0.0482	0.6740	0.8060
Unknown Marital Status	0.0533	-0.0573	2.0027	0.5090
Custody Level Three	-0.1114	0.0968	0.9582	1.0276
Custody Level Four	0.0323	-0.0162	1.0396	0.9790
Custody Level Five	0.0131	-0.0998	1.0500	0.7113
Unknown Custody Level	0.1100	-0.0288	1.1408	0.9628
Incarcerated Days	-0.0205	0.0259	0.9952	0.9038
Property Incarceration Offense	-0.0188	-0.0837	0.9583	0.8123
Drug Incarceration Offense	-0.0114	0.1770	0.9717	1.4031
Other Incarceration Offense	0.0026	-0.0722	1.0044	0.8787
Missing Incarceration Offense	0.0414	0.0281	1.1066	1.0708
Highest Prior MH C	-0.0291	-0.0184	0.9553	0.9696
Highest Prior MH D	-0.2632	0.2510	0.3160	1.9479
Total Prior Violent Program	0.0050	0.1028	0.9428	1.0299
Total Prior Alcohol/Drug Program	-0.0802	-0.0406	0.5978	0.7385
Total Prior Other Program	0.0921	-0.0087	1.1598	0.7093
Current Violent Program	0.0687	-0.0147	1.4178	0.9174
Current Alcohol/Drug Program	0.0393	0.0221	1.2250	1.1236
Current Other Program	0.0591	0.0459	1.4954	1.4058
Worked at Time of First DC Stay	0.0314	0.0796	0.9668	0.9139
Current Property Misconduct	0.0946	-0.0381	1.2641	0.9012
Current Drug Misconduct	0.3011	-0.0339	6.4974	0.8403
Current Prison Rule Misconduct	-0.7946	0.1254	0.6977	1.0263
Current Other Misconduct	-0.1012	-0.0361	0.7990	0.9241
Prior Misconduct Count	-0.0623	-0.0125	0.8670	0.9663
Albion	-0.0618	0.0871	0.6338	1.7239
Benner Township	0.0642	-0.0091	1.3002	0.9593
Cambridge Springs	-0.1828	0.2547	0.1208	3.6607
Chester	0.1314	0.0175	2.0235	1.1053
Coal Township	0.0505	0.0390	1.3291	1.2541
Dallas	0.1601	-0.0887	2.1494	0.6807
Fayette	-0.1332	-0.0507	0.5012	0.7883
Forest	-0.1226	-0.0177	0.6260	0.9209
Frackville	0.1508	0.0155	4.9434	1.1303
Graterford	0.0167	-0.0861	1.0514	0.7744
Greene	0.1294	-0.0210	2.5912	0.8524
Houtzdale	-0.0800	0.0060	0.7219	1.0300
Huntingdon	-0.0306	0.0588	0.8494	1.3553
Laurel Highlands	-0.1479	0.0034	0.3268	1.0244
Mahanoy	-0.0557	-0.0071	0.6938	0.9509
Mercer	0.0209	0.0155	1.1569	1.1385

Muncy	0.0087	0.0746	1.0458	1.3215
Pine Grove	0.2770	-0.0115	2.7985	0.9527
Pittsburgh	-0.0063	-0.0261	0.9695	0.8576
Retreat	-0.0277	0.0431	0.7563	1.4183
Rockview	-0.2368	-0.0753	0.2921	0.6667
Smithfield	-0.0359	0.0050	0.7344	1.0421
Somerset	-0.0536	-0.0514	0.8313	0.8215
Waymart	0.1843	-0.0189	3.7694	0.8950

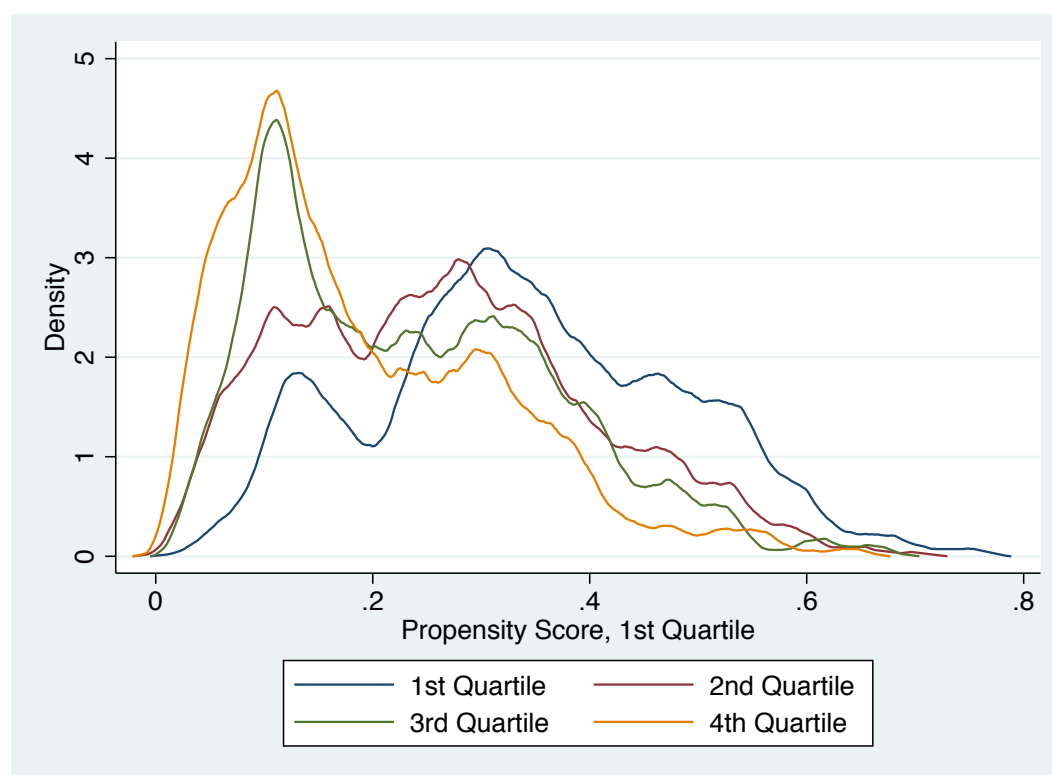
To address the issues of selection bias inherent in the current study, I utilized IPWRA. For the first model, length of stay, the treatment was broken down into the quartiles of the length of stay distribution, and the outcome was a binary indicator of whether the individual committed a misconduct of lesser severity post-release from disciplinary custody. This model was estimated and the results of the six pairwise ATEs are shown in Table 16.

Prior to a discussion of the results, it is important to first examine and check the assumptions made by these models. First, to trust the results of IPWRA, one must examine the balance of the covariates after weighting. Table 15 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates. However, there are a few notable exceptions, namely *Unknown Marital Status* and *Highest Prior MHD*. Some covariate imbalance is to be expected, particularly due to the large number of covariates used in the current analyses.

One concern with IPWRA is the possibility for extreme weights that can result in large and unstable estimates (Lopez & Gutman, 2017). The `teffects` code in Stata has an automatic overlap violator error that identifies all individuals with a small probability (it flags those with propensity scores less than 1.00e-.05) of receiving the treatment. No such

small propensity scores were identified in the change in severity analyses. Another check to the overlap assumption is the examination of overlap plots. Figure 7 displays the estimated densities of the probability of receiving each treatment level for length of stay in the change in severity sample. As described above, one of the assumptions inherent to IPWRA is the requirement of a non-zero probability of receiving each level of the treatment. As can be seen in Figure 7, are observations in all four treatment categories at each combination of covariate values.

Figure 7. Length of Stay and Change in Severity Overlap Graph (n=2,792)



After these checks have satisfied the assumptions of inverse probability weighting, the estimated pairwise ATEs can be examined and discussed. As shown in Table 16, there are some statistically significant mean differences across the length of

stay quartiles. The likelihood of committing a follow-up misconduct of a lesser severity after release from the first disciplinary custody stay is .08 less for those in the fourth quartile of length of stay relative to those in the first. Another result of statistical significance comes from the pairwise ATE of the 2nd and 4th quartiles. The likelihood of a less severe follow-up misconduct is about .05 lower for those in the fourth quartile of LOS relative to those in the second quartile. Lastly, there is a marginally significant mean difference between the third and the first quartiles. The likelihood of a follow-up misconduct is .04 lower for those in the third quartile of length of stay relative to those in the first quartile. The remaining pairwise ATEs do not show statistically significant mean differences.

Table 16. Length of Stay and Change in Severity Pairwise ATEs (n=2,792)

	ATE	Robust SE	Z	P> z	95% CI	
2 vs. 1	-.0326	.0243	-1.34	0.180	-.0801	.0150
3 vs. 1	-.0444	.0247	-1.80	0.072	-.0928	.0040
4 vs. 1	-.0786	.0227	-3.46	0.001	-.1231	-.0341
POmean 1	.3582	.0188	19.04	0.000	.3214	.3951
3 vs. 2	-.0119	.0231	-0.51	0.608	-.0572	.0335
4 vs. 2	-.0461	.0210	-2.19	0.029	-.0873	-.0048
POmean 2	.3257	.0167	19.50	0.000	.2929	.3584
4 vs. 3	-.0342	.0214	-1.59	0.111	-.0762	.0078
POmean 3	.3138	.0172	18.29	0.000	.2802	.3474
POmean 4	.2796	.0142	19.65	0.000	.2517	.3075

These results do not support the tenets of restrictive deterrence, though they substantively align with the main results. Individuals who have the longest stays in disciplinary custody (more than 56 days) actually have lower likelihoods of committing less severe follow-up misconducts. In other words, those serving the longest disciplinary custody stays are more likely to commit subsequent misconducts that are as severe or

more severe than the originating misconduct that led to the disciplinary custody stay. This relationship is statistically significant relative to both the 1st and 2nd quartiles and is marginally significant for the 3rd quartile.

Binary Early Release

The sample for the examination of the binary early release treatment on the change in severity outcome contains 2,045 individuals. This sample is decreased from that of the length of stay sample above due to the restrictions placed on the early release sample, either due to the offense not being eligible for early release or due to the individual serving more than 100% of their original sanction length. Of the 3,275 individuals in the early release sample, 835 do not have any follow-up misconduct; in other words, these individuals did not commit an additional misconduct within the remainder of their stay in PADOH or before the end of the study period (August 2017). An additional 395 committed a follow-up misconduct that resulted in an administrative custody sanction and thus do not have any charging information. Of those 2,045 individuals eligible for early release with a follow-up misconduct with charging information, 623 (30.5% of the sample) were released prior to serving 100% of their original sanction length.

Prior to estimating the model, I examined the balance for all covariates across the treatment and comparison groups and these results are shown in Table 17. Because these analyses only have one treatment, early release, the comparison is between those who were released prior to serving their full sanction length and those serving exactly 100% of their original sanction length. This table shows that there are covariate differences that exist between those who are released early and those who serve the full 100% of their

original sanction length. The main differences come in the institutional variables, namely *Fayette* and *Somerset*.

I ran a model examining the relationship between binary early release and the change in severity outcome, and these results are displayed in Table 18. Prior to a discussion of these results, it is first important to examine the assumptions made by the model. First, for binary treatments, there is an overidentification test for covariate balance within Stata. This test performs a chi-squared test to examine the null hypothesis that the covariates are balanced, and the results for this test are shown in Table 19. This test failed to reject the null hypothesis, indicating that the weighted model achieved balance across the covariates. In addition to this check, balance can be assessed by looking at the weighted columns of Table 17, which shows that the covariates are more balanced after the weighted model was run compared to the raw comparisons. Lastly, the overlap between the comparison and treatment groups can be examined by looking at Figure 8. Although densities vary, there is overlap across the distribution.

Table 17. Raw and Weighted Covariate Balance Summary for Binary Early Release and Change in Severity Analyses (n=2,045)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
Age at First DC Stay	-0.0367	-0.0180	0.8996	0.8413
Black	-0.0306	-0.0004	1.0136	1.0002
Hispanic	0.1148	-0.1056	1.3810	0.7756
Other Race	-0.1135	-0.0314	0.0900	0.5252
Married	0.0562	-0.0173	1.2082	0.9478
Divorced, Separated, Widowed	-0.0699	0.0331	0.7198	1.1770
Unknown Marital Status	-0.0123	0.0169	0.8586	1.2649
Custody Level Three	0.0855	-0.0680	1.0300	0.9799

Custody Level Four	0.0132	0.1083	1.0160	1.1632
Custody Level Five	-0.0828	0.0395	0.7336	1.1496
Unknown Custody Level	-0.0541	0.0016	0.9396	1.0020
Incarcerated Days	-0.2998	-0.0020	0.8476	1.0324
Property Incarceration Offense	0.0367	0.0511	1.0920	1.1395
Drug Incarceration Offense	-0.0579	0.0404	0.8741	1.1042
Other Incarceration Offense	-0.0007	-0.0358	0.9983	0.9442
Missing Incarceration Offense	0.0459	0.0296	1.1153	1.0741
Highest Prior MH C	-0.1083	0.0298	0.8343	1.0553
Highest Prior MH D	0.0797	-0.0782	1.3978	0.7501
Total Prior Violent Program	-0.0338	0.0570	1.0805	1.4148
Total Prior Alcohol/Drug Program	-0.0669	0.0434	0.9048	1.2315
Total Prior Other Program	-0.0090	0.0419	0.8871	1.1729
Current Violent Program	-0.0797	-0.0027	0.6567	0.9850
Current Alcohol/Drug Program	-0.1150	-0.0087	0.5641	0.9549
Current Other Program	0.0551	0.0126	1.5378	1.1005
Worked at Time of First DC Stay	0.1520	0.0303	0.8491	0.9648
Current Property Misconduct	0.0785	-0.0536	1.2257	0.8799
Current Drug Misconduct	-0.0682	-0.0421	0.7303	0.8155
Current Prison Rule Misconduct	0.1030	0.0493	1.0203	1.0084
Current Other Misconduct	-0.0291	-0.0100	0.9417	0.9805
Prior Misconduct Count	-0.1230	-0.0045	0.8745	1.0758
Albion	-0.1387	0.0128	0.3879	1.0885
Benner Township	-0.4176	0.0145	0.1926	1.0549
Cambridge Springs	-0.0107	0.0021	0.9125	1.0174
Chester	-0.0429	0.0069	0.7877	1.0404
Coal Township	0.2182	0.0101	5.4948	1.0601
Dallas	0.0510	0.0080	1.2649	1.0369
Fayette	-0.4786	-0.0326	0.0441	0.8557
Forest	0.2903	0.0253	5.6283	1.1175
Frackville	-0.0404	0.0093	0.7613	1.0667
Graterford	-0.1034	0.0063	0.7325	1.0198
Greene	0.0085	0.0110	1.0711	1.0947
Houtzdale	0.3056	-0.0761	7.3268	0.7397
Huntingdon	0.0837	-0.0002	1.5844	0.9987
Laurel Highlands	0.0352	0.0083	1.2570	1.0548
Mahanoy	0.1812	0.0533	8.1387	1.5896
Mercer	-0.0154	0.0051	0.8050	1.0785
Muncy	-0.0759	0.0021	0.6928	1.0104
Pine Grove	0.0391	0.0184	1.1596	1.0729
Pittsburgh	0.0394	0.0145	1.2787	1.0936
Retreat	0.1054	-0.0432	3.3609	0.6905
Rockview	0.1104	0.0176	1.6971	1.0840

Smithfield	-0.1326	0.0018	0.4104	1.0122
Somerset	-0.4389	0.0049	0.2235	1.0164
Waymart	-0.1570	0.0056	0.3855	1.0348

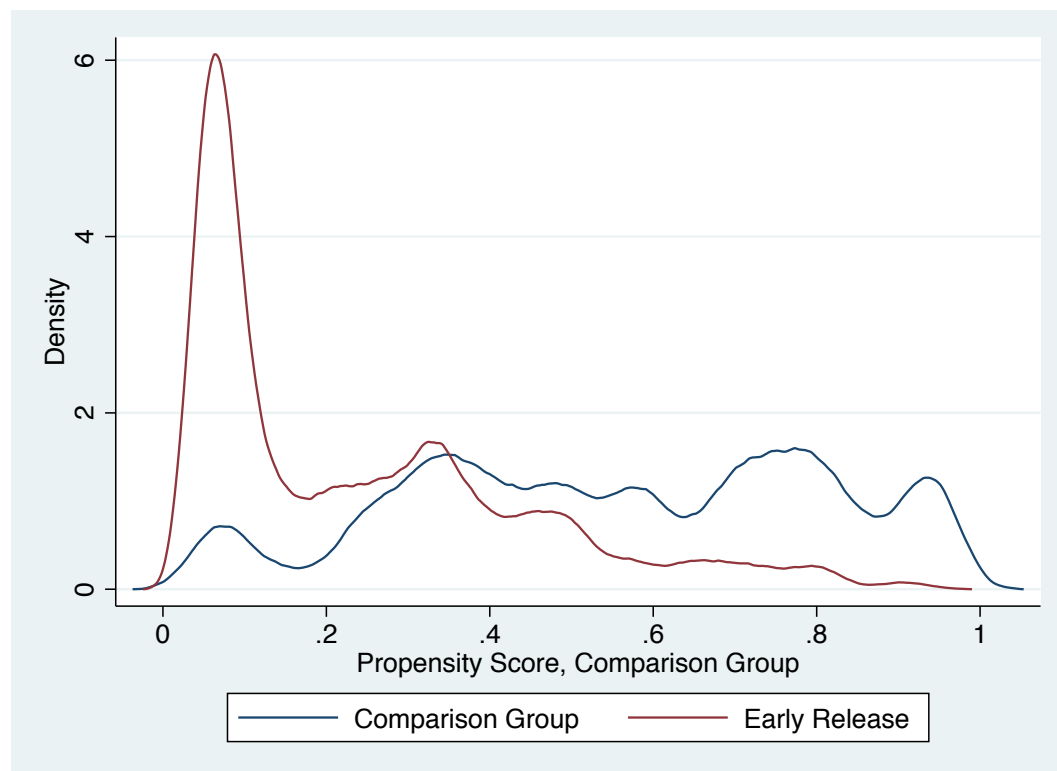
Table 18. Binary Early Release and Change in Severity Pairwise ATE (n=2,045)

	ATE	Robust SE	z	P> z	95%CI	
Early Release (1 vs 0)	.0322	.0219	1.47	0.141	-.0107	.0751
POmean	.2705	.0189	14.34	0.000	.2335	.3074

Table 19. Binary Early Release and Change in Severity Overidentification Test (n=2,045)

chi2(54)	44.3562
Prob > chi2	0.8469

Figure 8. Binary Early Release and Change in Severity Overlap Graph (n=2,045)



As can be seen in Table 18, there is no statistically significant relationship between being released prior to serving 100% of the original sanction length relative to serving the full sanction length (exactly 100%) in the likelihood of a committing a less severe misconduct after release from disciplinary custody. Like the prior analysis, this result does not support the tenets of restrictive deterrence.

Multivalued Early Release

Although several attempts were made to examine the differences among the multivalued early release quartiles and the change of severity outcome, the model did not achieve convergence. These issues are potentially due to the sparseness of the data in the most restricted sample of the early release and change in severity model. As the model will not converge, it is not possible to fully examine the change in severity analyses using all three treatment indicators.

Additional Operationalizations of Early Release

The next set of supplemental analyses provide further examination of the early release treatment. First, I examine early release using the full sample ($n=4,425$), only excluding those individuals whose misconduct offenses made them ineligible for early release (e.g. aggravated assault, rape, etc.). As all those in this sample had the opportunity to be released early from disciplinary custody, this analysis provides an additional test of how the proportion of time served can impact later institutional behavior. The first set of supplemental analyses further examining early release includes those who served more than 100% of their original sanction. From the full original sample of 4,425, these analyses only exclude the 411 individuals who were ineligible for early release based on

their misconduct offense type, bringing the sample size for these analyses to 4,014. Of this group, 2,132 (53.1%) were released prior to serving their full sanction length. The zero category in the binary early release treatment now includes both those who served exactly 100% of their original sanction and those who served more than 100%.

Full Sample Early Release- Binary Treatment

The comparison group is larger for these analyses relative to the restricted early release sample, due to the inclusion of those serving more than 100% of their original sanction to this group. As shown in Tables 21 and 23 below there are no statistically significant differences in the results for either of the binary early release analyses (with either outcome of binary misconduct or days to misconduct). Assumption checks were run, and as can be seen in Table 20, covariate balance improved after weighting and the chi-squared test failed to reject the null hypothesis (Table 22) that the covariates were balanced. Figure 9 shows overlap between the distributions. Just like with the binary early release analyses discussed in the main results above, there were no significant mean differences for the binary early release treatment in either likelihood of subsequent misconduct or in days to subsequent misconduct.

Table 20. Raw and Weighted Covariate Balance Summary for Full Sample Binary Early Release and Binary Misconduct Analyses (n=4,014)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
Age at First DC Stay	-0.0889	-0.0031	0.8489	0.9294
Black	0.0261	-0.0032	0.9936	1.0007
Hispanic	0.0155	0.0063	1.0395	1.0156
Other Race	-0.0162	-0.0091	0.8158	0.8875

Married	0.0291	-0.0127	1.0893	0.9657
Divorced, Separated, Widowed	-0.0280	-0.0046	0.8882	0.9803
Unknown Marital Status	0.0257	-0.0075	1.3612	0.9175
Custody Level Three	0.0513	-0.0213	1.0182	0.9926
Custody Level Four	-0.0019	0.0181	0.9974	1.0238
Custody Level Five	-0.0631	0.0264	0.7985	1.0949
Unknown Custody Level	-0.0131	-0.0022	0.9842	0.9973
Incarcerated Days	-0.2016	-0.0132	0.8859	0.9474
Property Incarceration Offense	0.0256	-0.0139	1.0611	0.9679
Drug Incarceration Offense	-0.0014	0.0010	0.9968	1.0023
Other Incarceration Offense	0.0049	0.0009	1.0079	1.0015
Missing Incarceration Offense	0.0152	0.0046	1.0370	1.0109
Highest Prior MH C	-0.1300	-0.0016	0.8057	0.9972
Highest Prior MH D	0.0578	-0.0088	1.2429	0.9683
Total Prior Violent Program	-0.0231	0.0080	1.0372	1.0384
Total Prior Alcohol/Drug Program	-0.0244	-0.0130	1.0051	0.9705
Total Prior Other Program	-0.0109	0.0087	0.8273	0.8739
Current Violent Program	-0.0595	0.0060	0.7266	1.0325
Current Alcohol/Drug Program	-0.0701	-0.0148	0.6943	0.9250
Current Other Program	0.0224	0.0084	1.1836	1.0662
Worked at Time of First DC Stay	0.1191	0.0327	0.8808	0.9649
Current Property Misconduct	-0.0064	-0.0159	0.9848	0.9643
Current Drug Misconduct	-0.0534	-0.0315	0.7796	0.8573
Current Prison Rule Misconduct	0.0458	0.0146	1.0093	1.0028
Current Other Misconduct	-0.0208	0.0202	0.9593	1.0407
Prior Misconduct Count	-0.1124	0.0073	0.9139	1.1410
Albion	-0.0805	0.0035	0.5780	1.0235
Benner Township	-0.2333	-0.0005	0.3475	0.9981
Cambridge Springs	-0.0555	0.0054	0.6487	1.0421
Chester	-0.1156	0.0058	0.5498	1.0296
Coal Township	0.1070	-0.0064	1.7916	0.9678
Dallas	0.0049	0.0012	1.0245	1.0057
Fayette	-0.3977	-0.0109	0.0489	0.9498
Forest	0.2304	-0.0005	3.6377	0.9975
Frackville	0.0374	-0.0036	1.3030	0.9757
Graterford	-0.0548	0.0005	0.8412	1.0015
Greene	-0.0494	-0.0007	0.7189	0.9952
Houtzdale	0.2764	0.0033	4.1051	1.0140
Huntingdon	0.0550	0.0008	1.3354	1.0041
Laurel Highlands	0.0045	0.0000	1.0291	1.0002
Mahanoy	0.0322	0.0008	1.2345	1.0050
Mercer	-0.2008	0.0012	0.2245	1.0080
Muncy	-0.1337	0.0008	0.5417	1.0034

Pine Grove	0.0564	0.0032	1.2646	1.0134
Pittsburgh	-0.0904	0.0013	0.6373	1.0064
Retreat	0.0784	-0.0061	2.0230	0.9504
Rockview	0.1275	0.0007	1.9008	1.0031
Smithfield	-0.0726	-0.0014	0.5876	0.9900
Somerset	-0.2802	0.0034	0.3101	1.0131
Waymart	-0.1155	-0.0018	0.5164	0.9897

Table 21. Full Sample Binary Early Release and Binary Misconduct Pairwise ATE

(n=4,014)

	ATE	Robust SE	z	P> z	95%CI	
Early Release (1 vs 0)	-.0193	.0168	-1.15	0.250	-.0523	.0136
POMean	.6266	.0120	52.37	0.000	.6031	.6500

Table 22. Full Sample Binary Early Release and Binary Misconduct Overidentification

Test (n=4,014)

chi2(54)	39.9609
Prob > chi2	0.9364

Figure 9. Full Early Release Sample—Binary Early Release Overlap Graph (n=4,014)

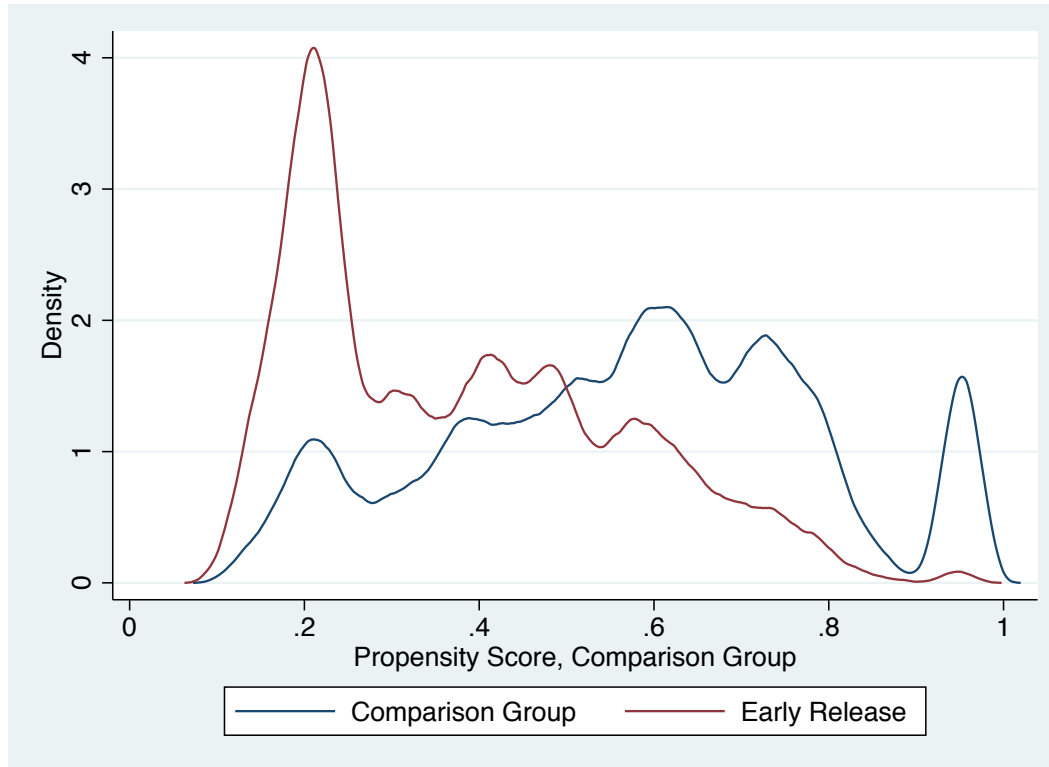


Table 23. Full Sample Binary Early Release and Days to Misconduct Pairwise ATE
(n=4,014)

	ATE	Robust SE	z	P> z	95%CI	
Early Release (1 vs 0)	-19.7224	76.7716	-0.26	0.797	-170.1925	130.7466
POmean	714.1876	55.7265	12.82	0.000	604.9656	823.4096

Table 24. Full Sample Binary Early Release and Days to Misconduct Overidentification Test (n=4,014)

chi2(54)	39.9609
Prob > chi2	0.9364

Full Sample Early Release- Multivalued Treatment

Although there were no significant differences for the binary early release models, the addition of those serving more than 100% of their sanction to the early release analyses impacted the multivalued early release and binary misconduct models. With the addition of those serving more than 100% of their sanction, the early release quartiles shifted substantially: 0%-60% (n=1,031); 60.01%-96.67% (n=1,029); 96.68%-100% (n=1,234); 100.01%+ (n=720). Those who serve more than 100% are now fully encompassed in the fourth quartile.

The first outcome of interest is a binary indicator of subsequent misconduct within one year of release from the first disciplinary custody stay. Table 25 shows the standardized differences and variance ratios for all covariates in the multivalued early release model both before and after weighting using the inverse probability of receiving each treatment. This table shows the covariate balance summary for each quartile relative to the first early release quartile. Prior to weighting, the covariates that are out of balance are in line with those initially out of balance in the original multivalued early release analyses discussed in the main results. In particular, variables such as *Incarcerated Days*, the mental health variables, and a few of the institution dummy variables are out of balance prior to weighting. Together, the differences across the different dosage levels indicate the potential for selection bias and the importance of accounting for such differences prior to making a conclusion of the deterrent capabilities of differential early release.

Table 25. Raw and Weighted Covariate Balance Summary for Full Sample Multivalued Early Release and Binary Misconduct Analyses (n=4,014)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
2nd Quartile				
Age at First DC Stay	0.0260	-0.0691	0.8963	0.7776
Black	-0.0291	-0.0006	1.0079	1.0001
Hispanic	0.0470	0.0009	1.1236	1.0022
Other Race	-0.0764	-0.0243	0.3359	0.6844
Married	0.0312	-0.0457	1.0937	0.8802
Divorced, Separated, Widowed	0.0331	0.0230	1.1567	1.1080
Unknown Marital Status	-0.0106	-0.0177	0.8915	0.8181
Custody Level Three	0.0252	0.0091	1.0075	1.0032
Custody Level Four	-0.0177	-0.0101	0.9766	0.9870
Custody Level Five	0.0005	0.0362	1.0018	1.1324
Unknown Custody Level	-0.0125	-0.0136	0.9849	0.9832
Incarcerated Days	0.2798	-0.0420	1.1578	0.8373
Property Incarceration Offense	0.0095	-0.0004	1.0215	0.9991
Drug Incarceration Offense	0.0182	0.0071	1.0416	1.0162
Other Incarceration Offense	0.0084	-0.0107	1.0137	0.9832
Missing Incarceration Offense	0.0007	-0.0238	1.0017	0.9467
Highest Prior MH C	0.1513	-0.0173	1.3375	0.9701
Highest Prior MH D	-0.0653	-0.0387	0.7975	0.8697
Total Prior Violent Program	0.1051	-0.0489	1.1190	0.7767
Total Prior Alcohol/Drug Program	0.0552	-0.0189	0.9228	0.9178
Total Prior Other Program	0.0093	-0.0552	0.8421	0.7422
Current Violent Program	-0.0177	0.0340	0.9009	1.2015
Current Alcohol/Drug Program	0.1149	-0.0080	2.0208	0.9583
Current Other Program	0.1338	-0.0108	2.9527	0.9242
Worked at Time of First DC Stay	-0.0971	0.0906	1.1280	0.9075
Current Property Misconduct	0.0131	0.0176	1.0333	1.0417
Current Drug Misconduct	0.1374	0.0340	2.1402	1.1971
Current Prison Rule Misconduct	-0.1052	-0.0565	0.9880	0.9919
Current Other Misconduct	-0.0574	-0.0113	0.8894	0.9781
Prior Misconduct Count	0.0467	0.0031	0.9043	0.8765
Albion	0.0473	-0.0122	1.4557	0.9244
Benner Township	0.0066	-0.0109	1.0409	0.9552
Cambridge Springs	0.0771	-0.0259	1.9862	0.8242
Chester	0.0848	-0.0151	1.7454	0.9275
Coal Township	-0.0044	-0.0033	0.9801	0.9834

Dallas	0.1048	-0.0183	1.7092	0.9153
Fayette	0.0501	0.0523	2.4975	1.3212
Forest	-0.0863	-0.0013	0.7210	0.9938
Frackville	0.1795	-0.0038	4.1498	0.9746
Graterford	-0.1644	0.0033	0.5732	1.0106
Greene	0.1521	0.0499	4.0972	1.4454
Houtzdale	-0.0955	-0.0053	0.7321	0.9777
Huntingdon	0.2704	0.0116	5.0704	1.0627
Laurel Highlands	0.0704	-0.0346	1.5647	0.8112
Mahanoy	0.1443	0.0130	2.5879	1.0897
Mercer	-0.0320	0.0013	0.7034	1.0084
Muncy	0.0726	-0.0146	1.4834	0.9374
Pine Grove	0.2194	-0.0057	2.5928	0.9771
Pittsburgh	0.0119	0.0064	1.0688	1.0324
Retreat	0.1130	-0.0126	2.4679	0.8999
Rockview	0.0004	0.0094	1.0018	1.0459
Smithfield	-0.0410	-0.0122	0.7107	0.9172
Somerset	0.0425	0.0280	1.2851	1.1142
Waymart	0.1656	-0.0052	3.5873	0.9714
3rd Quartile				
Age at First DC Stay	0.0518	-0.0318	1.0495	0.9645
Black	0.0253	-0.0360	0.9916	1.0078
Hispanic	-0.0136	0.0400	0.9647	1.0979
Other Race	-0.0361	0.0007	0.6517	1.0102
Married	-0.0097	0.0093	0.9711	1.0248
Divorced, Separated, Widowed	0.0333	0.0696	1.1572	1.3399
Unknown Marital Status	-0.0361	-0.0449	0.6517	0.5695
Custody Level Three	-0.0469	0.0028	0.9827	1.0010
Custody Level Four	0.0103	-0.0680	1.0133	0.9104
Custody Level Five	0.0564	-0.0088	1.2208	0.9686
Unknown Custody Level	0.0254	0.0134	1.0298	1.0162
Incarcerated Days	0.3899	-0.0368	1.2304	0.8782
Property Incarceration Offense	-0.0779	-0.0192	0.8256	0.9535
Drug Incarceration Offense	0.0261	0.0244	1.0596	1.0558
Other Incarceration Offense	0.0240	-0.0104	1.0387	0.9836
Missing Incarceration Offense	-0.0229	-0.0181	0.9458	0.9595
Highest Prior MH C	0.1895	0.0126	1.4207	1.0217
Highest Prior MH D	-0.1506	0.0143	0.5558	1.0496
Total Prior Violent Program	0.0715	-0.0453	1.0559	0.8484
Total Prior Alcohol/Drug Program	0.0438	-0.0152	0.9160	0.9286
Total Prior Other Program	0.0131	-0.0272	1.0708	0.9458
Current Violent Program	0.0640	0.0056	1.3947	1.0323

Current Alcohol/Drug Program	0.1642	0.0119	2.5680	1.0638
Current Other Program	0.0735	-0.0245	1.9329	0.8318
Worked at Time of First DC Stay	-0.2481	0.0749	1.2955	0.9243
Current Property Misconduct	0.0336	0.0373	1.0856	1.0888
Current Drug Misconduct	0.1830	0.0730	2.6091	1.4422
Current Prison Rule Misconduct	-0.1648	-0.0380	0.9715	0.9952
Current Other Misconduct	-0.0322	-0.0255	0.9377	0.9507
Prior Misconduct Count	0.0894	-0.0205	0.9084	0.7647
Albion	0.1205	-0.0110	2.3357	0.9319
Benner Township	0.3411	-0.0060	4.0637	0.9751
Cambridge Springs	0.0185	-0.0057	1.2044	0.9602
Chester	0.1287	-0.0210	2.2126	0.8998
Coal Township	-0.1876	-0.0277	0.2939	0.8633
Dallas	0.0277	-0.0191	1.1701	0.9115
Fayette	0.4840	0.0697	50.6423	1.4377
Forest	-0.2962	-0.0252	0.1749	0.8808
Frackville	0.1170	-0.0068	2.7792	0.9551
Graterford	0.0428	-0.0024	1.1206	0.9923
Greene	0.1522	0.0444	4.0991	1.3915
Houtzdale	-0.3250	-0.0042	0.2001	0.9820
Huntingdon	0.1375	0.0078	2.6601	1.0420
Laurel Highlands	0.0705	-0.0307	1.5655	0.8316
Mahanoy	-0.0712	0.0303	0.4808	1.2148
Mercer	-0.0262	0.0097	0.7537	1.0648
Muncy	0.1243	-0.0010	1.8815	0.9955
Pine Grove	0.1456	-0.0132	1.9833	0.9472
Pittsburgh	-0.0456	0.0028	0.7544	1.0141
Retreat	-0.0569	0.0178	0.5037	1.1489
Rockview	-0.1281	-0.0147	0.5273	0.9300
Smithfield	0.0942	-0.0167	1.8401	0.8871
Somerset	0.3995	0.0164	4.8565	1.0665
Waymart	0.2133	-0.0058	4.6639	0.9684
4th Quartile				
Age at First DC Stay	0.1392	-0.0559	1.1460	0.9082
Black	-0.1547	-0.0160	1.0229	1.0038
Hispanic	0.0540	0.0378	1.1426	1.0924
Other Race	-0.0043	0.0177	0.9554	1.2652
Married	-0.0023	-0.0465	0.9937	0.8782
Divorced, Separated, Widowed	0.0583	0.0158	1.2824	1.0734
Unknown Marital Status	-0.0377	-0.0107	0.6387	0.8874
Custody Level Three	-0.0341	0.0230	0.9884	1.0078
Custody Level Four	0.0263	-0.0329	1.0347	0.9572

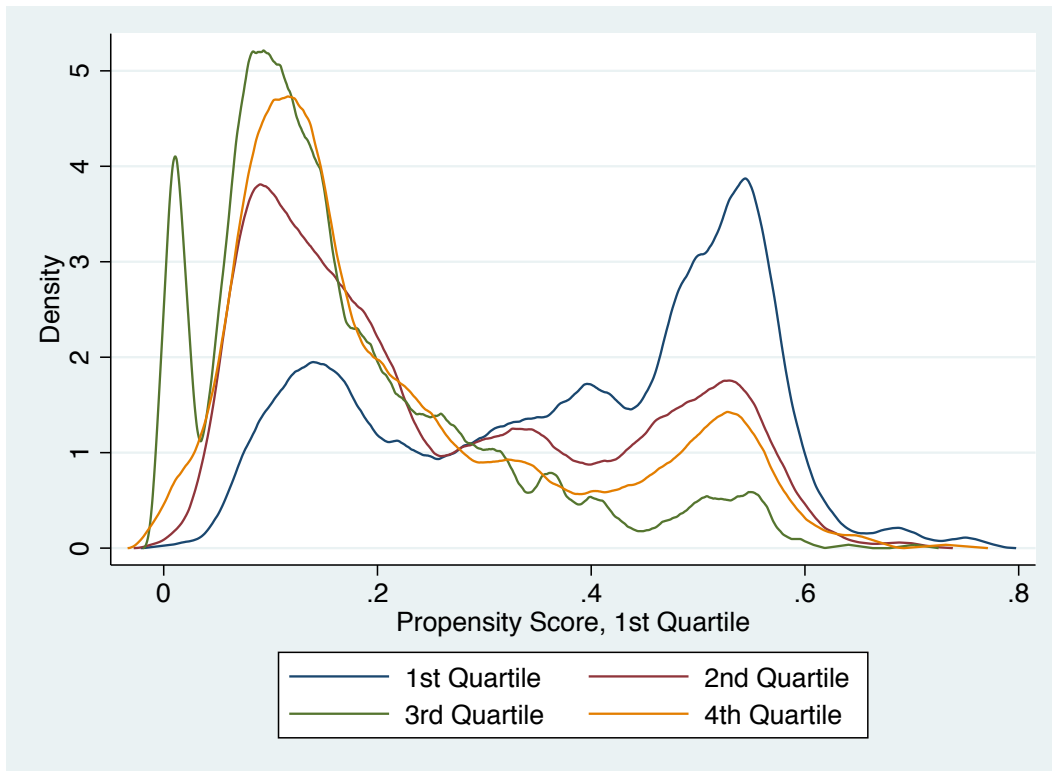
Custody Level Five	0.0350	-0.0354	1.1357	0.8762
Unknown Custody Level	-0.0603	0.0359	0.9261	1.0428
Incarcerated Days	0.2928	-0.0287	1.2516	0.9021
Property Incarceration Offense	0.0486	0.0204	1.1113	1.0496
Drug Incarceration Offense	-0.0101	0.0317	0.9774	1.0726
Other Incarceration Offense	-0.0463	0.0117	0.9243	1.0183
Missing Incarceration Offense	0.0120	-0.0470	1.0288	0.8951
Highest Prior MH C	0.2462	-0.0195	1.5419	0.9662
Highest Prior MH D	-0.0294	-0.0346	0.9073	0.8831
Total Prior Violent Program	0.1191	-0.0317	1.0447	0.8109
Total Prior Alcohol/Drug Program	0.0475	0.0011	0.9847	1.2221
Total Prior Other Program	0.0445	-0.0620	1.1882	0.8082
Current Violent Program	0.0223	0.0144	1.1317	1.0835
Current Alcohol/Drug Program	0.0934	0.0122	1.8035	1.0655
Current Other Program	0.0601	0.0075	1.7393	1.0545
Worked at Time of First DC Stay	-0.0441	0.0374	1.0601	0.9631
Current Property Misconduct	0.0748	0.0225	1.1927	1.0536
Current Drug Misconduct	0.1093	0.0475	1.8740	1.2796
Current Prison Rule Misconduct	-0.1009	-0.0628	0.9893	0.9906
Current Other Misconduct	0.0366	-0.0082	1.0708	0.9841
Prior Misconduct Count	0.1878	0.0064	1.1801	0.8516
Albion	0.0641	-0.0181	1.6392	0.8891
Benner Township	-0.0644	0.0144	0.6361	1.0607
Cambridge Springs	0.1720	-0.0212	3.7253	0.8550
Chester	0.2481	-0.0129	3.7641	0.9380
Coal Township	-0.0050	0.0121	0.9778	1.0624
Dallas	0.1183	-0.0301	1.8142	0.8624
Fayette	0.2277	0.0560	15.3062	1.3459
Forest	-0.2003	0.0116	0.4008	1.0566
Frackville	0.0347	-0.0268	1.4277	0.8280
Graterford	-0.2094	0.0027	0.4687	1.0086
Greene	0.1995	0.0557	5.5716	1.5023
Houtzdale	-0.2888	-0.0015	0.2728	0.9935
Huntingdon	0.1081	0.0058	2.2343	1.0313
Laurel Highlands	-0.0879	-0.0429	0.4570	0.7685
Mahanoy	0.1719	0.0194	2.9823	1.1355
Mercer	0.3760	0.0053	8.5539	1.0348
Muncy	0.2041	-0.0228	2.5812	0.9029
Pine Grove	0.0144	-0.0080	1.0840	0.9679
Pittsburgh	0.2470	0.0055	2.8378	1.0280
Retreat	0.0611	-0.0136	1.7070	0.8925
Rockview	-0.1337	0.0080	0.5092	1.0386
Smithfield	-0.0737	-0.0022	0.5098	0.9851

Somerset	0.0285	0.0154	1.1883	1.0621
Waymart	0.1720	-0.0006	3.7253	0.9969

To address the issues of selection bias inherent in the current study, I utilized IPWRA. The model was estimated and the results of the six pairwise ATEs are shown in Table 26. Prior to a discussion of the results, one must examine the balance of the covariates and the overlap assumption. Table 25 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates. However, there are a few notable exceptions. For example, *Age at First DC Stay* remains out of balance after the model has been weighted between both the 2nd and 3rd quartiles relative to the first quartile. Some covariate imbalance is to be expected, particularly due to the large number of covariates used in the current analyses. However, it does appear that the addition of those serving more than 100% of their sanction increased the issue of covariate imbalance compared to the results displayed in Table 12 from the original multivalued early release results.

Unlike the earlier multivalued early release analyses in the main results in which 19 overlap violators were identified and removed from those analyses, there were no overlap violators identified in the current analyses, meaning everyone in the sample had a non-zero probability of receiving the treatment. Another check to the overlap assumption is the examination of overlap plots. Figure 10 displays the estimated densities of the probability of receiving each treatment level for the multivalued early release treatment quartiles. These results satisfy the overlap assumption in that there are observations in all four treatment categories at each combination of covariate values.

Figure 10. Full Early Release Sample—Multivalued Early Release Overlap Graph
(n=4,014)



After these checks have satisfied the assumptions of inverse probability weighting with regression adjustment, the estimated pairwise ATEs can be examined. As shown in Table 26, unlike the results shown in Table 13 for the original analyses, there are some statistically significant differences between the various quartile comparisons in the likelihood of a subsequent misconduct within one year of release from disciplinary custody. The results largely reflect the addition of those serving more than 100% of their original sanction, as there are statistically significant differences between the fourth quartile and other quartiles. First, there is a positive, but marginally significant mean difference between those in the 4th quartile and those in the 1st quartile, indicating that those in the 4th quartile have a .05 higher likelihood of committing a new misconduct

within one year of release from disciplinary custody than those in the 1st quartile. There are also statistically significant differences for both the 2nd and 3rd quartiles relative to the 4th quartile in that those in the 4th quartile have a .06 higher likelihood of committing a new misconduct post-release from disciplinary custody relative to both the 2nd and 3rd quartiles.

Table 26. Full Sample Multivalued Early Release and Binary Misconduct Pairwise ATE (n=4,014)

	ATE	Robust SE	z	P> z	95% CI	
2 vs. 1	-.0072	.0249	-0.29	0.771	-.0561	.0416
3 vs. 1	-.0050	.0254	-0.20	0.845	.0514	.0448
4 vs. 1	.0514	.0275	1.87	0.062	-.0025	.1053
POmean 1	.6087	.0195	31.25	0.000	.5706	.6469
3 vs. 2	.0023	.0227	0.10	0.920	-.0423	.0468
4 vs. 2	.0587	.0251	2.34	0.019	.0095	.1078
POmean 2	.6015	.0159	37.86	0.000	.5704	.6326
4 vs. 3	.0564	.0255	2.21	0.027	.0064	.1063
POmean 3	.6038	.0166	36.47	0.000	.5713	.6362
POmean 4	.6602	.0196	33.61	0.000	.6217	.6987

These results indicate that those serving more than their original sanction length have a higher likelihood of committing additional misconduct post-release relative to those who serve less than or exactly 100% of their original sanction. These results do not provide support for deterrence theory. It is possible that these results are a reflection of the limited improvement in covariate balance in this model relative to the main analyses. The inherent selection bias in those serving more than their original sanction length, potentially due to misconducts or misbehavior committed while in disciplinary custody, may not be adequately addressed in this model.

Although the quartiles of the early release distribution changed, the results for the days to misconduct model are similar to those described in the main analyses above. As seen in Table 27, there were no statistically significant mean differences between the full sample early release quartiles and days to the first subsequent misconduct. Regardless of the operationalization of early release as either excluding or including those who served more than 100% of their original sanction length, there is no evidence of a deterrent effect of early release on time to subsequent misconduct.

Table 27. Full Sample Multivalued Early Release and Days to Misconduct Pairwise ATE (n=4,014)

	ATE	Robust SE	z	P> z	95% CI	
2 vs. 1	-145.5132	134.3519	-1.08	0.279	-408.8382	117.8118
3 vs. 1	-23.3189	147.8009	-0.16	0.875	-313.0033	266.3656
4 vs. 1	-153.1417	146.9010	-1.04	0.297	-441.0625	134.7790
POmean 1	819.3120	118.0380	6.94	0.000	587.9618	1050.662
3 vs. 2	122.1943	110.7734	1.10	0.270	-94.9175	339.3061
4 vs. 2	-7.6285	109.2943	-0.07	0.944	-221.8414	206.5843
POmean 2	673.7988	65.58268	10.27	0.000	545.2591	802.3385
4 vs. 3	-129.8229	125.4090	-1.04	0.301	-375.6201	115.9744
POmean 3	795.9931	90.0935	8.84	0.000	619.413	972.5732
POmean 4	666.1703	88.4428	7.53	0.000	492.8256	839.515

Early Release as a Proportion of Median Time Served- Binary Treatment

In the next supplemental analyses examining early release, I modified the operationalization of early release. Originally, early release was defined as relative to the original sanction length an individual was assigned by a hearing examiner. For example,

if an individual received a sentence of 30 days in disciplinary custody as punishment for participation in a misconduct event but they were released back to the general population after 15 days, that individual was coded as serving 50% of their original sanction. This operationalization of early release was based on the idea that an individual would expect to serve their full sanction length and an earlier release would impact their risk/reward calculation for the misconduct committed. However, it is possible there are institutional norms that would have a greater impact on an individual's expectations for the amount of sanction to be served rather than the original sanction length assigned. Existing research points to prison managers and correctional officers contributing to a set of understood institutional norms for operating in individual facilities (Dilulio, 1990) and discretionary patterns amongst correctional officers in reporting misconduct incidents (Haggerty & Bucerius, 2020). For example, if it is widespread knowledge that individuals are often released early from a first disciplinary custody stay, then early release might be expected rather than serving the full sanction length. To examine this possibility, this set of analyses defines early release as a proportion of the median time served within each institution. Within this sample, the median proportion served for the first disciplinary custody stay varies widely by institution, from a low of 63.33% in Camp Hill to a high of 103.33% in Mercer, although most institutions have a median proportion time served of 100%. The median proportion time served by institution can be seen in Table 28.

Table 28. Median Proportion Time Served by Institution (n=4,014)

Institution	Median Proportion Time Served
Albion	100%
Benner Township	100%

Cambridge Springs	100%
Camp Hill	63.33%
Chester	100%
Coal Township	83.33%
Dallas	97.78%
Fayette	100%
Forest	66.67%
Frackville	96.67%
Graterford	100%
Greene	100%
Houtzdale	66.67%
Huntingdon	96.67%
Laurel Highlands	95.56%
Mahanoy	95%
Mercer	103.33%
Muncy	100%
Pine Grove	97.78%
Pittsburgh	100%
Retreat	80%
Rockview	81.67%
Smithfield	100%
Somerset	100%
Waymart	100%

The current analyses use the full early release sample used in the supplemental analyses above, with a sample size of 4,014.³⁰ Of this sample of 4,014 individuals, 1,778

³⁰ Original attempts were made to use the original early release sample used in the main analyses. However, due to the changes in the operationalization of early release, there were hundreds of individuals who served 100% of their original sanction length and were thus part of the comparison group in the original analyses, but in these analyses were classified only as a 0 if they were in an institution with a median proportion served as 100%. Otherwise, they were classified as serving more than the median proportion served and were removed from the analyses. Due to these changes, and an additional 312 overlap violators, the sample was reduced to just 2,374. The reduction in sample size resulted in the models for days to follow-up misconduct failing to converge. Thus, the supplemental analyses examining this issue use the full sample, only excluding the 411 individuals whose misconduct offense precluded them from early release eligibility.

(44.29%) were released prior to the median proportion of time served in disciplinary custody in that facility. Prior to estimating the model for the first outcome of interest, binary misconduct, I examined the balance for all covariates across the treatment and comparison groups, and these results are shown in Table 29. This table shows that there are covariate differences between those who are released early from disciplinary custody (relative to the median proportion time served by institution) and those who serve 100% or more. These include the number of days incarcerated prior to the first disciplinary custody stay and several institution dummy variables.

Table 29. Raw and Weighted Covariate Balance Summary for Early Release as a Proportion of Median Time Served: Binary Early Release (n=4,014)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
Age at First DC Stay	-0.0384	-0.0076	0.9585	0.9539
Black	-0.0318	-0.0076	1.0078	1.0018
Hispanic	0.0179	0.0104	1.0459	1.0261
Other Race	0.0118	-0.0093	1.1599	0.8871
Married	0.0566	0.0139	1.1803	1.0405
Divorced, Separated, Widowed	0.0281	-0.0096	1.1261	0.9595
Unknown Marital Status	0.0430	0.0000	1.6709	0.9997
Custody Level Three	0.0611	-0.0187	1.0212	0.9931
Custody Level Four	-0.0402	0.0187	0.9486	1.0242
Custody Level Five	-0.0449	0.0214	0.8511	1.0772
Unknown Custody Level	-0.0126	-0.0075	0.9849	0.9908
Incarcerated Days	-0.1674	-0.0087	0.8422	0.9744
Property Incarceration Offense	0.0256	-0.0116	1.0611	0.9732
Drug Incarceration Offense	0.0144	-0.0033	1.0327	0.9926
Other Incarceration Offense	-0.0278	-0.0086	0.9558	0.9861
Missing Incarceration Offense	-0.0055	0.0135	0.9871	1.0321
Highest Prior MH C	-0.0958	-0.0113	0.8507	0.9812
Highest Prior MH D	0.0914	-0.0030	1.4060	0.9888
Total Prior Violent Program	0.0036	0.0108	1.1214	1.1294

Total Prior Alcohol/Drug Program	0.0329	-0.0201	1.1679	0.9478
Total Prior Other Program	0.0239	0.0022	0.9876	0.9140
Current Violent Program	-0.0363	-0.0021	0.8221	0.9891
Current Alcohol/Drug Program	-0.0326	-0.0127	0.8436	0.9347
Current Other Program	0.0224	-0.0018	1.1824	0.9868
Worked at Time of First DC Stay	0.0922	0.0270	0.9047	0.9705
Current Property Misconduct	-0.0090	0.0070	0.9788	1.0167
Current Drug Misconduct	-0.0526	-0.0345	0.7804	0.8428
Current Prison Rule Misconduct	0.0549	0.0099	1.0109	1.0018
Current Other Misconduct	-0.0324	0.0171	0.9373	1.0343
Prior Misconduct Count	-0.0968	0.0190	0.9029	1.1244
Albion	-0.0298	0.0034	0.8174	1.0227
Benner Township	-0.1602	-0.0012	0.4863	0.9951
Cambridge Springs	-0.0109	0.0055	0.9191	1.0422
Chester	-0.0500	0.0062	0.7735	1.0315
Coal Township	0.0702	-0.0029	1.4449	0.9850
Dallas	0.0475	-0.0001	1.2631	0.9996
Fayette	-0.3518	-0.0117	0.0686	0.9465
Forest	0.0563	0.0005	1.3099	1.0026
Frackville	0.0285	0.0008	1.2206	1.0057
Graterford	0.0481	-0.0008	1.1632	0.9974
Greene	0.0024	-0.0013	1.0165	0.9915
Houtzdale	0.0793	0.0014	1.4020	1.0060
Huntingdon	0.0482	0.0000	1.2842	0.9998
Laurel Highlands	0.0517	-0.0007	1.3929	0.9954
Mahanoy	0.0380	0.0010	1.2789	1.0066
Mercer	-0.0021	0.0001	0.9868	1.0005
Muncy	-0.0603	0.0020	0.7596	1.0089
Pine Grove	0.1051	0.0016	1.5445	1.0065
Pittsburgh	-0.0222	-0.0003	0.8960	0.9984
Retreat	0.0046	-0.0015	1.0403	0.9871
Rockview	0.0666	0.0005	1.3769	1.0025
Smithfield	-0.0253	-0.0009	0.8318	0.9935
Somerset	-0.2018	0.0011	0.4325	1.0043
Waymart	-0.0559	-0.0003	0.7281	0.9982

I ran a model examining the relationship between binary early release and binary misconduct within one year of release from disciplinary custody and these results are displayed in Table 30. Prior to a discussion of these results, I first examined the

assumptions made by the model. First, for binary treatments, there is an overidentification test for covariate balance within Stata. This test performs a chi-squared test to examine the null hypothesis that the covariates are balanced, and the results are shown in Table 31. This test failed to reject the null hypothesis, indicating that the weighted model achieved balance across the covariates. In addition to this check, balance can be assessed by looking at the weighted columns of Table 29, which shows that the covariates are more balanced after the weighted model was run compared to the raw comparisons. Lastly, the overlap between the comparison and treatment groups can be examined by looking at Figure 11, which shows overlap between the two distributions.

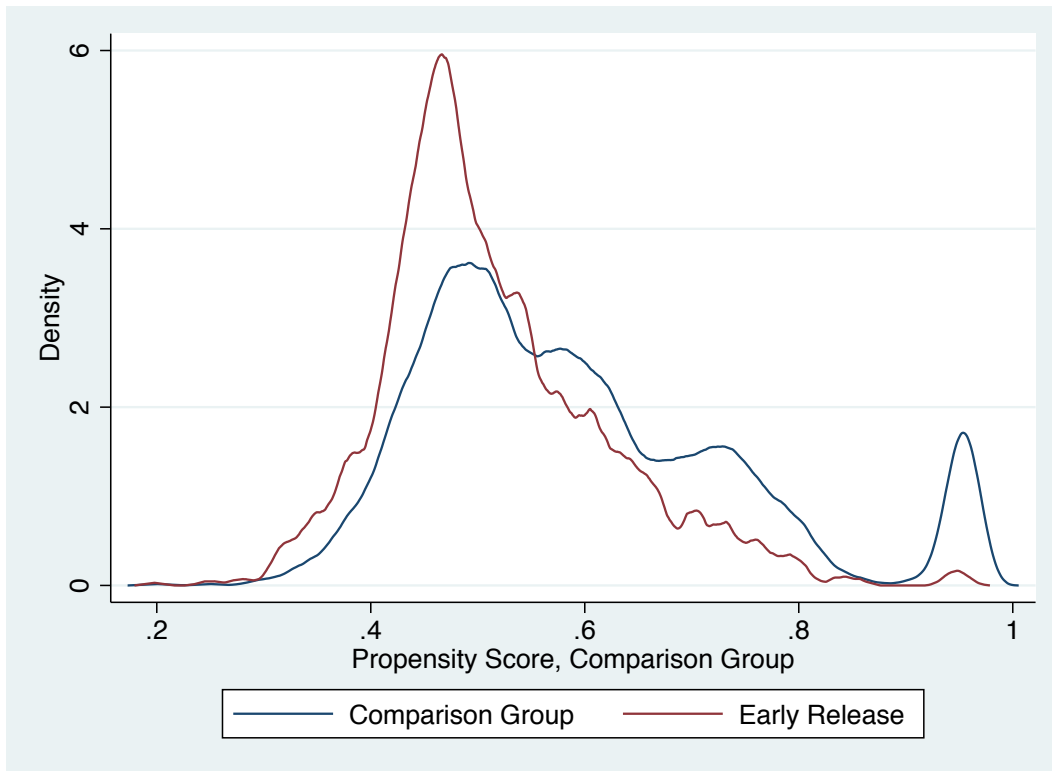
Table 30. Early Release as a Proportion of Median Time Served: Binary Early Release and Binary Misconduct Analyses Pairwise ATE (n=4,014)

	ATE	Robust SE	z	P> z	95%CI	
Early Release (1 vs 0)	-.0055	.0160	-0.34	0.732	-.0368	.0259
POmean	.6165	.0103	60.10	0.000	.5964	.6366

Table 31. Early Release as a Proportion of Median Time Served: Binary Early Release and Binary Misconduct Overidentification Test (n=4,014)

chi2(54)	32.7696
Prob > chi2	0.9925

Figure 11. Early Release as a Proportion of Median Time Served—Binary Early Release
Overlap Graph (n=4,014)



As can be seen in Table 30, there is no statistically significant relationship between early release measured relative to the median proportion time served for the institution and the likelihood of a follow-up misconduct within a year of release from disciplinary custody. These results mirror those presented in the main analyses.

The next outcome examined for these analyses is days to misconduct. Due to the censored nature of days to a subsequent misconduct, linear regression does not adequately address the issues of right-censored data. Prior to estimating the model using IPWRA for survival outcomes, I examined the balance for all covariates across the treatment and comparison groups and these results are shown in Table 29. There are several covariates that are out of balance prior to weighting. Namely, there are clear pre-

existing differences in the early release decision between institutions, as evidenced by the imbalance among several of the institution dummy variables. Among the 4,014 individuals in the early release sample, 2,981 (74.3%) committed a subsequent misconduct prior to release from PADOX or prior to the end of the study period in August 2017.

I ran a model examining the relationship between binary early release and days to subsequent misconduct within one year of release from disciplinary custody and these results are displayed in Table 32. Prior to examining the results, I performed the chi-squared overidentification test for covariate balance examining the null hypothesis that the covariates are balanced. As shown in Table 33, this test failed to reject the null hypothesis, indicating that the weighted model achieved balance across the covariates. In addition to this check, balance can be assessed by looking at the weighted columns of Table 29, which shows that the covariates are more balanced after the weighted model was run compared to the raw comparisons. Lastly, the overlap between the comparison and treatment groups can be examined in Figure 11. Although densities vary, there is overlap across the distribution.

Table 32. Early Release as a Proportion of Median Time Served: Binary Early Release and Days to Misconduct Pairwise ATE (n=4,014)

	ATE	Robust SE	z	P> z	95%CI	
Early Release (1 vs 0)	-34.6150	65.8423	-0.53	0.599	-163.6635	94.4335
POmean	706.9001	47.1430	14.99	0.000	614.5016	799.2986

Table 33. Early Release as a Proportion of Median Time Served: Binary Early Release and Days to Misconduct Overidentification Test (n=4,014)

chi2(54)	32.7696
Prob > chi2	0.9925

As can be seen in Table 32, there is no statistically significant relationship between early release (relative to the median proportion time served of the institution) and days to a follow-up misconduct. These results for the binary early release indicator fall in line with the results for other binary early release models described above. There does not appear to be support for deterrence theory in the examination of a binary treatment of early release, regardless of the operationalization of that treatment.

Early Release as a Proportion of Median Time Served- Multivalued Early Treatment

One issue with this set of analyses is that due to the distribution of the proportion of median time served by institution, it is difficult to divide the distribution into four relatively even groups based on the quartiles. Using the quartile cutoff points creates slightly disproportionate groups: 1st quartile is 0-81.58% (n=1,025); 2nd quartile is 81.59-100% (n=753); 3rd quartile is 100.01-104.65% (n=1,240); and 4th quartile is 104.66% and higher (n=996).

The first outcome of interest is a binary indicator of subsequent misconduct within one year of release from the first disciplinary custody stay. Table 34 shows the standardized differences and variance ratios for all covariates in the multivalued early release model both before and after weighting using the inverse probability of receiving each treatment. This table shows the covariate balance summary for each quartile relative

to the first early release quartile. Covariates such as the mental health variables and a few of the institution dummy variables are out of balance prior to weighting. The differences across the different dosage levels indicate the potential for selection bias and the importance of accounting for such differences prior to making a conclusion of the deterrent capabilities of differential early release (relative to the early release policies of an institution).

Table 34. Raw and Weighted Covariate Balance Summary for Early Release as a Proportion of Median Time Served: Multivalued Early Release (n=4,014)

	Standardized Differences		Variance Ratio	
	Raw	Weighted	Raw	Weighted
2nd Quartile				
Age at First DC Stay	-0.0632	-0.0808	1.0134	0.8793
Black	0.0071	0.0311	0.9989	0.9919
Hispanic	0.0022	0.0484	1.0057	1.1249
Other Race	-0.0602	-0.0378	0.4561	0.5598
Married	0.0390	-0.0447	1.1131	0.8820
Divorced, Separated, Widowed	0.0463	-0.0286	1.2067	0.8770
Unknown Marital Status	0.0054	0.0051	1.0586	1.0644
Custody Level Three	0.0306	-0.0019	1.0088	0.9993
Custody Level Four	0.0085	0.0071	1.0121	1.0094
Custody Level Five	-0.0359	0.0019	0.8723	1.0064
Unknown Custody Level	-0.0170	0.0097	0.9795	1.0117
Incarcerated Days	-0.0901	-0.0227	0.7670	0.8041
Property Incarceration Offense	-0.0830	-0.0388	0.8270	0.9096
Drug Incarceration Offense	-0.0086	-0.0140	0.9817	0.9695
Other Incarceration Offense	-0.0101	0.0156	0.9836	1.0255
Missing Incarceration Offense	-0.0162	0.0382	0.9622	1.0927
Highest Prior MH C	0.0920	-0.0417	1.1848	0.9303
Highest Prior MH D	-0.2200	-0.0687	0.4466	0.7630
Total Prior Violent Program	0.0171	0.0062	1.0657	0.9959
Total Prior Alcohol/Drug Program	0.0075	-0.0396	0.9058	0.8719
Total Prior Other Program	0.0427	-0.0338	1.0351	0.8053
Current Violent Program	-0.1170	-0.0072	0.4878	0.9614

Current Alcohol/Drug Program	-0.0663	-0.0171	0.6888	0.9115
Current Other Program	0.1163	-0.0198	2.3440	0.8708
Worked at Time of First DC Stay	0.0739	0.0522	0.9136	0.9421
Current Property Misconduct	-0.0904	0.0284	0.8005	1.0695
Current Drug Misconduct	0.0589	-0.0368	1.3464	0.8253
Current Prison Rule Misconduct	-0.0612	-0.0136	0.9913	0.9972
Current Other Misconduct	0.0140	0.0021	1.0298	1.0041
Prior Misconduct Count	-0.1416	0.0452	0.6902	1.1813
Albion	-0.0515	-0.0085	0.6856	0.9461
Benner Township	-0.0471	0.0108	0.7672	1.0476
Cambridge Springs	0.0106	-0.0251	1.0879	0.8309
Chester	-0.0610	-0.0115	0.7077	0.9457
Coal Township	-0.1336	-0.0113	0.5109	0.9418
Dallas	-0.0271	0.0035	0.8825	1.0172
Fayette	-0.0135	0.0454	0.8178	1.2550
Forest	-0.1667	0.0021	0.4465	1.0101
Frackville	-0.0663	0.0181	0.6346	1.1246
Graterford	-0.3391	0.0093	0.2978	1.0300
Greene	0.1670	0.0151	3.2575	1.1130
Houtzdale	-0.0885	-0.0063	0.7060	0.9734
Huntingdon	0.1200	0.0053	1.7870	1.0280
Laurel Highlands	-0.0478	-0.0123	0.7526	0.9262
Mahanoy	0.0283	0.0076	1.1860	1.0501
Mercer	0.1509	-0.0101	2.7618	0.9375
Muncy	-0.1988	-0.0383	0.3199	0.8404
Pine Grove	0.2181	-0.0134	2.2247	0.9462
Pittsburgh	-0.1684	0.0070	0.3802	1.0351
Retreat	0.0362	-0.0063	1.3559	0.9484
Rockview	-0.0932	0.0045	0.6567	1.0219
Smithfield	-0.0797	0.0038	0.5240	1.0281
Somerset	-0.1089	0.0121	0.5418	1.0471
Waymart	0.0414	0.0013	1.2913	1.0075
3rd Quartile				
Age at First DC Stay	0.0164	-0.1232	1.0887	0.8551
Black	0.0352	-0.0089	0.9915	1.0019
Hispanic	-0.0423	0.1502	0.8969	1.3922
Other Race	-0.0370	-0.0094	0.6448	0.8803
Married	-0.0420	-0.0774	0.8821	0.7983
Divorced, Separated, Widowed	-0.0003	-0.0025	0.9987	0.9891
Unknown Marital Status	-0.0268	0.0572	0.7364	1.8382
Custody Level Three	-0.0413	-0.0243	0.9855	0.9910
Custody Level Four	0.0304	0.0144	1.0413	1.0191

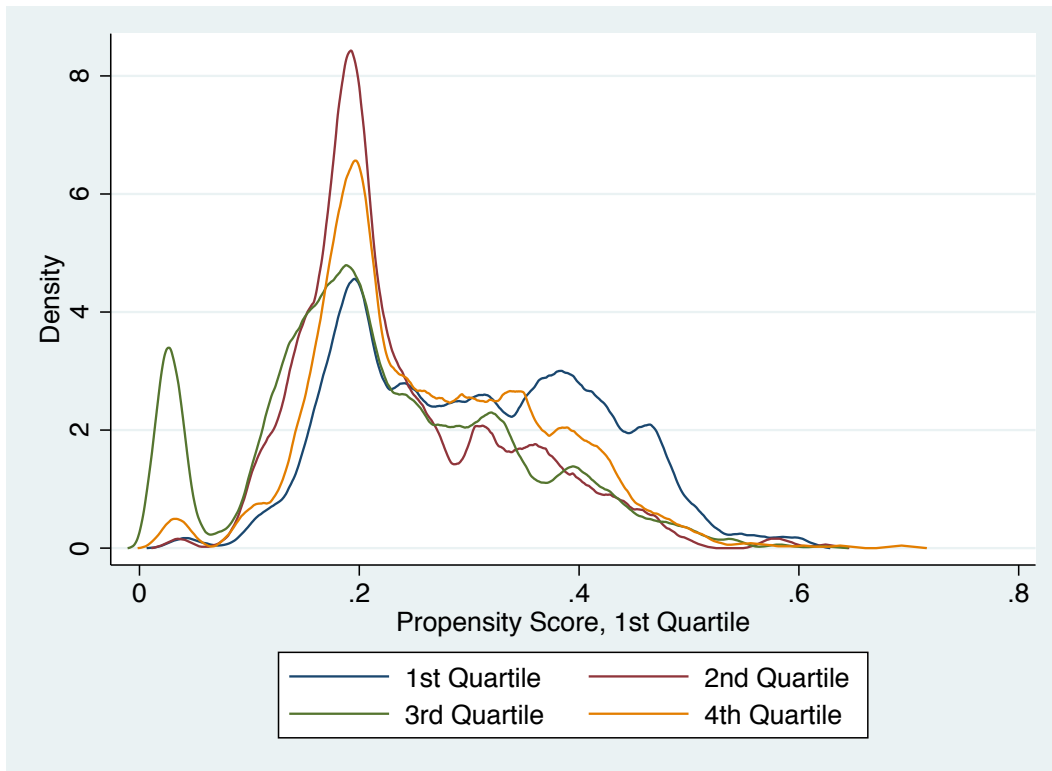
Custody Level Five	0.0382	-0.0478	1.1427	0.8442
Unknown Custody Level	0.0154	0.0385	1.0183	1.0458
Incarcerated Days	0.1905	-0.0562	1.0146	0.7978
Property Incarceration Offense	-0.0720	-0.0638	0.8493	0.8520
Drug Incarceration Offense	-0.0008	-0.0006	0.9982	0.9986
Other Incarceration Offense	0.0273	0.0828	1.0450	1.1336
Missing Incarceration Offense	-0.0085	0.0993	0.9798	1.2422
Highest Prior MH C	0.1569	-0.0162	1.3108	0.9731
Highest Prior MH D	-0.2138	-0.0971	0.4602	0.6717
Total Prior Violent Program	0.0122	-0.0763	0.8978	0.6965
Total Prior Alcohol/Drug Program	-0.0047	-0.0164	0.8438	0.8956
Total Prior Other Program	0.0113	-0.0110	1.0822	0.9208
Current Violent Program	0.0261	-0.0400	1.1332	0.7946
Current Alcohol/Drug Program	0.0588	-0.0188	1.3155	0.9029
Current Other Program	0.0440	-0.0300	1.4387	0.8073
Worked at Time of First DC Stay	-0.1486	0.0524	1.1497	0.9419
Current Property Misconduct	-0.0766	-0.0446	0.8300	0.8925
Current Drug Misconduct	0.0897	0.0872	1.5448	1.4689
Current Prison Rule Misconduct	-0.0969	0.0852	0.9820	1.0090
Current Other Misconduct	0.0005	-0.0977	1.0009	0.8068
Prior Misconduct Count	0.0155	0.0132	0.8815	0.8889
Albion	0.0713	-0.0119	1.5220	0.9250
Benner Township	0.2880	0.0104	2.9457	1.0457
Cambridge Springs	0.0534	-0.0255	1.4769	0.8283
Chester	0.0285	-0.0212	1.1506	0.9007
Coal Township	-0.2679	-0.0238	0.1331	0.8796
Dallas	-0.0519	-0.0041	0.7795	0.9802
Fayette	0.4691	0.0455	20.5004	1.2553
Forest	-0.3168	-0.0197	0.0729	0.9061
Frackville	0.0028	-0.0073	1.0167	0.9514
Graterford	-0.0311	-0.0010	0.9288	0.9966
Greene	0.0820	0.0142	1.9333	1.1059
Houtzdale	-0.3531	0.0479	0.0395	1.2114
Huntingdon	0.0537	0.0087	1.3262	1.0464
Laurel Highlands	-0.0024	-0.0138	0.9868	0.9175
Mahanoy	-0.1488	0.0138	0.2454	1.0926
Mercer	0.1145	-0.0063	2.2508	0.9610
Muncy	0.0515	-0.0043	1.2163	0.9816
Pine Grove	0.0733	-0.0183	1.3682	0.9270
Pittsburgh	-0.1125	-0.0056	0.5631	0.9724
Retreat	-0.0654	-0.0065	0.4851	0.9468
Rockview	-0.3289	0.0316	0.0159	1.1590
Smithfield	0.0644	-0.0030	1.4800	0.9784

Somerset	0.3195	0.0013	2.8949	1.0048
Waymart	0.1646	0.0014	2.3646	1.0078
4th Quartile				
Age at First DC Stay	0.0062	-0.0199	1.0020	0.9658
Black	0.0343	-0.0415	0.9919	1.0078
Hispanic	0.0134	0.0848	1.0331	1.2200
Other Race	-0.0321	0.0410	0.6880	1.6148
Married	-0.0372	-0.0811	0.8954	0.7888
Divorced, Separated, Widowed	-0.0176	0.0236	0.9255	1.1060
Unknown Marital Status	-0.0598	-0.0268	0.4596	0.6938
Custody Level Three	-0.0565	0.0001	0.9797	1.0000
Custody Level Four	0.0604	0.0047	1.0815	1.0063
Custody Level Five	0.0196	-0.0686	1.0726	0.7791
Unknown Custody Level	-0.0071	0.0145	0.9914	1.0175
Incarcerated Days	0.0530	-0.0333	1.1337	0.8858
Property Incarceration Offense	-0.0448	0.0030	0.9060	1.0070
Drug Incarceration Offense	-0.0400	0.0121	0.9133	1.0265
Other Incarceration Offense	0.0187	0.0138	1.0311	1.0227
Missing Incarceration Offense	0.0075	-0.0414	1.0178	0.9012
Highest Prior MH C	0.1087	0.0047	1.2174	1.0078
Highest Prior MH D	-0.1289	-0.0220	0.6608	0.9217
Total Prior Violent Program	-0.0063	-0.0299	0.9396	0.7994
Total Prior Alcohol/Drug Program	-0.0603	0.0271	0.7934	1.3802
Total Prior Other Program	-0.0278	-0.0252	0.9595	0.9729
Current Violent Program	-0.0576	-0.0187	0.7307	0.9015
Current Alcohol/Drug Program	-0.0716	-0.0226	0.6657	0.8839
Current Other Program	0.0208	-0.0091	1.1978	0.9395
Worked at Time of First DC Stay	0.0529	0.0093	0.9386	0.9899
Current Property Misconduct	0.0287	-0.0116	1.0647	0.9718
Current Drug Misconduct	0.0638	-0.0014	1.3772	0.9931
Current Prison Rule Misconduct	-0.0607	0.0241	0.9910	1.0040
Current Other Misconduct	0.0836	0.0100	1.1750	1.0198
Prior Misconduct Count	0.0646	0.0283	1.0714	1.0247
Albion	-0.0918	-0.0209	0.4733	0.8698
Benner Township	-0.1508	0.0120	0.3406	1.0526
Cambridge Springs	-0.0416	-0.0203	0.6893	0.8622
Chester	0.0215	-0.0111	1.1127	0.9475
Coal Township	0.0112	0.0074	1.0462	1.0387
Dallas	-0.0673	-0.0065	0.7184	0.9684
Fayette	0.1241	0.0263	3.6558	1.1448
Forest	0.0478	0.0036	1.1857	1.0177
Frackville	-0.1456	-0.0179	0.2823	0.8830

Graterford	-0.4017	0.0077	0.1914	1.0249
Greene	0.0828	0.0285	1.9442	1.2182
Houtzdale	0.0793	0.0020	1.2913	1.0085
Huntingdon	-0.0606	0.0242	0.6807	1.1317
Laurel Highlands	-0.1822	-0.0190	0.2042	0.8874
Mahanoy	0.0802	0.0075	1.5643	1.0498
Mercer	0.0203	-0.0258	1.1846	0.8441
Muncy	-0.1091	-0.0278	0.5958	0.8831
Pine Grove	-0.1168	-0.0223	0.5124	0.9113
Pittsburgh	0.0337	0.0056	1.1488	1.0283
Retreat	0.0806	-0.0060	1.8669	0.9514
Rockview	0.0767	0.0061	1.3207	1.0297
Smithfield	-0.1267	0.0199	0.2984	1.1505
Somerset	-0.1453	0.0052	0.4116	1.0200
Waymart	-0.0918	0.0159	0.4733	1.0910

Prior to a discussion of the results, one must examine the balance of the covariates after weighting. Table 34 also displays the standardized differences and variance ratios after the model was weighted. The model appears to have improved the balance of the covariates. However, there are a few exceptions. For example, *Age*, *Unknown Marital Status*, and *Other Race* remain out of balance. Some covariate imbalance is to be expected, particularly due to the large number of covariates used in the current analyses. There were no issues of overlap violators identified in the current analyses. Additionally, Figure 12 displays the estimated densities of the probability of receiving each treatment level for the proportion median time served multivalued early release treatment. These results satisfy the overlap assumption in that there are observations in all four treatment categories at each combination of covariate values.

Figure 12. Early Release as a Proportion of Median Time Served—Multivalued Early Release Overlap Graph (n=4,014)



After these checks have satisfied the assumptions of IPWRA, the estimated pairwise ATEs are shown in Table 35. There are limited statistically significant differences between the various quartile comparisons in the likelihood of a subsequent misconduct within one year of release from disciplinary custody. There is a statistically significant difference between the 2nd and 4th quartiles. Those in the 4th early release quartile have a .05 higher likelihood of committing a misconduct within a year of release from disciplinary custody. Thus, it appears that those who serve more than 104.7% of the median proportion time served have a higher likelihood of follow-up misconduct compared to those serving between 81.6% and 99.99%. This result does not appear to lend support to deterrence theory in that those serving a higher proportion of their

sentence relative to the median proportion within that institution have higher likelihoods of follow-up misconduct, at least relative to those serving slightly less than the median proportion.

Table 35. Early Release as a Proportion of Median Time Served: Multivariate Early Release and Binary Misconduct Pairwise ATEs (n=4,014)

	ATE	Robust SE	z	P> z	95% CI	
2 vs. 1	-.0420	.0257	-1.63	0.102	-.0923	.0084
3 vs. 1	-.0261	.0264	-0.99	0.324	-.0779	.0257
4 vs. 1	.0115	.0252	0.46	0.647	-.0378	.0609
POmean 1	.6259	.0167	37.43	0.000	.5931	.6587
3 vs. 2	.0159	.0284	0.56	0.576	-.0397	.0715
4 vs. 2	.0535	.0272	1.97	0.049	.0002	.1068
POmean 2	.5839	.0198	29.53	0.000	.5452	.6227
4 vs. 3	-.0376	.0278	1.35	0.176	-.0169	.0922
POmean 3	.5998	.0206	29.15	0.000	.5595	.6401
POmean 4	.6374	.0190	33.50	0.000	.6001	.0169

In examining the second outcome, days to subsequent misconduct, it is necessary to account for the censoring inherent in this variable. Table 34 shows the standardized differences and variance ratios for all covariates in the multivalued early release model both before and after weighting. As mentioned previously, prior to weighting, several of the institution dummy variables consistently have large standardized differences across the quartiles relative to the first quartile.

To address the issues of selection bias inherent in the current study, I utilized IPWRA for survival outcomes. For the multivalued early release model, the treatment was broken down into the quartiles of the early release distribution, and the outcome was the number of days to a subsequent misconduct following release from disciplinary custody. This model was estimated and the results of the six pairwise ATEs are shown in

Table 36. Prior to a discussion of the results, I first checked the assumptions required by IPWRA. First, I examined the balance of the covariates after weighting. Table 34 also displays the standardized differences and variance ratios after the model was adjusted using the inverse probabilities of treatment. The model appears to have improved the balance of the covariates. However, there are a few exceptions as discussed above. No overlap violators were detected in the current analyses, and as can be seen in Figure 12, there is overlap between the four treatment categories, satisfying the overlap assumption.

After these checks have satisfied the assumptions of inverse probability weighting, the estimated pairwise ATEs can be examined and discussed. As shown in Table 36, are no statistically significant relationships in the multivalued early release analyses. These results do not provide support for differential deterrent effects resulting from differential proportions of time served.

Table 36. Early Release as a Proportion of Median Time Served: Multivariate Early Release and Days to Misconduct Pairwise ATEs (n=4,014)

	ATE	Robust SE	z	P> z	95% CI	
2 vs. 1	128.4645	134.0678	0.96	0.338	-134.3036	391.2325
3 vs. 1	-43.0876	101.5659	-0.42	0.671	-242.1531	155.9778
4 vs. 1	98.9396	143.4036	0.69	0.490	-182.1264	380.0055
POmean 1	672.4759	71.1223	9.46	0.000	533.0788	811.873
3 vs. 2	-171.5521	135.2827	-1.27	0.205	-436.7013	93.5972
4 vs. 2	-29.5249	168.9220	-0.17	0.861	-360.606	301.5562
POmean 2	800.9404	114.4654	7.00	0.000	576.5923	1025.2880
4 vs. 3	142.0271	143.7527	0.99	0.323	-139.7231	423.7773
POmean 3	629.3884	73.0501	8.62	0.000	486.2129	772.5639
POmean 4	771.4155	125.3219	6.16	0.000	525.7891	1017.042

Summary

Table 37 provides a summary table of the results presented in this dissertation. The main six analyses discussed above did not provide support for any of the current study's hypotheses rooted in deterrence theory. Some of the findings even lend support for the tenets of defiance theory (Sherman, 1993); although without data on perceptions of treatment heterogeneity in disciplinary custody stays, it is not possible to isolate the mechanism behind these relationships.

The first set of supplemental analyses examined a separate indicator of restrictive deterrence, a decrease in the severity of a subsequent misconduct. However, these results did not lend support to deterrence theory as those serving the longest sanction lengths had a lower likelihood of committing a less serious follow-up offense. Additionally, there was no relationship between binary early release and a subsequent severity change in misconduct. Due to issues of sample size, the models examining the multivalued early release treatment were not able to converge.

The second set of supplemental analyses examined early release while including all those who served more than 100% of their original sanction length. While the addition of these individuals did not affect the binary early release results, there was an impact on the multivalued early release and the binary misconduct model. These results showed that those who served more than 100% of their original sanction length (the fourth quartile of the multivalued early release distribution) were more likely to commit a follow-up misconduct within one year of release from disciplinary custody relative to those who served 100% or were released early. However, there was no relationship between the multivalued early release and time to subsequent misconduct. These results do not

provide support for deterrence theory, as those serving more than their original sanction length actually had higher likelihoods of committing future misconduct.

Finally, the last set of supplemental analyses examined another operationalization of early release, rather than examining early release as a proportion of the original sanction length assigned by an individual hearing examiner, early release was operationalized as relative to the median proportion time served within the institution where the misconduct and disciplinary custody stay took place. While there was no relationship between early release and misconduct for either of the binary early release treatment models or for the multivalued early release and days to subsequent misconduct models, there was a statistically significant difference within the multivalued early release and binary misconduct model. That result showed that those in the fourth quartile of the early release distribution (those serving 104.7%+) had a higher likelihood of subsequent misconduct relative to the 2nd quartile. While it is unexpected for there to be a relationship between the 2nd and 4th quartiles and none of the other quartile pairwise ATEs, this result does not support deterrence theory. The theoretical implications of these results will be discussed further below.

Table 37. Summary of Results

	Sample Size	Statistically Significant Pairwise ATEs
Main Analyses		
Length of Stay and Binary Misconduct	4,425	Those with the longest lengths of stay (i.e., the 4 th quartile of the distribution) were significantly more likely to commit a follow-up misconduct within a year of release relative to the other quartiles.
Length of Stay and Days to Misconduct	4,425	Those with the longest lengths of stay (i.e., the 4 th quartile of the distribution;

		those serving 56 or more days) had significantly fewer days to a follow-up misconduct relative to those in the 3 rd quartile (serving between 31 and 55 days).
Binary Early Release and Binary Misconduct	3,275	No significant mean difference
Binary Early Release and Days to Misconduct	3,275	No significant mean difference
Multivalued Early Release and Binary Misconduct	3,275	No significant mean differences
Multivalued Early Release and Days to Misconduct	3,275	No significant mean differences
Supplemental Analyses		
Change in Severity		
Length of Stay	2,792	Those serving the longest stays in DC have significantly lower likelihoods of committing less severe follow-up misconducts relative to those in the 1 st and 2 nd quartiles; there was a marginally significant difference between the 3 rd and 4 th quartiles.
Binary Early Release	2,045	No significant mean difference
Multivalued Early Release	2,045	Model did not converge
Full Sample Early Release		
Binary Early Release and Binary Misconduct	4,014	No significant mean difference
Binary Early Release and Days to Misconduct	4,014	No significant mean difference
Multivalued Early Release and Binary Misconduct	4,014	Those serving more than 100% of their original sanction length were significantly more likely to commit a new misconduct relative to those serving 60-96.67% and those serving 96.68-100%; there was a marginally significant difference between those serving 100% and those serving less than 60% of their original sanction length.
Multivalued Early Release and Days to Misconduct	4,014	No significant mean differences
Early Release as a Proportion of Median Time Served		
Binary Early Release and Binary Misconduct	4,014	No significant mean difference
Binary Early Release and Days to Misconduct	4,014	No significant mean difference

Multivalued Early Release and Binary Misconduct	4,014	Those serving more than 104.7% of the median proportion time served in that institution have a higher likelihood of follow-up misconduct relative to those in the 2 nd quartile (serving between 81.6% and 99.99% of the median proportion time served).
Multivalued Early Release and Days to Misconduct	4,014	No significant mean differences

Chapter 5: Discussion

The motivation for this dissertation was to examine the possible deterrent capabilities of the use of disciplinary custody as a punishment for institutional infractions within a large state correctional system. Disciplinary custody is used in every state and federal correctional system but there has to this point been little evidence supporting a specific deterrent effect of the punishment on subsequent behavior (Labrecque, 2015; Labrecque & Smith, 2019; Lucas & Jones, 2017; Medrano et al., 2017; Morris, 2017; Woo et al., 2019). However, there are significant drawbacks to the use of solitary confinement generally, necessitating research examining all types of solitary confinement, including disciplinary custody. Mainly, there has been documented mental health deterioration associated with stays in isolation conditions (Arrigo & Bullock, 2008; Haney, 2003, 2018; Haney & Lynch, 1997; Smith, 2006), housing individuals in isolation conditions is significantly more expensive than housing them in the general prison population (Browne et al., 2011; Reiter, 2012), and there is some evidence of within-institutional behavior and experiences impacting subsequent recidivism post-release (Cochran et al., 2014).

Given the intolerable nature of solitary confinement as a whole, research examining the stated justifications for these policies is imperative. Moreover, prior research examining disciplinary custody and subsequent behavior has suffered from methodological inconsistencies and data limitations, namely treating disciplinary custody as a singular experience and ignoring all aspects of heterogeneity in that experience

(Lucas & Jones, 2017; Medrano et al., 2017; Morris, 2016). Subsequent studies have begun to address this limitation by examining one area of treatment heterogeneity, length of stay, and examining subsequent misconduct (Labrecque & Smith, 2019; Woo et al., 2019). However, one of these studies grouped all solitary confinement types (Labrecque & Smith, 2019), ignoring the different purposes of the varying uses of solitary confinement, and another study limited the sample and the types of behavior examined (Woo et al., 2019). In order to address these gaps in the literature, this dissertation examined two types of treatment heterogeneity in disciplinary custody stays: length of stay and early release.

This dissertation tested six hypotheses rooted in deterrence theory, examining whether differences in length of stay, a binary indicator of early release, or a multivalued early release treatment resulted in subsequent differences in institutional behavior. To examine these research questions, I used inverse probability weighting with regression adjustment to control for pre-existing differences in the sample and examined both the likelihood of subsequent misconduct and days to subsequent misconduct in the main analyses. An additional outcome of interest, capturing restrictive deterrence, the change in severity of subsequent misconduct, was also examined in the supplemental analyses.

There are three main takeaway lessons from the current research. First and foremost, this dissertation does not find any support for deterrent effects stemming from heterogeneity in disciplinary custody sanctions. In none of the models examined did those serving more severe disciplinary custody stays, either by serving longer stays or by serving 100% or more of their original sanction length, display a lower likelihood for future misconduct or a longer time until a subsequent misconduct. Thus, these results do

not provide support for either absolute or restrictive deterrence. Due to the use of administrative data in the current study, it is not possible to pinpoint the mechanism behind the lack of support for the tenets of deterrence theory, discussed further below in the limitations section. There are many potential reasons for the lack of deterrent finding. For example, it is possible that differences in disciplinary custody experiences do not result in changes in the risk/reward calculation of deterrence theory (Nagin, 2013). Another possible explanation is that minimal changes in length of stay or in the percent of the original sanction served do not contribute to the perceived severity of disciplinary custody. Disciplinary custody is a severe environment, placing restrictions on social and environmental stimuli. It is possible that just a short-term stay is perceived as a severe punishment and additional days spent in these severe conditions do not substantively increase the perceived severity of that punishment. These findings align with some of the research finding a lack of deterrent effects between incarceration length and subsequent recidivism (Nagin, Cullen, & Jonson, 2009; Snodgrass et al., 2011). As stated by Nagin (2013), “Unless perceptions adjust, however crudely to changes in the sanction regime, the desired deterrent effect will not be achieved” (p. 204). Future research is needed to fully understand the lack of deterrent effects resulting from heterogeneity in disciplinary custody stays.

Second, this dissertation provides some tentative support for the tenets of defiance theory (Sherman, 1993). Relative to those serving lower sanction lengths, those serving the longest stays in disciplinary custody (the 4th quartile of the distribution) were significantly more likely to commit additional misconduct, had significantly fewer days to a follow-up misconduct, and were significantly less likely to commit less severe

follow-up misconducts. Additionally, in the set of supplemental analyses examining early release that included those who served more than 100% of their sanction length, those serving more than 100% of their sanction were significantly more likely to commit a follow-up misconduct within a year of release from disciplinary custody relative to those who were released early (i.e., in the 1st and 2nd quartiles who served less than 100% of their original sanction length). Together, these findings indicate potential support for defiance theory. First, those serving longer sanction times have significantly worse outcomes, indicating the opposite of what would be expected given deterrence theory. Sherman's (1993) defiance theory posits that punishments seen as fair or legitimate are more likely to result in deterrent effects, but a punishment may be seen unfair and result in a defiant reaction if it is "substantially arbitrary, discriminatory, excessive, undeserved, or otherwise objectively unjust" (p. 461). Defiance theory would thus hypothesize that longer disciplinary custody stays could result in a defiant reaction if the longer stays are perceived as unjust or excessive (Sherman, 1993). Additionally, the set of supplemental analyses finding that those serving more than 100% of their original sanction were significantly more likely to commit subsequent misconduct (see Table 26) also points to this potential mechanism. Those who expect to serve a specific sanction length and then must serve a longer stay due to misbehavior or other factors within disciplinary custody may view that as an unfair punishment and thus have a defiant reaction after release from disciplinary custody. These results align with findings in one prior study in which the authors plotted punishments prior to and after a solitary confinement stay and found that there was a heightened disciplinary issue after exposure to solitary confinement compared to prior to the punishment (Medrano et al., 2017). Although this is a prior study that

examined disciplinary custody as a singular experience, it does provide some support for the findings of the current study of the potential for a defiant reaction to disciplinary custody.

Another potential theoretical avenue of explaining these findings could be general strain theory (GST) (Agnew, 1992). GST has been applied to imprisonment experiences, highlighting that the pains of imprisonment such as exposure to a threatening environment or victimization could impact misconduct and subsequent recidivism (Listwan, Sullivan, Agnew, Cullen, & Colvin, 2013; Morris, Carriaga, Diamond, Piquero, Piquero, 2012). In the same vein, GST can be applied to solitary confinement experiences in that exposure to isolation conditions could negatively impact personal relationships with other inmates, prison staff, or family members due to restricted visitation (Listwan et al., 2013). Additionally, the strain associated with long-term solitary confinement stays has been shown to result in physical and psychological harm (Arrigo & Bullock, 2008; Haney, 2003; Haney & Lynch, 1997; Smith, 2006) and these consequences could explain other negative behavioral outcomes. Future research leveraging qualitative interviews with those experiencing solitary confinement could attempt to differentiate between competing theoretical explanations for these findings.

However, these results should be taken with caution. The results that those who spend the longest terms in disciplinary custody have the highest likelihood of subsequent misconduct as well as fewer number of days until a subsequent misconduct may also be attributed to the potential that the propensity for misbehavior has not been fully accounted for by the model. In other words, these results could also be explained by the premise that those placed in disciplinary custody for longer periods or who serve more

than their original sanction length have a higher level of risk of misbehavior to begin with, thus resulting in a higher level of misconduct post-release from disciplinary custody. The methodology used in the current study, IPWRA, aims to control out these pre-existing differences in the sample and the balance achieved in the covariates shows support for the controlling of pre-existing differences among all *measured* covariates.³¹ The current study utilizes a wide-range of covariates capturing aspects of individual demographics, institutional life, and risk factors for both the treatment and outcome models. However, there is the possibility of unobserved bias, which could lead to the results shown here, discussed in more detail in the limitations section below. The potential for unmeasured confounding is a core limitation of IPWRA and propensity score methods in general, and inferences should therefore be cognizant of this possibility.

Third, this dissertation provides some evidence that early release from disciplinary custody does not have a detrimental impact on subsequent behavior within prisons, indicating that this is a potential avenue of relief from some of the detriments of extended disciplinary custody stays. This is the first study that examines the effects of early release from disciplinary custody on institutional misconduct following release back to the general prison population. Although additional research is needed, the current results indicate there is no relationship between release prior to serving the full 100% of the assigned sanction length and subsequent behavior. Once those serving more than 100% of their sanction length are added to the early release models in the second set of supplemental analyses (see Table 26) there is a significant effect of those serving more

³¹ However, as documented in the supplemental analyses examining the full early release sample, the improvement to covariate balance after weighting the multivalued early release model was limited. The possibility that this model did not control for all pre-existing differences is supported by these results. The length of stay models substantively improved covariate balance in all models examined.

than 100% of their original sanction length; however, this group was significantly more likely to commit a new misconduct relative to those serving 100% or less of their original sanction length. These results lend some support to the possibility that early release (along with other punishment types, see below) provides a possible tool to limit the suffering of some of those placed within disciplinary custody without causing issues for the safety and order of the general prison population. As currently used, early release provides an outlet to minimize the severity of a disciplinary custody sanction for a group of individuals without impacting the institutional rule-breaking of that group post-release. Future work should examine possibilities of expanding the use of early release from disciplinary custody.

Limitations

Although this dissertation addressed some of the gaps in prior literature, there are limitations to the current research. First and foremost, it relies on administrative data to examine differences in disciplinary custody experiences and potential deterrent effects. However, deterrence theory at its core is a perceptual theory (Nagin, 2013). As stated by Nagin (2013), “Deterrence is the behavioral response to the perception of sanction threats” (p. 204). As data on individual perceptions of punishments within institutions is not available, I have relied on heterogeneity in disciplinary custody stays to serve as proxies for differences in perceived severity. For example, the current study examines early release as a treatment. Although perceptual measures are not available, the assumed mechanism is that those who are released early from their disciplinary custody stay update their perceptions based on that experience. This is based on research that has found that individuals update their risk perceptions based on personal offending and

sanctioning experiences (Anwar & Loughran, 2011; Horney & Marshall, 1992; Lochner, 2007; Matsueda et al., 2006). However, it is possible that sanctions within prison do not operate in the same way on individual perceptions as apprehension or incarceration does in these risk-updating studies. Individuals may have perceptions that are not grounded in or susceptible to changes in their disciplinary custody sanctions.

Thus, the current study cannot explicitly test the mechanism of deterrence theory. It is possible that changes in the objective severity of punishment, such as a longer stay in disciplinary custody, do not result in a change in subjective perceptions of the severity of punishment. However, prison officials and administrators rely on administrative data in making their decisions regarding day-to-day prison operations as well as policy changes. Thus, the current study's use of administrative data and the findings of a lack of a deterrent effect of changes in disciplinary custody sanctions are informative to the practitioners who deal with these punishments.

A second limitation of the current study is the possibility of confounding. One of the main assumptions of inverse probability weighting with regression adjustment is the ignorability assumption (Rosenbaum & Rubin, 1983), which holds that there is no unmeasured confounding in the model (Cole & Hernan, 2008; Wooldridge, 2010). The current study uses a wide range of covariates capturing multiple aspects of individual characteristics, time within prison, misconduct factors, and the institution in which the misconduct took place, all of which are theoretically relevant for predicting the treatments as well as the behavioral outcomes. However, there are some additional variables which may contribute unmeasured confounding into the models. For example, PADOX uses a risk assessment tool to identify those in need of specific programming or

higher levels of security. In the current study, there was significant missing data in this variable, requiring the use of other covariates that are used by PADOX in their risk assessment model and are highly correlated to the risk assessment score. However, there is the possibility that the risk assessment score could provide additional information above and beyond its component parts.

Additionally, and more importantly, propensity score methods are only able to assume unmeasured confounding based on the covariates in the model that are measured prior to the imposition of the treatment condition. Thus, in the current study, the treatment is only independent of the covariates that are observed *prior to* the first disciplinary custody stay. Thus, any behavior or changes to the underlying covariates that take place while the individual is in disciplinary custody could be a source of potential confounding. The main concern is with the issue of committing an infraction within disciplinary custody, an act that would likely affect both treatments, length of stay and early release, as well as the outcome of subsequent misconduct. This is a limitation of the current study and thus, lends caution to the results. Although committing an infraction within disciplinary custody increases the likelihood of a longer disciplinary custody stay, and decreases the likelihood of early release, infractions within the disciplinary custody unit do not preclude individuals from early release. Among all those eligible for early release (n=4,014), only 539 (13.43%) committed a misconduct during their first stay. Among those who were released early, only 160 (7.5% of the 2,132 released prior to serving 100% of their sanction) had a misconduct within disciplinary custody (compared to 379 (20.14%) of the 1,882 who served 100% or more of their sanction length).

Regardless of the results, the possible confounding related to behavior while in the disciplinary custody unit is a limitation of the current study.

There is also an issue of not having data on the decision-makers themselves, namely the hearing examiners who determine the original sanction and how long that sanction is to be served, and the Program Review Committee (PRC) who make the early release decision. Although this study includes many covariates that would be expected to impact the hearing examiner and PRC's decision-making, data on the decision-makers themselves was not available. It is likely that individual hearing examiners or members of the PRC have specific goals in mind when making punishment decisions, and these goals could contribute to patterns of sanction lengths or early release decisions. Additionally, this study lacks data on the social experiences and organization of the prisons themselves, important context that could impact both punishment decisions and misconduct rates. Some of this discretion is likely captured within the institutional dummy variables included in the models as institutions generally have only one or two hearing examiners responsible for making the punishment decisions and there is likely overlap in who serves on the PRC within each institution. The institutional dummies also provide some context on the organizational factors, as there are likely differences between institutions in day-to-day operations impacting misconduct decisions. However, information on decision-makers and institutional factors would improve the model and provide additional context for how and why particular sanctioning decisions are made. This is a critique applicable to much of the research examining solitary confinement in that these studies pay "limited attention to institutional patterns such as fluctuations in bed capacity, shifts in

demographic make-up, and reforms or retrenchments in policies governing solitary confinement and release” (Lovell et al., 2020, p. 1305).

Lastly, it is important that we bound inferences based on the exclusion criteria and the resulting generalizability of the current study. Multiple exclusion criterion had to be implemented for the current study due to the specific nature of the research questions. The current study only set out to examine heterogeneity in disciplinary custody, thus by definition it could only include those individuals housed within PADOX with at least one disciplinary custody stay. To address issues of possible confounding extending from prior incarceration stays or prior disciplinary custody experiences, the sample was restricted to those serving their first disciplinary custody stay within their first incarceration stay within the state of Pennsylvania. Finally, to allow for adequate follow-up time, this study excluded all those who were released from PADOX prior to serving one full year after release from disciplinary custody. These restrictions, although necessary for the research questions at hand, restrict the sample to a very specific group of individuals. This study thus cannot generalize to all disciplinary custody stays or to disciplinary custody stays among those with prior incarcerations. This group is, as expected, a riskier sample than the general prison population as they have committed at least one misconduct serious enough to result in a disciplinary custody sanction and were incarcerated for a long enough term to allow for enough follow-up after release from disciplinary custody. As this is a sample with a likely higher underlying propensity for misbehavior, the current study provides a more conservative analysis of the disciplinary custody, by examining whether that punishment has the possibility of deterring those who are most likely to receive such a punishment. Although there were substantive sample restrictions in the

current study, the results provide estimates for understanding how a first time disciplinary custody stay during a first time incarceration affects subsequent misconduct. As this sample has higher rates of misconduct than the general prison population, it is worthwhile to understand how disciplinary custody can affect this group's behavior, as finding ways to limit institutional misconduct among this group could improve overall institutional safety.

Future Directions

The current study adds to our understanding of the behavioral effects of disciplinary custody sanctioning. Taken with prior findings, research to date shows that disciplinary custody, either measured as a single event relative to other punishments, or as the number of days spent in disciplinary custody, does not have a specific deterrent effect on subsequent misconduct within prisons, at least among first disciplinary custody stays. However, there are multiple areas of future research that should be explored to further our understanding of punishment practices within prisons prior to making policy prescriptions. The current research supports the lack of specific deterrent effects among a sample of first-time incarcerated individuals who commit a misconduct and serve a first disciplinary custody stay as a result. A first-time disciplinary custody stay would be expected to have the largest impact on individual perceptions of the punishment, as individuals update their perceptions based on new information (Anwar & Loughran, 2011). However, of interest to policy makers would be the role that disciplinary custody could play in the larger context of prison management, and the potential role of this punishment in incapacitation and general deterrence among other incarcerated individuals. For example, higher rates of disciplinary custody usage in a unit or facility

could have general deterrent effects of limiting institutional rule breaking within that unit as a whole. These are avenues that should be explored in future work to provide a better understanding for practitioners on the role of the sanction patterns within their institutions.

First, future research should collect and examine perceptual measures of certainty, severity, and celerity to fully understand the relationship between disciplinary custody and deterrence. Qualitative data on incarcerated persons is limited in all aspects of the correctional system (Kreager, Young, Haynie, Schaefer, Bouchard, & Davidson, 2020; Travis et al., 2014), and to my knowledge, no study to date has examined individual perceptions of institutional rules or punishment practices. This is an important step in understanding an area of the criminal justice system that has received limited attention.

Second, additional aspects of treatment heterogeneity in disciplinary custody experiences could contribute to deterrent capabilities of this punishment. Research to this point has generally concluded that differences in length of stay in disciplinary custody has minimal to no significant deterrent impact on subsequent misconduct (Labrecque, 2015; Labrecque & Smith, 2019; Lucas & Jones, 2017; Medrano et al., 2017; Morris, 2017; Woo et al., 2019), and the current study also draws the same conclusion. However, this is the only study that examines an additional type of treatment heterogeneity in disciplinary custody experiences, early release. Future research should examine other aspects of the disciplinary custody experience, as it is imperative to further understanding the impacts of disciplinary custody. For example, one particular source of heterogeneity that deserves further attention is single- and double-celling within disciplinary custody. Prison overcrowding has contributed to the common practice of double-celling within

isolation units (Browne et al., 2011), an environmental factor that would be expected to have a substantive impact on the experience of disciplinary custody. The practice of double-celling goes against what is expected when serving a term of “solitary” confinement. It is possible that the companionship of another individual lessens the hardships associated with a disciplinary custody term or, conversely, the limited privacy associated with being housed with another individual in a confined space for 23 hours a day could contribute additional difficulties to an already severe punishment. These conditions could thus affect the severity calculus of the punishment of disciplinary custody, resulting in subsequent differences in deterrent effects.

This study focused solely on examining treatment heterogeneity in disciplinary custody sanctions. Although research is needed on additional aspects of treatment heterogeneity, another area of future research is the further examination of treatment effect heterogeneity. Prior research has examined differences in effects by individual factors such as gender and gang membership (Labrecque, 2015; Medrano et al., 2017). Future research should continue this avenue, with the potential to examine both treatment heterogeneity and treatment effect heterogeneity within the same studies, examining how differences in disciplinary custody stays have differing effects by individual or institutional characteristics.

Another area of research deserving future attention is that of other punishments for institutional misconduct besides disciplinary custody. To date, there is no research examining the assignment of other punishments following an institutional infraction or of the outcomes of such punishments. Although disciplinary custody is a common punishment, particularly in Pennsylvania’s system, the general conclusions of prior

research and of the current dissertation calls into question the main justification used for this controversial practice. Other punishments are available in response to a misconduct event. For example, within PADO, other punishments available to hearing examiners following a guilty verdict include: cell restriction; confiscation of contraband; loss of privileges; payment for property loss; reprimand, warning, counseling; revoking pre-release status; and suspension/removal from job. Often these punishments are given in conjunction with one another. Future research could examine how these punishments are given and the behavioral outcomes associated with each one. This research will likely also need to rely on qualitative methodologies, particularly using interviews with both incarcerated persons, to understand how these punishments are perceived, and with hearing examiners, to provide a greater understanding of the punishment decision process itself.

Conclusion

This dissertation set out to examine the potential deterrent capabilities of solitary confinement as a punishment for institutional misconduct. Addressing limitations in prior studies, this study examined two aspects of treatment heterogeneity in disciplinary custody stays: length of stay and early release. Overall, this study fails to find any support for deterrence theory, those with longer stays in disciplinary custody actually have a higher likelihood of subsequent misconduct and a shorter number of days to subsequent misconduct. Additionally, there were no significant deterrent effects for the early release treatment, regardless of the operationalization of that treatment. Taken in conjunction with prior research, these results call into question the ability of disciplinary custody, or differences in disciplinary custody sanctions, to result in deterrent outcomes.

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