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The Deterrent Effect of Disciplinary Segregation on Prison Inmate Misconduct

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Walden University
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Abstract

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by

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JD, Willamette University College of Law, 2011

BS, University of Oregon, 2007

Dissertation Submitted in Partial Fulfillment

of the Requirements for the Degree of

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Abstract

Although a widely used practice, it was previously unknown whether disciplinary segregation is actually effective at modifying prison behavior. This quantitative, retrospective observational study tested deterrence theory and explored the effectiveness of disciplinary segregation in deterring subsequent prison inmate misconduct among those subjected to it (N = 228). It compared a cohort of male inmates incarcerated by the Oregon Department of Corrections who had spent time in disciplinary segregation in 2011 and/or 2012 with a comparison cohort who had not spent any time in disciplinary segregation. Three models were tested, each with the outcome variable operationalized in a different way: overall total rule violations in 2013-2014, total major rule violations in 2013-2014, and total minor rule violations in 2013-2014. Multiple regression analysis was used to control for the influence of age, time spent on current sentence, risk score, prior major and minor rule violations, and time spent in disciplinary segregation. These analyses revealed that for each model, disciplinary segregation was not a significant predictor of subsequent prison inmate misconduct. The findings suggest that deterrence theory does not explain the relation between the experience of disciplinary segregation and subsequent prison misconduct among those subjected to it. The findings further suggest that disciplinary segregation neither decreases nor increases subsequent prison misconduct. These results indicate that disciplinary segregation should undergo a critical evaluation by prison administrators, which could lead to the practice of disciplinary segregation being exercised in a more judicious and informed manner, thus limiting its potentially negative effects and contributing to positive social change.

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Dedication

This dissertation is dedicated to the Oregon Correctional Officers and the inmates they supervise.

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Chapter 1: Introduction to the Study

Introduction

Disciplinary segregation is a type of sanction employed by the Oregon Department of Corrections (Oregon DOC) within its prison facilities. Inmates who are officially found to have committed a prison rule violation may be removed from the general inmate population and confined in a separate cellblock known as the Disciplinary Segregation Unit (DSU; Or. Admin. R. 291-105-005 *et seq.*; Or. Admin. R. 291-011-0005 *et seq.*). Solitary confinement has a demonstrated potential for causing serious negative effects on inmates subjected to it (Arrigo and Bullock, 2008; Haney & Lynch, 1997; Haney, 2003; Pizarro & Stenius, 2004; Smith, 2006). The many similarities between disciplinary segregation and solitary confinement suggest that negative psychological and physiological effects may also accompany the experience of disciplinary segregation.

The purpose of disciplinary segregation, at least in part, is to deter inmates from engaging in prison misconduct (i.e., committing rule violations). However, it is currently unknown whether this form of punitive segregation is actually effective at changing the behavior of those inmates subjected to it. This study was designed to fill this gap in knowledge in order to better inform this potentially harmful disciplinary practice.

Background

From 2011–2014, an average of 14,311 individuals were incarcerated each month in the Oregon DOC system (Oregon DOC, n.d.a). Each month, some of these individuals were further isolated in disciplinary segregation units. However, there is a lack of

previous research investigating whether or not this segregation and isolation practice is effective at changing the behavior of those inmates and reducing their subsequent prison rule violation rates. This study was designed to address this research gap, and was guided by deterrence theory, which suggests that such a practice will successfully curb prison misbehavior.

Applying Bales and Piquero's (2012) rationale of incarceration to disciplinary segregation: "According to deterrence theory, [the experience of disciplinary segregation] should serve to remind offenders of the costs of punishment and should prevent them from engaging in further [offending] upon release" from disciplinary segregation (p. 72). Under deterrence theory, the goal of punishment is the prevention of future wrongdoing (Zimring & Hawkins, 1973, p. 75). This goal is pursued by increasing the costs associated with wrongdoing in order to outweigh the benefits associated with committing the offense (Nagin, 2013; Paternoster, 2010; Zimring & Hawkins, 1973). Deterrence theory assumes that most people who commit offenses are rational individuals that calculate and weigh the costs and benefits of a course of conduct prior to taking action (Nagin, 2013; Paternoster, 2010).

While faith remains strong in the overall deterrent effect of the criminal justice system (e.g., Cook, 1980; Doob & Webster, 2003; Nagin, 1998), the marginal deterrent benefits of specific programs and policies are often in doubt, and the results of the research testing the deterrence hypothesis remains mixed. For example, Lynch's (1999) evaluation of the incarceration rates and crime rates in the United States failed to support the deterrence hypothesis. However, such studies that attempt to examine deterrence

through the comparison of crime rates with incarceration rates typically suffer from serious methodological flaws, such as failing to take into account possible changing demographic factors that may mask any deterrent effect (Nagin, 1998; Nagin, 2013). In addition, several studies have found a negative correlation between incarceration rates and crime rates (i.e., when incarceration rates increase, crime rates decrease) (Nagin, 2013).

A review of the studies examining custodial versus noncustodial sanctions suggests that incarceration and imprisonment may have a criminogenic effect rather than a deterrent effect, although the findings only weakly support that hypothesis (Nagin, Cullen, & Jonson, 2009). 20% to 30% of the crime drop that occurred during the 1990s may be attributable to the increased use of incarceration, although this drop in the crime rate could have been a mixture of both deterrence and incapacitation. Moreover, it is unclear whether prison solitary confinement reduces inmate violence (Briggs, Sundt, & Castellano, 2003) or reduces recidivism (Mears & Bales, 2009).

Problem Statement

Disciplinary segregation is a practice utilized by the Oregon DOC where inmates are removed from the general inmate population and placed into lockdown within a special cellblock. Ostensibly, its purpose is to modify prison behavior by deterring inmates from committing prison rule violations. Deterrence theory suggests that practices such as disciplinary segregation should be effective (Paternoster, 2010; Nagin, 2013; Zimring & Hawkins, 1973). However, it was not known prior to this study

whether the experience of disciplinary segregation in Oregon prisons was actually effective in modifying behavior.

There is limited extant literature on disciplinary segregation in prisons, especially concerning the use of disciplinary segregation by the Oregon DOC. There is much research, however, on the topic of solitary confinement, which is very similar to disciplinary segregation. Some of the characteristics that these two forms of prison segregation have in common are:

- the prisoners are isolated from the general prison population in a separate cellblock,
- held within their cells for 22-24 hours each day and only permitted one hour of exercise,
- placed in restraints when removed from their cells,
- housed in cells that are continuously lit all day and night by artificial light, with no prisoner control over how brightly their cells are lit, and
- their exposure to physical and social stimulation is severely limited (Arrigo & Bullock, 2008; Briggs, Sundt, & Castellano, 2003; J. Duncan, personal communication, March 13, 2014; Haney & Lynch, 1997; Haney, 2003; Lippke, 2004; Pizarro & Stenius, 2004; Smith, 2006).

Previous research has linked the experience of solitary confinement with serious physiological and psychological negative effects. Some of those negative consequences include suicidal ideation, lethargy, rage, hallucinations, panic, cognitive dysfunction, emotional breakdowns, aggression, anxiety, insomnia, paranoia, depression, “increases in

negative attitudes and affect," self-mutilation, hypersensitivity, withdrawal, hopelessness, and "loss of control" (Haney & Lynch, 1997, p. 530). Since disciplinary segregation and solitary confinement are closely related practices, these research findings suggest that subjecting inmates to disciplinary segregation might place them at risk of psychological and physiological harm. This creates a clear need to review disciplinary segregation with a critical eye, and determine whether or not the subjective experience of disciplinary segregation deters prisoners from engaging in subsequent prison misbehavior.

On a general level, there is limited available information regarding the effectiveness of prison segregation (including solitary confinement) on deterring misbehavior. Extant research on prison segregation and deterrence does not provide strong or conclusive evidence that prison segregation is or is not effective at deterring misbehavior. On a more specific level, there is very little published research concerning *disciplinary* segregation, especially as it is specifically used by the Oregon DOC. Arrigo and Bullock (2008) noted this "absence of studies focused specifically on short-term segregation for disciplinary and/or punitive purposes" (p. 638). This gap in the literature includes a lack of direct research on whether disciplinary segregation is effective in deterring misbehavior, which was the object of this study.

Research on solitary confinement suggests that disciplinary segregation may have harmful effects on those subjected to it. Therefore, it is important to determine whether its benefits outweigh its costs and the risks of harm (Zimring & Hawkins, 1973, pp. 56–62). However, before attempting to calculate the benefits versus the costs, a fundamental

investigation was necessary to determine whether the practice of disciplinary segregation is effective at deterring inmates from subsequent prison misconduct.

Purpose of the Study

A critical evaluation of disciplinary segregation includes asking whether the experience of disciplinary segregation deters those subjected to it from subsequently engaging in prison misconduct. From an empirical perspective, it was unknown whether the experience of disciplinary segregation is an effective deterrent within the Oregon prison system. This dissertation study was designed to address this gap in knowledge.

The purpose of this quantitative study was to examine the effectiveness of disciplinary segregation in deterring prison inmate misconduct within the Oregon DOC prison system. Specifically, the purpose of this retrospective observational study was to test the theory of deterrence that relates disciplinary segregation to prison misconduct. The participants were drawn from the Oregon DOC prison inmate population.

Research Questions and Hypotheses

The purpose of this study was to examine whether disciplinary segregation is effective in deterring subsequent prison misbehavior among those inmates subjected to it, especially with regard to inmates who spent time in disciplinary segregation from 2011–2012 within the Oregon DOC system. Determining whether or not disciplinary segregation had an effect on subsequent prison misconduct was intended to reveal whether or not it is an effective deterrent. This study was also designed to determine if disciplinary segregation had a criminogenic effect, rather than a deterrent effect, as this had not previously been investigated. These observations led to the development of a

central research question (RQ1), and two sub-questions (RQ2, RQ3). Associated with this line of inquiry are a central null hypothesis and a central alternative research hypothesis (NH1, RH1), along with two secondary null hypotheses and two secondary alternative research hypotheses (NH2, NH3 and RH2, RH3).

RQ1: Does deterrence theory explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among inmates subjected to it, after controlling for the effects of gender, age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation in 2011-2012?

RQ2: Does the experience of disciplinary segregation reduce subsequent prison misconduct among inmates who are subjected to it, and if so, to what extent?

RQ3: Does the experience of disciplinary segregation have a criminogenic effect on inmates who are subjected to it? In other words, does the experience of disciplinary segregation lead to increases in prison misconduct?

NH1: There is no significant difference between the treatment cohort and the comparison cohort on prison misconduct.

RH1: There is a difference between the treatment cohort and the comparison cohort on prison misconduct.

NH2: The experience of disciplinary segregation does not significantly reduce subsequent prison misconduct among inmates who are subjected to it.

RH2: The experience of disciplinary segregation reduces subsequent prison misconduct among inmates who are subjected to it.

NH3: The experience of disciplinary segregation does not have a significant criminogenic effect on inmates who are subjected to it. In other words, the experience of disciplinary segregation does not lead to significant increases in prison misconduct.

RH3: The experience of disciplinary segregation has a criminogenic effect on inmates who are subjected to it. In other words, the experience of disciplinary segregation leads to increases in prison misconduct.

Theoretical Framework for the Study

The theoretical framework for this study was deterrence theory. The roots of deterrence theory can be traced back to Cesare Beccaria (1764) and Jeremy Bentham (1789). Since then, it has been further refined and modified by numerous scholars, including Becker (1968) and Zimring and Hawkins (1973). Deterrence theory is founded upon the assumption that those who commit crimes do so making a rational calculated choice (Nagin, 2013; Paternoster, 2010). According to this theory, an individual will refrain from engaging in criminal behavior if the perceived benefits are outweighed by the perceived costs, and vice versa (Nagin, 2013; Paternoster, 2010; Zimring & Hawkins, 1973). Deterrence occurs when the threat of unpleasant consequences for certain behavior causes individuals to refrain from engaging in that behavior (Zimring & Hawkins, 1973, p. 71).

Deterrence theory holds that the prevention of crime is influenced by the severity, certainty, and swiftness of punishment (Paternoster, 2010). Therefore, holding all other

variables constant, increases in the certainty, severity, and swiftness of punishment should lead to increased crime prevention (Paternoster, 2010). Deterrence theory can be further categorized into two different types: (1) general deterrence, and (2) specific deterrence (Nagin, 2013; Paternoster, 2010). General deterrence refers to whether the threat of punishment deters crime, whereas specific deterrence refers to whether the experience of being subjected to punishment deters an individual from subsequent future offending (Nagin, 2013; Paternoster, 2010).

Essentially, deterrence theory postulates that sanctions affect behavior, and that increasing the severity, swiftness, or certainty of sanctions will reduce misconduct or prevent individuals from engaging in misbehavior. Disciplinary segregation operates as a sanction for prison misconduct. In addition, since the inmate is already incarcerated for committing a crime or crimes, and disciplinary segregation is a more intense form of incarceration (it can be viewed as a prison-within-the-prison), disciplinary segregation therefore also functions as an increase in the severity of the punishment the inmate is already experiencing.

Typically, deterrence theory is focused on criminal offending. Although prisoners are not necessarily committing crimes, but rather are committing prison rule violations, deterrence theory is still applicable to the disciplinary segregation context, since the same basic principles – of individuals being threatened with and subjected to sanctions for misbehavior – are still present. Bentham (1789) structured deterrence theory in fairly broad terms, especially when he defined "offenses" as acts that "have a tendency to produce mischief" (p. 178, n. 1), and alternatively defined an "offense" as "an act

prohibited" (p. 309). Therefore, disciplinary segregation is a vehicle through which specific deterrence can be examined and tested. If disciplinary segregation has a specific deterrent effect on inmates, such a result would support deterrence theory. In particular, the findings of this study would have supported deterrence theory if the experience of disciplinary segregation significantly negatively predicted prison misconduct. However, there is some research that suggests that disciplinary segregation may have a criminogenic or null effect on those subjected to it, instead of a deterrent effect (e.g., Barak-Glantz, 1983; Briggs, Sundt, & Castellano, 2003; Mears & Bales, 2009; Motiuk & Blanchette, 2001).

Nature of the Study

This study employed a quantitative retrospective observational research design. For this study two cohorts were formed, a treatment cohort and a comparison cohort. Both cohorts were drawn from all of the inmates who were incarcerated from 2011 through 2014 within a facility (or facilities) with a medium-security component in the Oregon DOC system. The treatment cohort was comprised of all the inmates who spent any time in disciplinary segregation during the years 2011 through 2012. The comparison cohort was comprised of all the inmates who, as of January 1, 2013, had not spent any time in disciplinary segregation. Then the data were analyzed to see whether cohort membership (treatment cohort or comparison cohort) significantly predicted prison misconduct, controlling for the effects of certain extraneous predictor variables that are related to prison misconduct. The data that were utilized in this study were the Oregon DOC administrative and archival data on the study's inmate participants. The

data analysis was accomplished with the multiple regression statistical method, using the IBM Statistical Package for the Social Sciences (SPSS) computer software program. Results of the multiple regression analyses indicating that membership in the treatment cohort (i.e., those who experienced disciplinary segregation) significantly negatively predicted prison misconduct would have provided support for deterrence theory.

Control/predictor variables were included in the application of the multiple regression analyses in order to better isolate the effect of disciplinary segregation on prison misconduct. These control/predictor variables were (1) age, (2) length of time spent incarcerated on current sentence, (3) LS/CMI risk score, (4) prior major rule violations in 2011-2012, (5) prior minor rule violations in 2011-2012, and (6) length of time spent in disciplinary segregation in 2011-2012. *Gender* also served as a control variable, in that all the participants were male. Most of these control variables were chosen based upon research demonstrating that they are related to institutional offending.

As mentioned above, a quantitative retrospective observational research design was chosen for this study. Such a design appeared to fit best with the research purpose and the context in which this study was performed. The purpose of this study was to empirically evaluate whether the experience of disciplinary segregation is effective at deterring subsequent prison misconduct among those inmates subjected to it. Therefore, the aim of the study was to generate findings that could be inferred to the general Oregon prison population outside the specific sample developed for the study. A quantitative study enables such a generalization (as opposed to a qualitative study) (Creswell, 2009). Furthermore, a retrospective observational research design was chosen since random

selection was not a feasible option for this specific study, and since the study relied on archival administrative data.

Definitions

Disciplinary Segregation: The isolation and segregation of an inmate, for disciplinary purposes, from the main general inmate population and placement of that inmate within a separate cellblock (disciplinary segregation unit) as punishment for a prison rule violation.

Criminogenic: For the purposes of this study, the term "criminogenic," when used in the context of prison misconduct, should be understood to mean "tending to cause or foster misconduct." This is an adaptation of the traditional definition of criminogenic, "tending to cause or foster criminal behavior" (Oxford English Dictionary online, 2014). This is because not all prison misconduct necessarily constitutes "criminal behavior" in the strict sense, but prison misconduct and criminal behavior do appear to be related. For further discussion, see Camp and Gaes (2005, pp. 427–428).

Incapacitation Effect: This occurs when individuals who would have otherwise committed offenses (e.g., crimes) are prevented from doing so by subjecting them to incarceration (Donohue, 2009, p. 274; Paternoster, 2010, pp. 802–803; Raphael & Ludwig, 2003, p. 254).

Multi-security facility: A facility that operates different levels of security, such as one that operates with both a minimum-security component and a medium-security component. It is also worth mentioning that there is only one maximum-security facility within the Oregon State Corrections system, and it is housed within the Oregon State

Penitentiary (the Oregon State Penitentiary is a multi-security facility, with both a medium-security component and a maximum-security component) (Oregon DOC, n.d.b).

The independent/predictor variable *Cohort:* This was defined along two levels: (1) the treatment cohort, and (2) the comparison cohort. The *treatment cohort* was comprised of the inmates who spent time in disciplinary segregation during the years 2011 and/or 2012 within the Oregon DOC system. The *comparison cohort* was comprised of the inmates who, as of January 1, 2013, had not spent any time in disciplinary segregation.

The dependent/outcome variable *Prison Misconduct:* This was defined as an official finding of a rule violation(s) within the prison system. For this study, this dependent/outcome variable was measured in three different ways: (1) overall total rule violations in 2013-2014, (2) total major rule violations in 2013-2014, and (3) total minor rule violations in 2013-2014.

The control/predictor variable *Age:* The inmates' chronological (biological) age as of January 1, 2011.

The control/predictor variable *Length of Time Spent Incarcerated on Current Sentence:* The number of days, as of January 1, 2011, that the inmate had spent incarcerated on the sentence the inmate was serving during the specific time period of the study.

The control/predictor variable *LS/CMI Risk Score:* The inmate's total score on section 1 of the LS/CMI administered during the intake process.

The control/predictor variable *Prior Major Rule Violations in 2011-2012:* The total number of rule violations (i.e., official findings of a rule violation) that were classified at the "major" levels (i.e., rule violation levels 1-4) during the years 2011 and 2012.

The control/predictor variable *Prior Minor Rule Violations in 2011-2012:* The total number of rule violations (i.e., official findings of a rule violation) that were classified at the "minor" levels (i.e., rule violation levels 5-6) during the years 2011 and 2012.

The control/predictor variable *Length of Time Spent in Disciplinary Segregation in 2011-2012:* The number of days that the inmate had spent in disciplinary segregation in 2011-2012. In the data provided for this variable by the Oregon DOC, the parameters for this data also included days consecutively spent in disciplinary segregation where at least one of those days was within the treatment window (January 1, 2011 through December 31, 2012). The number of days were calculated as the date the participant moved out of disciplinary segregation (or December 31, 2012, whichever was earlier) minus the date the participant moved into disciplinary segregation; plus any additional days the participant spent in disciplinary segregation in 2011 or 2012.

The control variable *gender:* The inmate's sex as categorized by the Oregon DOC.

Assumptions

An important assumption contained within this study centers on the relationship between prison misconduct and official findings of prison rule violations. It was assumed

that official findings of prison rule violations accurately represent the commission of prison misconduct by the study participants during the timeframe of the study. However, there are concerns that official findings of prison rule violations may not accurately reflect actual prison misconduct (Light, 1990). For example, there is the chance that some rule violations go unnoticed or do not result in the inmate being officially found in violation of the prison rules. This would not be an issue if the relationship between the prison misconduct rate and the rate of official findings of prison rule violations was constant (Zimring & Hawkins, 1973, p. 327). Unfortunately, it cannot be determined with certainty the relation between the two rates, or whether the relation between the two rates is the same for both cohorts.

Using official findings of prison rule violations as a proxy for prison misconduct is similar to using arrest or conviction rates (i.e., crime rates) as a proxy for criminal behavior, which is a practice often used within the deterrence literature (e.g., Kovandzic & Vieraitis, 2006; Lynch, 1999; Marvell & Moody, 1994). In addition, other studies have also used official findings of prison rule violations as a measurement of prison misconduct (e.g., Bonta & Motiuk, 1992; Gaes et al., 2002; Kroner & Mills, 2001). Furthermore, Camp, Gaes, Langan, and Saylor (2003) observed that due to "the increased surveillance of inmates in prison" (as opposed to individuals in the community), it should be expected that the "data on prison misconduct [reflects] more accurately the universe of prison behavior than arrest or conviction data do for street crimes" (p. 505). In addition, measuring rule violations through the use of administrative data may be more reliable than other methods, such as conducting a survey, as inmates may not accurately identify

and self-report their past rule violations. Also, ethical issues would accompany any attempt to administer a survey to prison inmates, since prisoners are a "vulnerable" population (Creswell, 2009, p. 89). For all of these reasons, it appears that using official findings of prison rule violations was the most appropriate measure of prison misconduct for this specific study.

Scope and Delimitations

A quantitative retrospective observational study was employed to evaluate the effect of the experience of disciplinary segregation on prison misconduct among inmates within the Oregon DOC prison system during the years 2013 and 2014. The study analyzed two groups, a treatment cohort that had spent time in disciplinary segregation during the years 2011 or 2012, and a comparison cohort that as of January 1, 2013, had not spent any time in disciplinary segregation. The scope of the study was limited to only those male inmates who were incarcerated in Oregon adult prison facilities with a medium-security component during the years 2011 through 2014. Specifically, only inmates who were incarcerated within such facilities from January 1, 2011, through December 31, 2014, were included in the study. The data used in the study were limited to archival data and official records held by the Oregon DOC. Due to differences in correctional practices and procedures, the generalizability of the study is limited to the Oregon correctional system, and particularly to the prison populations of those facilities with a medium-security component within that system.

There is a significant gap in the literature concerning whether the practice of disciplinary segregation is effective at deterring prison inmate misconduct. This study

addressed one aspect of that gap: whether the experience of disciplinary segregation deters inmates from subsequent prison misconduct. Due to its design, the results of the study do not address any possible general deterrent effect disciplinary segregation may have; that is, the study did not address the issue of whether or not the practice of disciplinary segregation deters prison misconduct regardless of whether the inmate has directly experienced it.

Limitations

A common problem among observational studies centers on potential pretreatment differences between the treatment cohort and the comparison cohort (Rosenbaum, 1989, p. 1024). The two cohorts may have had pretreatment differences that make them noncomparable groups, thus interfering with the ability to draw causal inferences from the results (Rosenbaum, 1989, p. 1024). In particular, the internal validity of the study may have been compromised by possible selection bias. Selection bias can occur when the participants of a study have a predisposition toward a certain outcome (Creswell, 2009, p. 163). Here, the inmates within the treatment cohort (i.e., those who spent time in disciplinary segregation in 2011 or 2012) may have been predisposed to committing rule violations to a greater degree than those inmates in the comparison cohort. Multiple regression analysis was used to analyze the data in order to reduce the chance that any potential selection bias influenced the results. The multiple regression analyses partialled out the effects of age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation

in 2011-2012, thereby helping to isolate the specific effect the experience of disciplinary segregation may have had on subsequent prison misconduct.

The use of multiple regression analysis limited the strength of the conclusions that could be drawn from the results. For this study, the strongest conclusion that could possibly have been drawn is that the experience of disciplinary segregation is (or is not) significantly predictive of prison misconduct when the variables gender, age, length of time incarcerated on current sentence, and LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation in 2011-2012 are controlled (Warner, 2013, p. 556). If the experience of disciplinary segregation had been found to significantly predict misconduct, such a result would not have been "proof of causality" (Warner, 2013, p. 556; Tabachnick & Fidell, 2014, p. 158). Rather, it would have been evidence suggesting "the possibility of causality" (Warner, 2013, p. 555). In other words, it would have been evidence that would have suggested that the experience of disciplinary segregation might have a causal relationship with prison misconduct.

This study was designed to isolate and evaluate the effect of disciplinary segregation on offending within the Oregon prison system. However, it is difficult to truly isolate the effect of deterrent measures, and variables that are unaccounted for could have influenced the results of the study. For the multiple regression analysis to deliver an accurate assessment of the predictive value of disciplinary segregation on prison misconduct, the model must include all the necessary control variables in order to isolate the effect on prison misconduct that is unique to disciplinary segregation (Warner, 2013,

pp. 547–610). Unfortunately, for this study it is unknown whether I have included all the necessary control variables in the analyses – "in general, we can never be sure that we have a correctly specified model" (Warner, 2013, p. 555). In essence, the results of the data analysis are limited to the specific sample, data, timeframe, and variables used in the study (Warner, 2013, p. 556). Therefore, the results of this study do not constitute definitive and conclusive proof for any particular hypothesis or theory, and should be interpreted cautiously. Although the study was designed to isolate and measure the deterrent effect of disciplinary segregation, there is no doubt that a great many variables can influence prison misconduct, and many of these variables may not have been adequately controlled by the study's design.

Significance

Decreased mental health and psychological functioning, along with negative physiological effects, have been associated with the experience of prison solitary confinement (Haney & Lynch, 1997; Haney, 2003; Pizarro & Stenius, 2004; Smith, 2006; Arrigo & Bullock, 2008). The similarities that are shared between solitary confinement and disciplinary segregation suggest that disciplinary segregation may place inmates at risk of psychological and physiological harm. This risk of harm might be decreased through the generation of more knowledge about disciplinary segregation. Establishing an understanding of the effectiveness of disciplinary segregation as a deterrent technique may better inform its application, and may also decrease the likelihood of unnecessarily putting inmates at risk of psychological and physiological harm. If the findings of the

study fail to support the premise that disciplinary segregation is an effective deterrent, the practice of disciplinary segregation may need to be reconsidered and critically evaluated.

For example, if the findings suggest that disciplinary segregation is not being successfully employed as a deterrence tool, such a result would support a policy shift away from the use of disciplinary segregation and towards an alternative correctional strategy or practice. Furthermore, such a finding could stimulate those involved in policymaking to move towards decreasing the use of disciplinary segregation, and/or utilizing it in a more focused and precise fashion. Such policy modifications would diminish the likelihood of placing a prisoner unnecessarily at risk of psychological and physiological harm due to being subjected to disciplinary segregation.

Summary

The segregation of prisoners may have negative psychological and physiological effects on those prisoners. Research has shown that solitary confinement can result in lethargy, rage, hallucinations, insomnia, depression, hypersensitivity, anxiety, and paranoia, among other negative consequences (Haney & Lynch, 1997, p. 530). The similarities between solitary confinement and the Oregon DOC practice of disciplinary segregation suggest that disciplinary segregation may also have negative psychological and physiological consequences. Inmates are subjected to the risk of these negative effects as discipline for prison rule violations. However, little is known about whether the practice of disciplinary segregation actually reduces prison inmate misconduct among those subjected to it. In fact, some research suggests that disciplinary segregation may have a criminogenic or null effect rather than a deterrent effect. Increasing the

knowledge about the behavioral effect disciplinary segregation may have on those subjected to it should help better inform the policies and practices regarding its use within the Oregon DOC system.

Chapter 2: Literature Review

Introduction

There is limited extant research evaluating the effect of disciplinary segregation on subsequent misconduct by prison inmates. This study was designed to address this gap in the literature, and to examine the effectiveness of disciplinary segregation in deterring prison inmate misconduct within the State of Oregon Department of Corrections (Oregon DOC) prison system. This retrospective observational study was specifically designed to test the theory of deterrence that relates disciplinary segregation to prison misconduct.

A review of the extant literature demonstrates the relevance of this issue. Research on deterrence theory has found moderate support for the deterrence hypothesis; but it can be difficult to isolate and detect the deterrent effect of specific policies and practices (Paternoster, 2010). Furthermore, some of the research has produced mixed results and is hampered by methodological flaws (see Nagin, 2013; Paternoster, 2010). With regard to prison segregation, the weight of the extant research indicates that it can have negative psychological and physiological effects on inmates, although these effects might only be associated with longer periods of isolation, and the research has focused primarily on solitary confinement. The prior research that has been performed on prison segregation in relation to deterrence is inconclusive thus far, so it is unclear whether prison segregation has a deterrent effect on prison inmate misconduct.

This literature review begins with an exploration of deterrence theory, beginning with its theoretical underpinnings. Next, after a general discussion of deterrence theory,

studies investigating deterrence theory are reviewed, followed by a survey of extant research on prison segregation, beginning with a discussion of the potential effects prison segregation can have on the inmates subjected to it. The next section explores the current research on segregation with regard to its deterrent effect. This is then followed by a review and discussion of the research on key variables that have been empirically shown to be related to prison misconduct, as well as a review of literature related to the research design.

Literature Search Strategy

The search for research literature related to deterrence, prison segregation, prison misconduct, the key variables involved in the study, and the research design was pursued primarily through two methods. First, I searched several Internet research databases using various combinations of search terms, including but not limited to: deterrence, prison, institution, misconduct, misbehavior, recidivism, offense, offender, inmate, segregation, isolation, disciplinary segregation, administrative segregation, punitive segregation, age, LS/CMI, gender, index offense, multiple regression, and observational studies. The databases that were used included Academic Search Complete, ProQuest Central, ProQuest Criminal Justice, LexisNexis Academic, PsychARTICLES, and ProQuest Dissertations and Theses Full Text. In addition, I used the search engine Google Scholar extensively. Second, scholarly articles that were discovered through the use of the above methods were then used to find more articles, which were then used to discover still other studies and reports. I specifically searched the text of the articles and their bibliographies for other possibly relevant studies and reports. In addition, academic

textbooks were referenced for information regarding statistical analysis techniques, methods, and processes such as multiple regression.

The literature search strategy that I employed cast an initially broad net to collect an array of different resources, and then became more specific as the research further clarified the issues relevant to the study. Some of the searches were conducted with no specified timeframe limitation, whereas other searches focused on literature that was published within the last five or six years in order to develop an understanding of the current state of the research. The types of literature that were discovered and utilized included peer-reviewed journal articles, government reports, and scholarly books.

Theoretical Foundation

There is a great amount of research concerning the individual topics of deterrence and isolation/solitary confinement. Deterrence theory traces its beginnings back to Beccaria's *On Crimes and Punishments* (1764), after which the theory has been tested, extrapolated upon, and refined. The literature on isolation and solitary confinement typically focuses on the effects of solitary confinement on prisoners, and to a lesser extent on the demographic characteristics of those inmates who spend time in solitary confinement. The solitary confinement literature in the United States has also focused on the legal implications of solitary confinement, such as its relationship to the Due Process Clause as well as the Eighth Amendment's prohibition against cruel and unusual punishment (e.g., Cockrell, 2013; Haney & Lynch, 1997; Walton, 1997), but those legal considerations are outside the scope of this current study.

Deterrence Theory

Philosophical underpinnings of deterrence theory. The seeds of deterrence theory can be found within the ideas of Thomas Hobbes (1588–1679). Within the philosophy of Hobbes, the rationale for the justification of punishment can be found within his concept of the social contract. Under Hobbes' social contract individuals relinquished all of their rights (except the right of self-preservation) to the sovereign (Hobbes, 1651, pp. 91–100, 111–115; Bagby, 2007, p. 36). The purpose of individuals entering into this contract was to protect their interests and personal security (Hobbes, 1651, p. 92). This contract (i.e., the agreement to relinquish their rights) occurred among the people with each other – the sovereign was not a party to the contract (Hobbes, 1651, p. 214; Bagby, 2007, pp. 59–60). In other words, the individuals were agreeing among themselves to give up their rights to the sovereign (Hobbes, 1651, pp. 91–100, 111–115, 214; Bagby, 2007, p. 59). Hence, the punishment meted out by the sovereign for transgressions is justified because: (1) the sovereign has the power to punish because "he that transferreth any Right, transferreth the Means of enjoying it, as farre as lyeth in his power" (Hobbes, 1651, p. 97), and (2) if that exercise of power happens to be directed at a specific individual, the sovereign is not violating the purpose of the contract (personal security) because he was not a party to the contract. For Hobbes, the sovereign's power to punish was technically unrestrained and, regardless of the cruelty of the punishment, the exercise of that punishment would not conflict with the rights of the individual being punished because the sovereign was not a party to the original social contract (Bagby, 2007). In the same vein, although individuals did not give up their right to self-

preservation, the sovereign could still take their life because the sovereign was not bound by the social contract; he was just the recipient of the powers relinquished by the social contract (Bagby, 2007).

For the most part, Beccaria (1764) essentially accepted and adopted Hobbes explication of the elements of the social contract. However, Beccaria departed from Hobbes' teachings in an important way. For Beccaria, the rationale and purpose of law and the justification of punishment lay in utilitarianism: the greatest good for the greatest number. Beccaria claimed that this principle derived directly from the social contract: "Each individual is indeed bound to society, but society is, in turn, bound to each individual by a contract which, of its very nature, places both parties under obligation. *This obligation . . . signifies* only *that it is in the interests of all that the pacts advantageous to the greatest number be observed* [emphasis added]" (p. 15, n. 1). Beccaria believed that every punishment, other than the death penalty, could be justified through this utilitarian principle (Beccaria, 1764; Shuster, 2010, p. 63).

However, Shuster (2010) argued, "Beccaria *mistakenly* [emphasis added] believed that the principle of the greatest happiness of the greatest number could be derived from the social contract" (p. 62). Shuster went on to state that, "Beccaria's argument rests on a non-sequitur. Contrary to his claim, it is *not* in the interest of *all* to observe agreements beneficial to the *greatest number*, and so the principle of the greatest happiness of the greatest number does not follow from the social contract" (p. 63). However, if Postema's (2006) interpretation of Bentham's utilitarianism also explains Beccaria's concept of the utilitarian principle, then Shuster's critique is misplaced. If Beccaria believed that the

greatest good was actually the "greatest good *held in common* [emphasis added]" by society (as proposed by Postema, 2006, with regard to Bentham), then the utilitarian principle and its justification for punishment could derive from the social contract. This is because it is in the interest of all to observe agreements that promote the greatest good, when that "greatest good" is defined as good that is *held in common*. For example, it is a good held in common by all men, including thieves, that in general those who steal are required to pay restitution. Although paying restitution in a specific situation may not be in the offender's specific personal interest, his interest in not paying the restitution is not one that is simultaneously held in common by all men.

This social contract can be seen as the font from which the sovereign obtains her authority to punish. But how should the sovereign punish and to what end? According to Hobbes, punishment should be forward-looking and aimed at encouraging obedience of the law (Hobbes, 1651, pp. 201–245; Shuster, 2010, p. 46). Hobbes defines punishment as an "evill" [sic] "inflicted" on someone who has broken the law, "to the end that the will of men may thereby the better be disposed to obedience" (Hobbes, 1651, p. 214). Given Hobbes' use of the term "evill" elsewhere, it is clear that he uses the term "evill" to denote something he considers bad that should be avoided (e.g., Hobbes, 1651, p. 231). Later on, the deterrence theorists Beccaria (1764) and Bentham (1789) also define punishment as an "evil" to be avoided (Beccaria, 1764, p. 43; Bentham, 1789, p. 171). Hobbes went on to explain that "revenge" is not the purpose of punishment (Hobbes, 1651, pp. 214–215). Rather, the purpose of punishment is to "dispos[e] of the Delinquent, or (by his example) other men, to obey the Lawes" [sic] (Hobbes, 1651, p.

215). In fact, Hobbes makes it clear that punishment should *only* be concerned with "some future good" (p. 219), and therefore retribution was not an acceptable purpose of punishment for Hobbes. Hobbes' forward-looking prescription for punishment could encompass both the concepts of deterrence and rehabilitation (Shuster, 2010), but without a doubt it is certainly aimed at crime prevention.

This idea that the primary and ultimate aim of punishment should be forward-looking (i.e., crime prevention) was further developed into deterrence theory, first by Beccaria (1764) and then by Bentham (1789). Hobbes' teachings are also connected to Bentham's rational choice theory. This theory holds that individuals, prior to acting, will weigh the costs and benefits of a course of action, and then choose to act when the benefits outweigh the costs (Bentham, 1789). The beginning of this idea can be found in Hobbes' seventh law of nature, in which he taught that the harm caused by the punishment must outweigh the benefit to be gained by the unlawful activity (Hobbes, 1651, p. 215).

In summary, the foundational basis of deterrence theory lies in the social contract. The idea of the social contract is that of people joining together and giving up some of their freedom in return for stability and protection. This idea, in effect, stands as a proxy for civil society; it is a way of explaining and defending the idea of a civil society. Furthermore, according to Beccaria, the principle of utilitarianism, "the greatest good for the greatest number of people," is also drawn from the social contract and is related to deterrence theory. In essence, deterrence theory is a methodology designed to fulfill the utilitarian principle and protect and promote civil society.

Deterrence theory. Cesare Beccaria began his foray into criminology from a utilitarian perspective, announcing that the aim of law should be the promotion of "the *greatest happiness shared by the greatest number*" (Beccaria, 1764, p. 8). As discussed above, these laws (and the utilitarian principle) derive from the social contract (Beccaria, 1764, pp. 10–13, 60–61). Beccaria argued that the "sole purpose" of punishment should be the prevention of future offending, that is, "to deter other men, by fear, from committing a similar crime" (Beccaria, 1764, p. 35). In that vein, he felt that punishment for the purposes of retribution or restitution was inappropriate.

> The purpose of punishment is neither to torture and afflict a sensitive being, nor to undo a crime already committed . . . . The purpose can only be to prevent the criminal from inflicting new injuries on its citizens and to deter others from similar acts. (Beccaria, 1764, p. 42)

Beccaria (1764) taught that punishment itself was "evil" (albeit a necessary evil), and therefore the evil inflicted by punishment should only be so great as to outweigh the benefits associated with committing the offense; anything above and beyond what is necessary for the prevention of offending "is superfluous and for that reason tyrannical" (p. 43). For example, he argued "punishments that exceed what is necessary for protection of the deposit of public security are by their very nature unjust" (Beccaria, 1764, p. 13). Beccaria recognized as well that "it is better to prevent crimes than to punish them" (p. 93). Furthermore, punishment should be proportionate to both the harm caused by the offense and the attractiveness of the offense; greater punishment for more harmful crimes, less punishment for less harmful crimes (Beccaria, 1764, pp. 62–63, 42,

47–48, 57). Beccaria also articulated the idea that punishment for crime is most effective at preventing future offending when it is certain, severe, and prompt.

Bentham (1789) expounded upon the work of Beccaria. Bentham (1789) began by adopting Beccaria's theory that the ultimate aim of government is to promote the greatest good for the greatest number of people (pp. 1, 70, 170). "The greatest happiness of all those whose interest is in question, as being the right and proper, and only right and proper and universally desirable, end of human action . . . and in particular in that of a functionary or set of functionaries exercising the powers of Government" (Bentham, 1789, p. 1, n.1). He calls this concept "the *principle of utility*" (Bentham, 1789, p. 1). It could be argued that under this principle the punishment of an innocent person, or the infliction of a punishment that is grossly disproportionate to the offense, would be acceptable as long as the good realized by the public outweighed the sacrificial cost suffered by the individual subjected to the punishment (Blumenson, 2012, p. 1542; see also Zimring & Hawkins, 1973, p. 35). Possible examples include imprisoning an innocent person or executing a pickpocket live on television. However, Bentham's theory of utilitarianism would not approve of such methods. Bentham's version of utilitarianism does not call for "heroic sacrifice" (Postema, 2010, p. 125). Rather, Bentham's utilitarian theory could be summarized as: The interests that are held *in common* by the people should be realized to the greatest extent possible (Postema, 2006, pp. 110–122).

Bentham (1789) argued that punishment is itself an evil that should only be pursued for the sake of deterring future wrongdoing.

> The general object which all laws have, or ought to have, in common, is . . . to exclude mischief. But all punishment is mischief: all punishment in itself is evil. Upon the principle of utility, if it ought at all to be admitted, it ought only to be admitted in as far as it promises to exclude some greater evil. (Bentham, 1789, p. 171)

Furthermore, Bentham specifically holds that punishment should never go beyond what is necessary for deterrence purposes (p. 182). In contrast to Beccaria, Bentham acknowledges that punishment may not just only serve the ends of deterrence but also retribution; however, just like Beccaria, Bentham makes it clear that "no punishment ought to be allotted merely [for the] purpose" of retribution (Bentham, 1789, p. 171, n. 1). In contrast, Immanuel Kant taught that retribution should be the primary aim of punishment (Kant, 1796; Shuster, 2010). For example, Kant proposed that a murderer should still be executed, for retributive purposes, even if a society is disbanding and its members will never see each other again (Kant, 1796, p. 198; see also Zimring & Hawkins, 1973, p. 16; Shuster, 2010). It is clear that both Beccaria and Bentham would disagree with Kant's proposition.

Bentham also goes into more detail on the difference between general deterrence and specific deterrence (Bentham, 1789, pp. 170–171; cf. Beccaria, 1764, pp. 42, 35). Bentham explains that the punishment of the offender works to prevent that individual from future offending through "*reformation*" and "*disablement*", whereas punishment of the offender works to prevent others from offending by "way of *example*" (Bentham, 1789, pp. 170–171, n. 1). Furthermore, for criminal justice policies to be successful,

Bentham clearly states that the costs associated with offending must outweigh the benefits associated with offending. "The value of punishment must not be less in any case than what is sufficient to outweigh that of the profit of the offence" [sic] (Bentham, 1789, pp. 179–180, citing Beccaria, 1764). Moreover, Bentham adds substance to Beccaria's assumption that individuals weigh the costs and benefits of their actions. In particular, he specifically argues that almost all individuals do in fact "calculate", at least to some degree, the potential pains and pleasure associated with their actions, going so far as to state: "I would not say, that even a madman does not calculate" (Bentham, 1789, pp. 187–188).

Of great importance is the fact that Bentham (1789) clarified and explored the proposition that people act in their own self-interest, in that people are motivated by pain and pleasure (i.e., costs and benefits), and operate with the goal of increasing their utility (i.e., benefits) (Paternoster, 2010, p. 770; e.g., Bentham, 1789, pp. 1, 31). This development served to transform Bentham's work into a theory of criminal behavior (Paternoster, 2010, pp. 770–772). This is in contrast to Beccaria's work, which resembled more of a set of guidelines and principles rather than a theory of criminal behavior (Paternoster, 2010, pp. 770–772).

Within Bentham's (1789) work can also be found the idea of perception, a principle which holds that it is an individual's perception of the certainty, severity, and swiftness of punishment that is important (pp. 33, 71, 183, 192), rather than the actual or objective certainty, severity, and swiftness inherent in the punishment. Although the

implication of the importance of perception can be found in Bentham's work, it was later developed more fully by other scholars (Paternoster, 2010, pp. 772, 780).

The economist Gary Becker (1968) contributed to the development of deterrence theory by drafting the first modern formal model of deterrence theory (Chalfin & McCrary, 2014, p. 3; Nagin, 2013, p. 207). Up until that point, research on criminal offending focused on pathology and psychology in order to understand why individuals committed crimes (Paternoster, 2010, pp. 772–773, 778). In contrast, Becker explained criminal offending through the concepts of utility and rational choice (Paternoster, 2010, p. 778). Becker argued that

> [A] person commits an offense if the expected utility to him exceeds the utility he could get by using his time and other resources at other activities. Some persons become "criminals," therefore, not because their basic motivation differs from that of other persons, but because their benefits and costs differ. (Becker, 1968, p. 176)

For example, Chalfin and McCrary (2014) provide the following Becker-based formula, in which an individual will choose to commit a crime when the following condition is present:

$$(1 - p)U_{c1} + U_{c2} > U_{nc}$$

(p. 3). Here, p represents the probability of being apprehended for committing the offense, U_{c1} represents the expected utility if the individual is caught and punished, U_{c2} represents the expected utility if the individual is not apprehended, and U_{nc} represents the

expected utility if the individual abstains from offending (Chalfin & McCrary, 2014, p. 3). These expected utilities can be manipulated by increasing the severity and/or certainty of apprehension and punishment, although they can also be influenced by sources outside of the criminal justice system (e.g., the economy) (Becker, 1968, p. 177; Chalfin & McCrary, 2014, pp. 3–4, 7). Becker also pointed out that the social costs of efforts at increased deterrence should be evaluated in light of the expected benefits (p. 180). This is because at times the increased deterrence may not be justifiable in economic terms with respect to its expected costs and benefits (Becker, 1968, p. 180).

Zimring and Hawkins (1973) embarked on a refinement of deterrence theory with a global analysis of its morality. They subsequently developed a set of "principles that need to be observed if deterrence is to be morally tolerable" (p. 50). They described these principles as follows:

> Three [principles] are of paramount importance: the retributive limit or the principle of just deserts, the principle that the offender's suffering should be regarded as a cost, and the principle that action in ignorance [of the efficacy of punishment] imposes a moral obligation to do research [on punishment]. Also of major importance, although infrequently at issue, is the fair notice principle in relation to offenders punished beyond the maximum authorized at the time they committed their offenses. Still important, but at a lower level, are the principle of equality and the principle of fair notice when unusual but authorized sentences are imposed. (Zimring & Hawkins, 1973, p. 50)

The first principle stems from the idea that a strict application of deterrence theory could result in unfair treatment ("purely reformatory or deterrent theories lack what are essential safeguards against inhumanity and the infringement of human rights") (Zimring & Hawkins, 1973, p. 35). In light of this issue, Zimring and Hawkins (1973) adopted what could be labeled as a more "modern" approach to deterrence theory, holding that the principle of retribution should set the "upper limit to the range within which penalties may be selected on utilitarian grounds" (Zimring & Hawkins, 1973, p. 35, 39). As touched upon earlier, Bentham would arguably have supported this version of deterrence theory (Postema, 2006), although perhaps for different ulterior reasons than for the sake of pure justice or fairness. This is because the imprisonment of an innocent individual or the grossly disproportionate punishment of an individual are not interests 'held *in common* by the people.' In particular, it would not be in the universal interest of the people in general to punish an innocent person or subject someone to a grossly disproportionate punishment, because, for example, it would reduce the public's trust in the criminal justice system, which would in turn lead to a reduction in society's respect for the rule of law and increase disobedience to the law. In other words, it could lead to a breakdown of civil society. Therefore, such unfair solutions would be outside the realm of Bentham's concept of utilitarianism and deterrence theory, as well as outside the realm of the version of deterrence theory espoused by Zimring and Hawkins (1973).

The second principle, that the suffering of the offender should be considered a cost, stems from the idea that the offender is a member of the public and her interests should be given due consideration (Zimring & Hawkins, 1973, pp. 42–43). Zimring and

Hawkins (1973) reference both Kant and Bentham as supporting this concept (p. 42). Bentham, as noted by Zimring and Hawkins, even specifically stated that the offender still remains "a member of the community," and "that there is just as much reason for consulting [the offender's] interest as that of any other. His welfare is proportionately the welfare of the community – his suffering the suffering of the community" (Zimring & Hawkins, 1973, p. 42, citing and quoting Bentham, 1843, p. 398).

Shifting to the concepts of specific deterrence and general deterrence, Zimring and Hawkins (1973) argued that such a taxonomy produces a false dichotomy (pp. 72–73, 224–226). They argued that the actual punishment of individuals is not a different type of deterrence but rather a "special effort to make individuals more sensitive to general deterrence" (p. 73). However, regardless of whether or not specific deterrence and general deterrence are truly separate concepts, I believe that the term *specific deterrence* can be useful in delineating between the effect of punishment on the individual person subjected to it, in contrast to the *general deterren*t effect on society of threats and the punishment of an individual member.

The thought process of evaluating the costs associated with offending in light of the benefits associated with offending can be thought of as "simple deterrence" (Zimring & Hawkins, 1973, pp. 75–75). Zimring and Hawkins (1973) argued that the basic psychology underlying "the notion of simple deterrence subscribed to by the classical theorists" (e.g., Bentham and Beccaria) "has long been considered inadequate" (p. 96). Zimring and Hawkins recognized the complex psychology that can be involved in criminal behavior decision-making (p. 89), and therefore attempted to expand deterrence

theory beyond just "simple deterrence" by including within the deterrence theory framework consideration of all of the effects of punishment and the criminal justice system on offending (p. 77). Beyond the simple calculus of a cost-benefit analysis, they included the effect of threats and punishments on preventing offending through their ability to: (1) provide an additional incentive to those tempted to offend, (2) increase respect for the law, (3) help establish among members of society the "habit" of following the law, and (4) provide a source of moral education for society by teaching right and wrong (Zimring & Hawkins, 1973, pp. 77–89).

Principles of deterrence theory. There are five primary principles of deterrence theory that are particularly relevant to this study. First, the purpose of punishment is the prevention of future misconduct. The purpose of punishment is not retribution or making the victim whole. Second, punishment should be limited to only that which is necessary to fulfill its purpose of preventing future wrongdoing. Moreover, modern deterrence theory holds in addition that the *just desert* associated with the offense should act as the upper limit to the punishment administered. Third, people act in their own self-interest, seeking to increase their utility or benefits. In addition, people act rationally, and weigh the costs and benefits of their actions. When the perceived costs of an offense outweigh its perceived benefits, then the typical individual will refrain from committing the offense. Fourth, sanctions and the threat of sanctions can alter behavior. Fifth, the certainty, severity, and swiftness of punishment can influence whether individuals engage in misconduct (especially by altering their cost-benefit calculus).

Incarceration and crime. There are many studies concerned with the relationship between incarceration and crime, and the following are particularly illustrative of this line of research. First, Lynch (1999) investigated whether imprisonment deterred crime. Lynch analyzed the trends in U.S. imprisonment rates in relation to crime rate trends. Lynch found that from 1972 to 1993, the rate of incarceration continuously increased each year (a 265% increase), while the crime rate also increased (a 32% increase). Lynch conducted statistical analyses on the crime rate and incarceration data, and was unable to reveal results supporting the hypothesis that imprisonment deters crime. Similarly, Kovandzic and Vieraitis (2006) studied the relationship between the imprisonment rate in Florida and Florida's crime rate for the years 1980 to 2000. Florida was an ideal choice because of the fact that its crime rate and imprisonment rate were very similar to the national crime rate and imprisonment rate (Kovandzic & Vieraitis, 2006, p. 220). Similar to Lynch's conclusions, Kovandzic and Vieraitis indicated there was "little or no significant relationship" between Florida's crime rate and Florida's incarceration rate (Kovandzic & Vieraitis, 2006, p. 227). Likewise, DeFina and Arvanites (2002) also did not find a statistically significant relationship between imprisonment and crime (p. 649).

Levitt (1996) did find that incarceration might reduce crime, although the possible deterrent effect of prison is not delineated from prison's incapacitation effect. Levitt studied the results of prison overcrowding litigation, in order to avoid the problem of *simultaneity* confounding the analysis. Simultaneity refers to the fact that incarceration and the crime rate "can mutually affect each other" (Paternoster, 2010, p. 800). Studies

that attempt to determine the effect of incarceration on the crime rate often fail to account for the effect the crime rate has on the incarceration rate, since rising crime can lead to rising incarceration (Nagin, 2013, p. 221). Levitt's (1996) analysis found that "incarcerating one additional prisoner reduces the number of crimes by approximately fifteen per year" (p. 348). This is in keeping with the estimates of others such as Marvell and Moody (1994) (see also Donohue, 2009). Marvell and Moody analyzed the annual crime rate data from several studies and the Uniform Crime Reports, and found that the data suggested that the imprisonment of each additional offender prevented 16–25 crimes (pp. 118, 136). Levitt (2004) concluded that about one-third of the decrease in the crime rate that occurred from 1990–2000 could be attributed to the increased use of incarceration (pp. 178–179).

Donohue (2009) reviewed six studies that found a statistically significant negative relationship between the incarceration rate and the crime rate after analyzing aggregate crime rates in relation to incarceration rates (included in those six studies were Marvel and Moody and Levitt (1996)). However, Durlauf and Nagin (2011) argued that those six "studies are not informative about the presence (or absence) of a causal mechanism that links imprisonment policy to crime" (p. 50). Durlauf and Nagin (2011) also issued a detailed critique directly calling into question the findings of the Marvell and Moody (1994) study, focusing on the design of the study and the interpretation of the results.

Research that attempts to study the relationship between incarceration and the crime rate is susceptible to several methodological flaws. For example, although Lynch's (1999) examination provides a useful overall analysis of crime and imprisonment rates

and their interaction, it fails to take into account or control for other variables that may be influencing the crime rate. In other words, it is possible that the deterrent effect of incarceration is being masked by countervailing forces that are causing the crime rate to go up in spite of prison's deterrent effect. For example, perhaps gang culture, the changing demographics of the population (e.g., an increased percentage of young males in the population) (Nagin, 1998), a reduction in economic opportunities, or other factors may be fueling the increase in crime and overwhelming the deterrent effect of imprisonment. Therefore, Lynch's conclusion and the study's results are informative, but should be viewed with caution.

Paternoster (2010) argued that the research of Levitt (1996, 2004) and others concerning incarceration vis-à-vis the crime rate should be regarded with caution. First, Paternoster observed that "there is a general consensus that the decline in crime is, at least in part, due to more and longer prison sentences, with most of the controversy being over how much an effect imprisonment had" (p. 801). This shows that Paternoster recognized that the research does indicate that increasing the rate of incarceration can, "to some degree," have an inverse effect on the crime rate (p. 802). However, Paternoster noted that it is not clear to what extent deterrence is responsible for the lowering of the crime rate and to what degree incapacitation is responsible for the lowering of the crime rate (pp. 802–803). Furthermore, Canada also experienced a decline in the crime rate during the same time period as the U.S., but Canada's crime drop was accompanied by a decline in incarceration (Paternoster, 2010, p. 803). Moreover, attributing the U.S. crime drop of the 1990s to increased incarceration is at odds with the trend of the 1980s, where

increased incarceration was accompanied by an increasing crime rate (Paternoster, 2010, p. 803; Blumstein, 2006, p. 31). Levitt (2004) argued that this inconsistency was partly caused by the fact that rising youth crime during that time ("due in part to the crack epidemic") masked the "steadily falling" adult crime rate (p. 179, n. 7; see also Blumstein, 2006). Among the other potential causes for the drop in the crime rate could have been changing police practices or changing demographics (Levitt, 2004, p. 163).

Nagin (2013) also adds his own critique of the incarceration rate/crime rate literature. Just like Paternoster (2010), Nagin noted that such studies are of limited use with respect to deterrence research, since, among other flaws, the deterrent effect of incarceration is often not separated from the incapacitation effect (p. 220). Nagin (2013) also highlighted the fact that the incarceration rate is not directly related to deterrence, and the incarceration rate/crime rate literature fails to analyze the relationship between the incarceration rate and the principles of deterrence (p. 225; see also Durlauf & Nagin, 2011, p. 53). Hence, the studies fail to link the variable *incarceration rate* with the deterrence hypothesis.

Nagin, Cullen, and Jonson (2009) analyzed the relationship between incarceration and crime from a different angle than other researchers such as Kovandzic and Vieraitis (2006), Marvell and Moody (1994), Levitt (1996, 2004), and Lynch (1999). Rather than focusing on imprisonment and the crime rate, they instead examined the effectiveness of incarceration at preventing reoffending by reviewing studies that compared the effects of custodial sanctions versus noncustodial sanctions (Nagin, Cullen, & Jonson, 2009). The authors found that the experimental and quasi-experimental studies indicated that

incarceration had a criminogenic effect (as opposed to a deterrent effect), although the evidence was weak and there were only six studies in this category (Nagin, Cullen, & Jonson, 2009). The eleven matching studies provided stronger evidence of a criminogenic effect, although the effect overall was not statistically significant (Nagin, Cullen, & Jonson, 2009). The overall results of the regression studies also indicated that incarceration had a criminogenic effect (there were 22 studies in this category) (Nagin, Cullen, & Jonson, 2009). Nagin, Cullen, and Jonson (2009) concluded that the effect of incarceration on reoffending is uncertain; in their words, "as imprisonment is used in contemporary democratic societies, the scientific jury is still out on its effect on reoffending" (p. 178). From these findings it can be inferred that disciplinary segregation may possibly have a criminogenic effect, as opposed to a deterrent effect. In addition, these findings suggest that disciplinary segregation may increase prison misconduct instead of deter it.

Bales and Piquero (2012), after adopting the framework set out by Nagin, Cullen, and Jonson (2009), performed a study comparing the effects of custodial sanctions versus noncustodial sanctions within the Florida correctional system. In order to compare the two types of sanctions, the researchers analyzed the recidivism data of two cohorts: those who were sentenced to prison (the treatment cohort), versus those who were assigned to a prison diversion program (the comparison cohort) (Bales & Piquero, 2012, pp. 73, 75). Other studies investigating the effects of custodial sanctions versus noncustodial sanctions have used various research design methods in order to account for possible selection bias (Bales & Piquero, 2012). In order to examine whether those different

research methods produced different results, Bales and Piquero analyzed their data using three separate methods: (1) precision/exact matching, (2) binary logistic regression, and (3) propensity score matching (Bales & Piquero, 2012). Each of the three design methods revealed that those who were sentenced to prison were significantly more likely to reoffend than those who were sentenced to the community prison-diversion program (Bales & Piquero, 2012, pp. 88, 91, 95). Although the three research design methods each provided slightly different results, it was unclear which method was "superior", since "they are all different ways of getting at the issue" (Bales & Piquero, 2012, p. 98).

Sentence severity. According to classical deterrence theory, the severity of the punishment can influence its deterrent effect. In other words, the deterrence hypothesis holds that increasing the severity of a punishment should prevent some individuals from committing an offense. So far, the research on the deterrent effect of the severity of punishment has returned mixed results and only weakly supports this prong of deterrence theory.

A study by Weisburd, Einat, and Kowalski (2008) showed that the threat of incarceration had a noticeable effect on probationers paying their court-ordered financial obligations. The researchers studied a group of probationers in New Jersey within a system where probationers were previously "seldom" threatened with a violation of probation solely for noncompliance with their financial obligations (Weisburd, Einat, & Kowalski, 2008, pp. 14, 17). The experiment compared three groups of probationers who were delinquent on their court-ordered financial obligations: (1) those who were kept on just regular probation (i.e., the control group), (2) those who were threatened with being

served a violation of a probation, and who were also given community service and placed on intensive probation, (3) those who were only threatened with being served a violation of probation (Weisburd, Einat, & Kowalski, 2008). The threat of being served with a probation violation carried with it the possibility of being incarcerated as punishment for the probation violation, and therefore served essentially as a threat of possible incarceration (Weisburd, Einat, & Kowalski, 2008, pp. 11, 28). The group that was threatened with possible incarceration and was assigned community service and intensive supervision "were significantly more likely to pay their [court-ordered] financial obligations" than those assigned to the regular probation group (Weisburd, Einat, & Kowalski, 2008, p. 12). However, the group that was only threatened with incarceration had similar positive results (Weisburd, Einat, & Kowalski, 2008, p. 12). These findings indicated that it was the threat of possible incarceration that had the primary effect of encouraging the compliance of the probationers. These results therefore suggest that the severity of punishment (possible incarceration versus regular probation processes) may indeed successfully influence behavior within the criminal justice system. This is because it appears that the more severe sanction had the primary effect; in that the primary effect seemed to derive from the possibility of incarceration rather than from the community service sanction or the intensive supervision (it should be noted that intensive supervision can be seen as both a sanction and as a rehabilitative measure) (Weisburd, Einat, & Kowalski, 2008).

Helland and Tabarrok (2007) analyzed the effect of California's three-strikes legislation. The three-strikes legislation required the imprisonment of those convicted of

a ‘third strike’ to be sentenced to 25 years to life, and mandated that they would not be eligible for release until they had spent at least 20 years in prison (Helland & Tabarrok, 2007, pp. 309–310). A violent or serious felony was required to constitute the ‘first strike’ (Helland & Tabarrok, 2007, p. 309). A conviction for a subsequent felony qualified as the second strike, and another subsequent felony conviction qualified as the third strike (Helland & Tabarrok, 2007, p. 309). The sentence for the third strike conviction was drastically more severe than the sentence mandated for a second strike. The second strike conviction resulted in a doubling of the sentence, 80% of which must be served prior to release (Helland & Tabarrok, 2007, p. 309). On average, individuals served sentences of about 43 months in prison for a second strike conviction; as opposed to the mandatory 240 months prison minimum to be served upon a third strike conviction (Helland & Tabarrok, 2007, p. 319). Helland and Tabarrok compared the arrest histories of two cohorts: (1) those inmates who had been released from prison with two strikeable convictions, and (2) those inmates who had undergone some form of adjudication (e.g., trial or an entry of a guilty plea) twice in which a strikeable offense was involved, but who only had one strikeable offense conviction (e.g., at one of the adjudications, the defendant pled guilty to a nonstrikeable offense and the strikeable offense was dismissed pursuant to the plea negotiations) (Helland & Tabarrok, 2007, pp. 310, 312). The analysis performed by Helland and Tabarrok revealed that the California three-strikes legislation resulted in the deterrence of about 31,000 crimes per year (p. 327). Furthermore, the researchers found that the increase in the severity of the third-strike sentence “significantly reduces felony arrests among the class of criminals with two

strikes by 17 to 20 percent" (Helland & Tabarrok, 2007, p. 328). These results support the deterrence theory hypothesis that the severity of punishment can function as a tool for preventing crime.

However, Helland and Tabarrok's (2007) generation of the comparison cohort is susceptible to criticism. They claimed that their generation of the comparison cohort was based upon the "fortuitous randomization of trial" (Helland & Tabarrok, 2007, p. 312). Granted, there is doubtless some randomness involved, but it should also be noted that the judges and prosecutors involved in the cases might have been taking into account their own professional opinion as to whether the defendant would recidivate. Their judgment of the chances of the defendant recidivating likely influenced whether the defendant ended up being convicted of a strikeable offense. The reality of the sentence associated with a second strike conviction and a third strike conviction could very well have had a large impact on the judges' and prosecutors' decisions with regard to the resolution of many of the cases in which the defendant was facing a possible conviction for a second or third strikeable offense. The judges and prosecutors may have purposely moved many of those individuals whom they thought had less of a chance to recidivate out of the second-strike position and instead resolved the case with a conviction for a nonstrikeable offense. This means that the comparison group might have been naturally less inclined to offend than the group comprised of those individuals with two strikeable offenses.

Raphael and Ludwig (2003) also examined the effectiveness of sentence enhancements with their study that focused on gun crimes. Richmond, Virginia, in

cooperation with the Federal government, instituted "Project Exile," which effectively increased the penalty for the illegal possession of guns (Raphael & Ludwig, 2003). The researchers measured the effectiveness of the program by looking at the homicide rate of Richmond and comparing it with other cities (Raphael & Ludwig, 2003). They also compared the adult arrest rates with juvenile arrest rates, both for Richmond and for other cities (Raphael & Ludwig, 2003). The juvenile arrest rates served as a comparison group, since the sentence enhancement policy of Project Exile was generally not applicable to them (Raphael & Ludwig, 2003). Using regression analysis, the researchers determined that the sentence severity enhancement policy of Project Exile exhibited no discernable deterrent effect (Raphael & Ludwig, 2003, p. 274).

Drago, Galbiati, and Vertova (2009) took advantage of a unique Italian law to study the effects of sentence severity on recidivism. The Italian parliament enacted the Collective Clemency Bill in July of 2006, which suspended the last three years of inmates' sentences if those inmates met certain criteria (Drago, Galbiati, & Vertova, 2009, pp. 258, 265). The Bill only applied to those inmates whose sentences were based on crimes committed prior to May 2, 2006 (Drago, Galbiati, & Vertova, 2009, p. 258). The law provided that those inmates who re-offended within five years of their release would have to serve the remainder of their sentence that had been suspended, in addition to the sentence given for their new crime (Drago, Galbiati, & Vertova, 2009, p. 258). So, an inmate who committed a new crime and whose sentence had previously been shortened by one month due to the Clemency law would have to serve the sentence for the new crime plus an additional month in prison (Drago, Galbiati, & Vertova, 2009, p.

258). Likewise, an individual who had received two years or three years suspended would have to serve two or three years (respectively) of additional time in prison if they were convicted and sentenced for a new crime within five years following their initial release (Drago, Galbiati, & Vertova, 2009, p. 258). To varying degrees, the suspension of the sentence effectively increased the potential severity of punishment awaiting the inmate upon the commission of a new crime. The researchers studied the data on the inmates who had been released between August 1, 2006, and February 28, 2007 (seven months) (Drago, Galbiati, & Vertova, 2009, p. 259). Their analysis of the recidivism data showed that a suspension of one month of prison "significantly reduces their propensity to recommit a crime" (Drago, Galbiati, & Vertova, 2009, p. 259). Therefore, their results indicated that increasing the expected punishment by one month of incarceration decreases the probability that individuals will commit a crime (Drago, Galbiati, & Vertova, 2009, p. 278).

Nagin and Pogarsky (2001) studied the deterrent effect of the severity of punishment through the lens of individuals' *perceptions* of severity, certainty, and swiftness of punishment. They administered a survey to college students that was based on a potential drunk-driving scenario (Nagin & Pogarsky, 2001, pp. 874–875). The students were asked to estimate their chances for getting caught, in order to measure the effect of the "certainty" of punishment (Nagin & Pogarsky, 2001, p. 875). Then the students were randomly assigned a length of suspension of driving privileges, ranging from 3 months to 15 months, in order to measure the effects of the "severity" of punishment (Nagin & Pogarsky, 2001, p. 875). In order to measure the effects of the

"celerity" of punishment, the students were randomly assigned a time when the suspension would take effect (in 6 months to 18 months) (Nagin & Pogarsky, 2001, p. 875). Then, the students were asked how likely they would have driven home (which measured their likelihood of offending) (Nagin & Pogarsky, 2001, p. 875). Nagin and Pogarsky found that both the certainty and severity of punishment had a statistically significant negative effect on the students' likelihood of offending, but their results did not support the idea that the swiftness of punishment has a preventative impact on offending (pp. 877–879). Further statistical analysis revealed that the results provided far stronger support for the certainty of punishment than they did for the severity of punishment (pp. 883–884).

Doob and Webster (2003) examined whether "harsher sentences deter" crime (p. 146). Starting out, the authors set out four critiques of the studies that suggest that harsher sentences do function as a deterrent to criminal behavior (Doob & Webster, 2003). First, they argued that such studies that utilize crime rates suffer from the difficulty of isolating the deterrent effect from other variables (e.g., incapacitation) (Doob & Webster, 2003, pp. 156–157). Secondly, they argued that such studies that utilize as a measure of deterrence 'decreases in incarceration rates' are flawed, since a decrease in incarceration rates could be due to other variables (e.g., decreases in conviction rates) (Doob & Webster, 2003, p. 160). Third, they argued that the results of perceptual deterrence surveys are not necessarily generalizable to the effects of actual criminal justice policies (Doob & Webster, 2003, p. 163). Fourth, they argued that some of the studies utilize questionable data selection processes (Doob & Webster, 2003, p. 164).

Next, Doob and Webster (2003) highlighted studies examining the deterrent effects of harsher sentences that produced inconclusive results. Then, the authors outlined the studies that supported their "null hypothesis" that harsher sentences do not result in a significant deterrent effect. These studies that reported no significant deterrent effects, coupled with some perceptual deterrence studies, provided support for the authors' premise (Doob & Webster, 2003). The perceptual deterrence studies highlighted by Doob and Webster (2003) primarily consisted of studies that interviewed inmates after they had committed crimes, and examined their thought-process associated with their criminal behavior (Doob & Webster, 2003). These studies revealed a lack of cost-benefit analysis on the part of the criminals prior to the commission of their crimes (Doob & Webster, 2003, p. 183). Because there is no "conclusive evidence" supporting the argument that harsher sentences deter crime, Doob and Webster argued that therefore the premise that harsher sentences *do not* deter crime should be adopted (Doob & Webster, 2003, pp. 187–191).

Statistical findings of modern deterrence research. Dölling, Entorf, Hermann, and Rupp (2009) performed a meta-analysis on the existing deterrence research. This meta-analysis was based on 700 deterrence studies (Dölling, Entorf, Hermann, and Rupp, 2009, p. 203). The authors converted the different effect estimates within the various studies to *t*-values, in order to enhance the comparability of the studies (Dölling et al., 2009, p. 203). For 50.5% of the studies, the deterrent effect estimate was not significant (Dölling et al., 2009, p. 204). However, 41.7% of the deterrent effect estimates were significant and supported the deterrence theory hypothesis, while 7.8% of the deterrent

effect estimates did not support the deterrent hypothesis but were also significant (Dölling et al., 2009, p. 204). Overall, these results suggest that there is "moderate" support for the deterrent hypothesis (Dölling et al., 2009, p. 204). The studies indicated that the certainty of punishment has a greater effect than the severity of punishment (Dölling et al., 2009, pp. 210, 216, 222). It should also be noted that the methodological rigor of the studies varied a great deal (Dölling et al., 2009, p. 208).

A further careful analysis of the studies involved in the meta-analysis creates a more nuanced picture of the research. Dölling et al. (2009) found that studies that utilize criminal statistics generally support deterrence theory, whereas studies that focus on capital punishment generally reject the deterrence hypothesis (p. 206). Within the studies utilizing criminal statistics, the deterrent hypothesis was more frequently supported with regard to property crimes than it was with regard to violent crimes (Dölling et al., 2009, p. 210). Overall, the authors discovered that deterrent effects are more pervasive for property and administrative-type offenses, as opposed to serious and violent crimes (Dölling et al., 2009, pp. 201, 215, 223). Similar to the criminal statistics studies, the survey-based studies showed that "the size of the deterrent effect is at least partly dependent on the seriousness of the offense" (Dölling et al., 2009, p. 219).

Review and analysis of deterrence theory and research. Paternoster (2010) evaluated the deterrent research concerning (1) policing and deterrence, (2) imprisonment and deterrence, (3) the objective realities of punishment and the subjective perception of punishment, and (4) the relationship between the subjective perception of punishment and crime. Paternoster found the research concerning the deterrent effect of policing

inconclusive (p. 795). Paternoster noted that the results suggest that policing policies (such as increasing the number of police) can exert a deterrent effect to some degree, but the size of the deterrent effect is unclear (p. 795). This is especially due to the lack of control over possible intervening variables and an inability to strictly isolate the effect of the policing strategy under investigation within the research studies (Paternoster, 2010, p. 795). The research reviewed by Paternoster regarding the objective reality of punishment and the subjective perception of punishment suggested that people's perceptions of the punishment associated with crime are not related or only weakly related to the actual real punishment associated with crime, but that people do update their perceptions based on experience (pp. 804–810). The literature examining the relationship between the subjective perception of punishment and crime suggests that the perceived certainty of punishment has a "modest" deterrent effect (Paternoster, 2010, p. 817). However, evidence is lacking to show a deterrent effect produced by the perceived severity or celerity of punishment (Paternoster, 2010, p. 817).

Nagin (1998, 2013) also evaluated the present state of deterrence research. Since Nagin's 1998 conclusions are similar to his more recent 2013 findings, I will just focus on his most recent review. Nagin (2013) first reviewed the deterrence literature up to the 1990s. Nagin divided this literature into (1) experimental and quasi-experimental studies, (2) aggregate studies, and (3) perceptual deterrence studies (p. 213). With regard to the experimental and quasi-experimental studies, Nagin surmised that this literature suggests that the certainty of punishment is a more important deterrent factor than the severity of punishment (Nagin, 2013, p. 214–216). Furthermore, the deterrent effects of

interventions tend to fade as time goes by (Nagin, 2013, pp. 214–216). The aggregate studies generally took the form of comparing and correlating, within and across jurisdictions, the (1) crime rates, with (2) the reported crimes, with (3) the imprisonment ratios, with (4) the mean time served (Nagin, 2013, p. 216). Although a significant negative correlation was typically "found between the crime rate and the certainty of imprisonment ratio", the studies suffered from serious methodological flaws (Nagin, 2013, p. 216). Nagin also briefly discussed the literature concerning "the deterrent effect of capital punishment," but made the determination that the findings from this area of research are inconclusive (Nagin, 2013, pp. 217–220). The post-1990s aggregate studies, although generally methodologically different from the pre-1990s aggregate studies (the post-1990s studies were generally longitudinal studies, as opposed to cross-section studies), again typically found a negative correlation between crime rates and imprisonment rates (Nagin, 2013, p. 220). However, just like the pre-1990s studies, these post-1990s studies suffer from serious methodological flaws (Nagin, 2013).

Nagin (2013) analyzed both the pre- and post-1990s perceptual deterrence literature together. These studies consistently found a negative association between the perceived sanction certainty and risk of offending, but the results regarding the perceived severity of punishment were less consistent (Nagin, 2013, p. 244). These studies have also found that informal sanctions (as opposed to formal sanctions, for example, arrest and imprisonment), such as loss of respect in the community, can play an important role in criminal deterrence (Nagin, 2013, p. 244). It has also been found that the perception of

potential formal sanction severity seems to be more accurate among those who have a stronger association with criminal activity (Nagin, 2013, p. 250).

Nagin (2013) identified two important policy implications for his research on deterrence. First, Nagin noted that mandatory minimum sentences cannot be justified by deterrence theory (although incapacitation theory or retribution may justify them) (pp. 199, 252–253). Second, the effectiveness of the criminal justice system may be improved by changing the focus from "corrections" to instead increasing the perceived risk of apprehension through policing methods (Nagin, 2013, p. 253).

The deterrent effect of the criminal justice system. Overall, it should also be noted that despite the lukewarm evidentiary support for the efficacy of deterrence theory found among the studies evaluating specific criminal justice tactics, practices, and policies, researchers often still retain their overall confidence in the general deterrent effect of the criminal justice system. Cook (1980) stated that his "assessment is that the criminal justice system, ineffective as it may seem in many areas, has an overall crime deterrent effect of great magnitude" (Cook, 1980, p. 213, cited by Nagin, 1998, p. 2). Nagin (1998) reached a similar determination: "my review leads me to conclude that the evidence for a substantial deterrent is much firmer than it was fifteen years ago. I now concur with Cook's more emphatic conclusion that the collective actions of the criminal justice system exert a very substantial deterrent effect" (p. 3). Even Doob and Webster (2003), although critical of the idea that harsher sentences deter crime, nevertheless included the caveat: "we agree with Nagin (1998), who concludes that substantial evidence exists that the overall system deters crime" (p. 144). Paternoster (2010) noted

that the challenge lies not in determining whether the criminal justice system overall has a deterrent effect, but in determining whether and to what extent "relative or marginal deterrent effects" exist (p. 766).

Deterrence theory and the present study. Deterrence theory holds that sanctions can affect behavior, and increasing the severity, swiftness, or certainty of sanctions can reduce and prevent misconduct. Since disciplinary segregation operates as a sanction for prison misconduct, then the experience of disciplinary segregation should alter and reduce the offender's subsequent behavior. Hypothetically, after experiencing disciplinary segregation the offender's perception of the threat of punishment for prison rule violations would be updated in that the threat would seem more realistic and applicable to his own personal situation, and the offender would subsequently modify his behavior to avoid the threatened punishment. As mentioned previously, "according to deterrence theory, [the experience of disciplinary segregation] should serve to remind offenders of the costs of punishment and should prevent them from engaging in further [offending] upon release" from disciplinary segregation (Bales & Piquero, 2012, p. 72) (to be clear, Bales and Piquero were referring to incarceration, and I have altered their statement to instead refer to disciplinary segregation). As noted above, typically deterrence theory is focused on criminal offending. Although prisoners are not necessarily committing crimes, but rather are committing prison rule violations, deterrence theory is still applicable to the disciplinary segregation context, since the same basic principles are still present (individuals are being threatened with, and subjected to, sanctions for misbehavior). Bentham (1789) himself defined deterrence in very broad

terms (referring to "offenses" as acts that "have a tendency to produce mischief" (p. 178, n. 1), and simply as acts that are prohibited (p. 309)). Evaluating the effect of disciplinary segregation on prison misconduct is an approach that can be used to test the theory of specific deterrence. Evidence that disciplinary segregation reduces subsequent prison misbehavior would support the deterrence hypothesis. Specifically with regard to this study, the results will support deterrence theory if the experience of disciplinary segregation is a significant negative predictor of subsequent prison misconduct.

This study's three core research questions were directly related to deterrence theory:

RQ1: Does deterrence theory explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among inmates subjected to it, after controlling for the effects of gender, age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation in 2011-2012?

RQ2: Does the experience of disciplinary segregation reduce subsequent prison misconduct among inmates who are subjected to it, and if so, to what extent?

RQ3: Does the experience of disciplinary segregation have a criminogenic effect on inmates who are subjected to it?

The primary research question, RQ1, applied the theory of deterrence to a specific situation and asked if deterrence theory was a viable explanation in that context, given the circumstances. The secondary research questions, RQ2 and RQ3, asked if the

experience of disciplinary segregation had an effect on subsequent inmate conduct, and if so in what direction (increase in misconduct or decrease in misconduct?). These secondary research questions served the purpose of defining how deterrence theory will be tested and measured in this study. RQ2 asked whether the experience of disciplinary segregation had a deterrent effect on subsequent prison misconduct, that is, whether deterrence theory is a viable theory in this context. RQ3 asked whether the experience of disciplinary segregation is *not* a viable theory, in that if the answer to RQ3 was "yes," then deterrence theory would not explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct.

Alternative Theories of Criminal Behavior

Although this study was designed to specifically test deterrence theory, there are other theories of criminal behavior that may explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct. Alternatively, another theory or a combination of theories may explain the results of the data analysis – without necessarily explaining the relationship, if any, between disciplinary segregation and prison misconduct. Such other possible theories of criminal behavior apart from deterrence theory include social learning theory, self-control theory, social-control theory, strain theories, and biosocial theories.

Social learning theory posits that criminal behavior is something that is learned (Akers & Jennings, 2009; Brauer & Bolen, 2015). Social learning theory assumes that people are not naturally inclined either toward law-abiding behavior or criminal behavior; rather, what matters is the balance of the competing influences of conformity versus

criminality (Schreck & Hirschi, 2009, p. 309). It is the direction in which that scale tips that leads to whether or not a criminal act is committed by an individual (Schreck & Hirschi, 2009, p. 309). Social learning theory, as conceived by Akers (e.g., 1985, 1998) encompasses four central concepts/variables (Akers & Jennings, 2009, p. 325). First, when a preponderance of an individual's associates (e.g., family and friends) display attitudes and behaviors that favor criminal behavior, then the likelihood that the individual will then herself engage in criminal behavior increases (i.e., "differential association") (Akers & Jennings, 2009, pp. 325, 328). The second concept is that of "differential reinforcement," which holds that the rewards and punishments (for criminal and conforming behavior) that an individual has experienced in the past and anticipates in the future affect the likelihood of criminal behavior (Akers & Jennings, 2009, pp. 326–327; Vaske, 2015, p. 126). Third, individuals may model their criminal or conforming behavior in imitation of the behavior of others that they have observed (Akers & Jennings, 2009, pp. 327). Lastly, social learning theory recognizes that an individual's attitudes and beliefs towards certain behaviors will also influence the likelihood that the individual will engage in criminal behavior (Akers & Jennings, 2009, p. 326).

Self-control theory, as conceptualized by Gottfredson and Hirschi (1990), holds that low self-control is the underlying cause of deviant/criminal behavior (Pratt & Cooper, 2009, p. 289; Burt, 2015). Self-control theory was built on the foundations of classical theories of crime (i.e., people act in their own self-interest), and positivism (different individuals have different propensities to commit criminal acts) (Pratt & Cooper, 2009, p. 290). According to self-control theory as originally conceived, an

individual's level of self-control is for the most part fully formed by ages 8-10, and then subsequently stable throughout the rest of a person's life (Burt, 2015, pp. 149–150; Pratt & Cooper, 2009, pp. 289–290). In addition, the default nature is for people to have low self-control; self-control is something that must be developed as a child (Burt, 2015, pp. 148–149; Pratt & Cooper, 2009, pp. 289–290). Self-control can be defined as "the basic capacity to regulate one's thoughts, emotions, and behaviors in the face of external demands" (DeLisi, 2015, p. 172). Self-control theory is especially concerned with the "time perspective" individuals incorporate into their cost-benefit calculation when considering criminal activity (Burt, 2015, p. 147). Individuals who consider the long-term consequences of their actions will be less likely to commit crime and exercise higher self-control, whereas individuals with a more short-term perspective will be more likely to commit crime and exhibit low self-control (Burt, 2015, p. 147). There is empirical support for the contention that low self-control can effectively predict criminal behavior, but research also suggests that it is not the *only* underlying cause of criminal behavior (Pratt & Cooper, 2009; Burt, 2015).

Unlike self-control theory, social control theory focuses on the social bond between an individual and society (Schroeder, 2015). The overarching principle of social control theory maintains that the likelihood of deviant/criminal behavior increases when the strength of an individual's bond to society decreases (Boisvert, 2015, p. 236; Schreck & Hirschi, 2009, p. 305). Social control theory assumes individuals act in their own self-interest, and criminal behavior can therefore be natural (Schreck & Hirschi, 2009, pp. 307, 309). This is in contrast to social learning theory, which assumes that criminal

behavior is learned, rather than naturally occurring (Schreck & Hirschi, 2009, p. 309). Furthermore, unlike deterrence theory, social control theory holds that the criminal justice system and formal legal sanctions do not affect the behavior of potential offenders (Schreck & Hirschi, 2009, p. 311). Schreck and Hirschi (2009) explained: "The criminal justice system receives people after they have committed offenses, but it has little or no influence on their prior or subsequent behavior" (p. 311).

In social control theory, there are four elements that make up the individual's "bond" to society: (1) attachment, (2) commitment, (3) involvement, and (4) belief (Schreck & Hirschi, 2009, p. 305; Schroeder, 2015, pp. 223–224). "Attachment" refers to one's level of sensitivity to the opinions other people might have of her (Schreck & Hirschi, 2009, p. 305). The greater an individual cares about what others might think of her, then the less likely it is that the individual will engage in criminal behavior (Schreck & Hirschi, 2009, pp. 305–306). This is because there is a general consensus among society that criminal behavior (e.g., "theft, robbery, murder") is wrong and should not be engaged in (Schreck & Hirschi, 2009, p. 307). "Commitment" refers to one's stake in conformity (Schreck & Hirschi, 2009, p. 306). As Schreck and Hirschi (2009) explained, "the more he or she has to lose, the greater the costs of the crime and the less likely it is to be committed" (p. 306). "Involvement" refers to the level of an individual's involvement in noncriminal activities (Schreck & Hirschi, 2009, p. 306). Individuals with higher levels of involvement in conformist activities should be less likely to engage in criminal behavior (Schreck & Hirschi, 2009, p. 306). This is in part because the time taken up by their conformist activities should reduce their opportunities to engage in

criminal behavior (Schreck & Hirschi, 2009, p. 306). "Beliefs" refers to the idea that personal beliefs are *not* the direct causes of criminal behavior, in that people are not engaging in criminal behavior in order to "liv[e] up to their beliefs" (Schreck & Hirschi, 2009, p. 307).

Strain theories are based upon the idea "that certain strains or stressors increase the likelihood of crime" (Agnew, 2009, p. 332; see also Morris, Carriaga, Diamond, Piquero, & Piquero, 2012). Not all stressors are "conducive to crime," and certain stressors are more conducive to crime than others (Agnew, 2009, p. 333; Agnew, 2015). Strain theories assume that individuals are naturally inclined to engage in conformist behavior, but that certain stressors push individuals into committing criminal acts (Schreck & Hirschi, 2009, p. 309). One specific strain theory, Agnew's (2006) General Strain Theory (GST), highlights several specific types of strain that are conducive to crime, including parental rejection, harsh/excessive/unfair discipline, child abuse and neglect, unemployment, discrimination, homelessness, and failure to achieve certain goals (Agnew, 2009, p. 333; Agnew, 2015, pp. 189–190). GST also attempts to answer why certain types of stressors increase the likelihood of an individual engaging in criminal behavior (Agnew, 2009, p. 334). For example, certain stressors may increase the likelihood of criminality by decreasing one's bond with society (re: social control theory), such as the stressor of parental rejection (Agnew, 2009, p. 334). Stressors can lead to negative emotions such as anger (which could then lead to a desire for revenge), or frustration, fear, or depression (which could then lead to illicit drug use in an attempt to feel better) (Agnew, 2009, p. 334). In addition, certain stressors can lead individuals to

associate with people who foster and/or reinforce pro-criminal attitudes (re: social learning theory) (Agnew, 2009, p. 334).

There is also the biosocial perspective of criminal behavior (Crews, 2009, p. 198). The biosocial perspective attempts to integrate sociological theories with biological theories (Crews, 2009, p. 198). The biosocial perspective holds that there are "biological risk factors" that may predispose individuals to criminal or noncriminal behavior; but at the same time it recognizes that the individual's environment and social atmosphere also play a key role in whether the individual engages criminal behavior (Boisvert, 2015, p. 237; Crews, 2009, pp. 195, 198). An individual's biological predisposition toward criminality could be due to certain physical traits, genes, hormones, neurotransmitters, and types of brain structural characteristics (Crews, 2009).

Segregation

A number of researchers have undertaken the task of evaluating the evidence as to whether or not solitary confinement causes negative psychological and/or physiological harm to those subjected to it. Some of the research that has been produced evaluating the effects of solitary confinement includes Haney and Lynch (1997), Haney (2003), Pizarro & Stenius (2004), Smith (2006), and Arrigo and Bullock (2008). Zinger, Wichmann, and Andrews (2001) also evaluated the psychological effects of prisoner segregation, but specifically studied administrative segregation in Canada. Miller (1994) and Miller and Young (1997) specifically examined whether the experience of disciplinary segregation increased levels of distress among inmates. Lippke (2004) provides an informative

scholarly argument against the use supermaximum confinement, which is a type of prisoner segregation practice.

The effects of solitary confinement on prisoners subjected to it. Haney and Lynch (1997) reviewed the extant literature on solitary confinement, which included research on sensory deprivation, social isolation, seclusion, torture, direct studies on solitary confinement, and studies that evaluated the secondary effects of solitary confinement. Their review of the research revealed that solitary confinement can have many detrimental effects on inmates. Haney and Lynch (1997) summarized their comprehensive review in the following manner:

> Direct studies of the effects of prison isolation have documented a wide range of harmful psychological effects, including increases in negative attitudes and affect, insomnia, anxiety, panic, withdrawal, hypersensitivity, ruminations, cognitive dysfunction, hallucinations, loss of control, aggression, rage, paranoia, hopelessness, lethargy, depression, emotional breakdowns, self-mutilation, and suicidal impulses. Among the correlation studies of the relationship between housing type and various incident reports, self-mutilation is prevalent in isolated housing, as is deterioration of mental and physical health, other-directed violence, such as stabbings, attacks on staff, and property destruction, and collective violence. (Haney & Lynch, 1997, p. 530)

In addition, the authors added that "there is not a single study of solitary confinement wherein non-voluntary confinement that lasted for longer than 10 days failed to result in negative psychological effects" (Haney & Lynch, 1997, p. 531). Six years later, Haney

(2003) reprised this review of the literature and arrived at similar, although stronger and more forceful, conclusions. Haney (2003) described the negative psychological effects as being “unequivocally documented” and “extremely well documented”, and the evidence supporting the findings relating to its negative effects as “robust” (p. 131) and “overwhelming” (p. 149).

Haney (2003) also compared and contrasted the prevalence rates of psychological indices and “symptoms of psychological trauma” in prisoners at the Pelican Bay supermax penitentiary in California against non-incarcerated individuals. Over 50% of the supermax prisoners experienced most of the “psychological effects” and symptoms of psychological trauma that were studied (e.g., lethargy, heart palpitations, headaches, irrational anger, chronic depression), as opposed to the 20% prevalence rate among nonincarcerated individuals (Haney, 2003, pp. 135–136).

Pizzaro and Stenius (2004) analyzed the research on the effects of supermax prisons on inmates (supermax prisons are a type of solitary confinement). Their examination revealed that much of the research is flawed or methodologically weak (Pizzaro & Stenius, 2004). Similar to Haney and Lynch (1997) and Haney (2003), Pizarro and Stenius (2004) also found that the evidence shows that solitary confinement likely causes psychological harm to inmates, but their conclusion is much more guarded. They stated that overall “the research suggests that solitary confinement has a detrimental impact on individuals’ mental health, although the extent and specific nature of this impact are unclear” (Pizzaro & Stenius, 2004, p. 257).

Smith (2006) also reviewed the relevant research related to solitary confinement. Smith discovered that the sensory deprivation and perceptual deprivation studies (primarily performed during the 1950s and 1960s) generally found that "isolating people and severely restricting sensory stimulation", even for short periods of time ("hours or days"), can produce negative effects, such as "hallucinations, confusion, lethargy, anxiety, panic, time distortions, impaired memory, and psychotic behavior" (pp. 470–471). Smith further observed that the "vast majority" of modern research on solitary confinement in prisons "report substantial health effects" (p. 471), and described the current state of solitary confinement research as "a massive body of data documenting serious adverse health effects" (p. 475).

Unlike Haney (2003), who stated that he had not found a single solitary confinement study "that failed to result in negative psychological effects" (p. 132), Smith (2006) did note that there are two solitary confinement studies, but only two, that have not "conclude[d] that there were substantial negative health effects" related to solitary confinement (p. 472). These two studies are Suedfeld et al., 1982, and Zinger and Wichmann (1999). Haney and Lynch (1997) and Haney (2003) also cited the Suedfeld et al. (1982) study, but not the Zinger and Wichmann (1999) study. However, the evidentiary support offered by these two studies for the premise that solitary confinement may not result in negative harm to inmates is questionable. Zinger and Wichmann (1999) had a very high attrition rate, with only 10 prisoners completing the entire full length of the study in nonvoluntary administrative segregation (Smith, 2006, p. 472). Suedfeld et al. (1982) did in fact report inmates as suffering from negative health effects, but did not

assign such effects as directly attributable to solitary confinement (Smith, 2006, p. 472–473), which appears why Haney and Lynch (1997) did not classify the Suedfeld et al. (1982) study as research that 'failed to result in negative psychological effects' (Haney & Lynch, 1997, p. 520).

Although the conditions of solitary confinement can differ to a great degree, there are "a similar range of symptoms" that are suffered by "a significant percentage" of inmates that are placed in solitary confinement (Smith, 2006, p. 488). These symptoms include physiological negative effects, such as "severe headaches," "heart palpitations," "oversensitivity to stimuli," "pains in the abdomen," as well as pain in the back, neck, and chest, "weight loss," and digestion problems (Smith, 2006, pp. 488–490). Other physiological and mental health effects include memory loss, decreased ability to concentrate, confusion, hallucinations, hearing voices, depression, anxiety, lethargy, and self-mutilation (Smith, 2006, pp. 490–493). These symptoms often recede upon the termination of solitary confinement, although some inmates continue to experience negative effects after being released from solitary confinement (Smith, 2006, 498).

Arrigo and Bullock (2008) also reviewed the evidence concerning the psychological effects of solitary confinement on inmates, and arrived at a conclusion similar to the ones drawn by Haney (2003), Haney and Lynch (1997), and Smith (2006): that the weight of the evidence shows that solitary confinement does have "damaging psychological consequences" for inmates (Arrigo & Bullock, 2008, p. 627). However, Arrigo and Bullock (2008) only apply this conclusion to "long term" solitary confinement, and note that it has not been "conclusively" demonstrated that short-term

solitary confinement is also accompanied by negative effects (Arrigo & Bullock, 2008, p. 627). This is in part due to the lack of research on short-term disciplinary or punitive segregation. Arrigo and Bullock explained:

> Much of the isolation research examining the psychological consequences of short-term segregation emphasizes administrative confinement only. The absence of studies focused specifically on short-term segregation for disciplinary and/or punitive purposes represents a serious deficiency in the literature and a significant limitation to the present inquiry. (p. 638)

In addition, studies purporting to show that solitary confinement is *not* harmful are "not representative of the conditions of secure isolation in U.S. correctional facilities" (Arrigo & Bullock, 2008, pp. 631–632).

The effects of administrative segregation on prisoners subjected to it. Zinger, Wichmann, and Andrews (2001) investigated whether the experience of Canadian administrative segregation had a negative effect on inmates' psychological functioning. Some of the participants had been placed voluntarily in administrative segregation, while some had been placed involuntarily in administrative segregation (Zinger, Wichmann, & Andrews, 2001, p. 60). The comparison group consisted of inmates drawn from the general inmate population (Zinger, Wichmann, & Andrews, 2001, p. 60). The inmates participated in an initial examination that consisted of a psychological assessment and a structured interview (Zinger, Wichmann, & Andrews, 2001, 60). After the initial evaluations, this process was repeated 30 days later, and then again 60 days later (Zinger, Wichmann, & Andrews, 2001, p. 60). Overall, the results revealed that the segregated

inmates suffered from "poorer mental health and psychological functioning" (Zinger, Wichmann, & Andrews, 2001, p. 48). However, the study also revealed that their mental health and psychological functioning did not "significantly deteriorate" during the time they spent in administrative segregation (Zinger, Wichmann, & Andrews, 2001, p. 48). Out of the 83 segregated inmates and 53 nonsegregated inmates that comprised the original two cohorts, full complete 60-day data were only gathered on 23 segregated inmates and 37 nonsegregated inmates, due to attrition (Zinger, Wichmann, & Andrews, 2001, p. 63). The authors noted that their study might not be generalizable to the United States prison population, due in part to the fact that U.S. prison conditions may be harsher than Canadian prison conditions (Zinger, Wichmann, & Andrews, 2001, p. 73).

The study by Zinger et al. (2001) is not without its critics. Jackson (2001) pointed out that there are "important quantitative problems with the research methodology" (p. 112). These include the small sample size and the lack of differentiation between those prisoners that voluntarily underwent segregation from those prisoners that were involuntarily subjected to segregation (Jackson, 2001, p. 112). Furthermore, Jackson argued that the Zinger et al. study failed to take into account the possible long-term effects of segregation (p. 112). In addition, Jackson pointed out that some of the possible effects of segregation are elusive to capture without spending a serious amount of time with the inmate(s).

The effect of disciplinary segregation on levels of distress. Miller (1994) and Miller and Young (1997) examined the effect prison segregation had on inmates' level of distress. The authors studied 30 inmates from a medium-security federal correctional

facility in Kentucky (Miller, 1994, p. 44; Miller & Young, 1997, p. 87–88). The participants were drawn from three different housing categories and matched according to age and race: 10 from the general prison population, 10 from administrative segregation, and 10 from disciplinary segregation (Miller, 1994, p. 44; Miller & Young, 1997, p. 88). The disciplinary segregation inmates were housed in single-person cells, whereas the inmates in administrative segregation often had a cellmate (Miller, 1994, p. 43). For the period under study, the average length of time spent in administrative segregation was 62 days, whereas the average length of stay in disciplinary segregation was 57 days (average length of stay in disciplinary segregation for the month prior to the period under study was 26 days) (Miller, 1994, p. 43).

Miller (1994) found that increasing the level of restriction among inmates resulted in increasing levels of distress (i.e., disciplinary segregation inmates experienced higher levels of general distress than administrative segregation inmates) (pp. 46–48). Miller and Young (1997) found that when compared with inmates from the general population, the disciplinary segregation inmates reported significantly higher interpersonal sensitivity (e.g., "more feelings of personal inadequacy, inferiority and marked discomfort during interpersonal interactions"), significantly higher hostility (e.g., "more thoughts, feelings or actions of anger"), and significantly higher obsessive-compulsive symptoms (e.g., "unremitting and irresistible impulses") (Miller & Young, 1997, pp. 89–90). Furthermore, when compared to the administrative segregation inmates, the inmates housed in disciplinary segregation reported significantly higher hostility (Miller & Young, 1997, p. 90). The findings of Miller and Young (1997) are in line with the

previous findings of Miller (1994). The findings of these two studies suggest that disciplinary segregation may increase the level of distress felt by those inmates subjected to it, and therefore negatively impact their psychological functioning.

Scholarly arguments against supermax. Lippke (2004) advocated against the use of supermax confinement. Lippke pointed out that one of the purported purposes of supermax incarceration is to reduce crime, either through incapacitation, or deterrence, or both (Lippke, 2004, p. 112). However, supermax inmates "seem like poor candidates for deterrence", since the threat of maximum-security incarceration did not previously prevent them from engaging in criminal behavior (Lippke, 2004, p. 112). In addition, the evidence supporting the idea that increasing the severity of sanctions results in a deterrent effect is weak (Lippke, 2004, p. 112). Moreover, the prevalence of mental illness and low mental functioning further reduces the possible deterrent effect of solitary confinement. Many inmates suffer from mental illness or "are of substandard or low intelligence" (Lippke, 2004, p. 119). Smith (2006) mentioned that "up to 20% of all prisoners [may be] 'seriously mentally ill' " (p. 453), and the rate is even higher for "disruptive prisoners" (p. 454). Such mentally ill inmates and those with low intelligence often act irrationally and are therefore not good candidates for deterrence (Lippke, 2004, p. 119). Lippke argued that the costs associated with supermax confinement (both the monetary costs and the negative impacts it has on inmates) outweigh any marginal benefits that may be achieved through its use.

Segregation and Deterrence

Barak-Glantz (1983) performed one of the first modern studies directly investigating solitary confinement. Data were gathered on prisoners from the Washington State Penitentiary (Walla Walla, Washington) who spent time in solitary confinement, delineated into four groups categorized by year (1966, 1971, 1973, and 1975), which was compared with data gathered on prisoners who had been discharged from the Washington State Penitentiary during those same four years (Barak-Glantz, 1983, pp. 30–31). Barak-Glantz found that solitary confinement had only a "minimal" deterrent effect on inmates (p. 36). Unfortunately, Barak-Glantz does not clearly explain the process or basis for reaching that conclusion.

Briggs, Sundt, and Castellano (2003) tested whether supermax prisons reduced "inmate-on-inmate violence", and whether supermax prisons reduced "inmate-on-staff violence" (p. 1350). Using a time series design coupled with a regression analysis (Briggs et al., 2003, p. 1350), the authors studied violence within the prison systems of "Arizona, Illinois, Minnesota, and Utah" (Briggs et al., 2003, p. 1351); with Arizona, Illinois, and Minnesota functioning as the experimental sites, and Utah functioning as the comparison site (Briggs et al., 2003, pp. 1353–1357, 1365). An analysis of their results revealed that the supermax prisons did not reduce inmate-on-inmate violence (Briggs et al., 2003, pp. 1341, 1365). However, their analysis of the impact of the supermax prisons concerning inmate-on-staff violence revealed "inconsistent" results (Briggs et al., 2003, p. 1365). It was determined that the opening of two of the four supermax facilities had no impact on inmate-on-staff violence (Briggs et al., 2003, p. 1365). The opening of one

of the four supermax facilities resulted in a decrease of inmate-on-staff violence (Briggs et al., 2003, p. 1365). Lastly, the opening of one of the four supermax facilities resulted in a "temporary increase" of inmate-on-staff violence (Briggs et al., 2003, p. 1365). Briggs et al. concluded that their "findings, on the whole, are not consistent with either incapacitation or deterrence theory" (p. 1367). The results concerning the Illinois supermax were further confirmed by the same authors in Sundt, Castellano, and Briggs (2008).

Mears and Bales (2009) tested two competing hypotheses: (1) supermax incarceration decreases recidivism, versus (2) supermax incarceration increases recidivism. For this study, Mears and Bales used data on inmates who had been released from supermax from 1996 to 2001, with a three-year follow-up period to measure their recidivism (p. 1141). These data were then compared to the data on nonsupermax inmates who had been matched using propensity score analysis (Mears & Bales, 2009, p. 1141). For this study, recidivism was defined as a new felony conviction occurring within the three-year follow-up period (Mears & Bales, 2009, p. 1142). Although, prior to matching, the supermax inmates recidivated at a much higher rate (58.8% versus 46.6%), the matching analysis presents a different picture (Mears & Bales, 2009, pp. 1149–1150). After matching the two groups using propensity scoring, the supermax inmate recidivism rate was only slightly higher than that of the nonsupermax inmates (58.8% versus 57.6%) (Mears & Bales, 2009, p. 1150, Table 2). Upon the division of recidivism into crime categories (any recidivism, violent recidivism, property recidivism, drug recidivism, and "other" recidivism), the picture became even clearer (Mears &

Bales, 2009, p. 1150). Within the five categories, the only statistically significant difference was among violent recidivism, with the supermax inmates' recidivism rate being 3.7% higher (the difference between the supermax inmates and the nonsupermax inmates on violent recidivism, prior to matching, was 13.3%) (Mears & Bales, 2009, pp. 1150–1151).

Mears and Bales (2009) also analyzed whether the length of time inmates spent incarcerated in a supermax institution affected their recidivism rate, but did not find evidence indicating any effect of sentence length on recidivism (p. 1151). Mears and Bales also evaluated whether the recency of an inmate's stay in supermax incarceration, relative to being released, affected their recidivism rate (pp. 1152–1153). Again, the data did not indicate that the recency of supermax confinement in relation to the inmates' release influenced their recidivism rate (Mears & Bales, 2009, p. 1152).

Motiuk and Blanchette (2001) compared the characteristics of those inmates who had been in administrative segregation (both voluntarily and involuntarily) in Canada's federal prison system against a group of randomly drawn nonsegregated inmates from Canada's general prison population. Significance tests showed that there were very few differences between the voluntarily segregated inmates (54.8% of the treatment group) and the nonvoluntarily segregated inmates (45.2% of the treatment group) (Motiuk & Blanchette, 2001, p. 134). Specifically, the two types of segregated inmates did not significantly differ with regards to age (Motiuk & Blanchette, 2001, p. 136), criminal history (except for sex offenses and prior adult incarcerations) (Motiuk & Blanchette, 1997, p. 6), "risk/need levels at admission" (Motiuk & Blanchette, 1997, p. 8), risk of

recidivism (Motiuk & Blanchette, 1997, p. 10), security classification (Motiuk & Blanchette, 1997, p. 11), and criminogenic needs (Motiuk & Blanchette, 1997, p. 13).

Motiuk and Blanchette (2001) compared the "prison release outcomes" of these segregated and nonsegregated prisoners, and found that the segregated prisoners were "significantly more likely than non-segregated offenders to have been returned to federal custody for any reason and with a new offence while on conditional release" (p. 137). The authors then compared subgroups of segregated and nonsegregated criminals. First, the authors compared the subgroups of segregated and nonsegregated criminals based upon their prior criminal history, dividing the inmates into three subgroups: (1) those who had previous youth court involvement, (2) those who had previous adult court involvement, and (3) those who had previously experienced segregation (Motiuk & Blanchette, 2001, p. 138). They found that "among all three criminal history background variables . . . segregated offenders were more likely to have been returned to federal custody and with a new offence post-release" (Motiuk & Blanchette, 2001, p. 138). Second, Motiuk and Blanchette divided the inmates up into subgroups based upon their Statistical Information on Recidivism – Revised1 (SIR-R1) score (Motiuk & Blanchette, 2001, p. 139). The SIR-R1 is designed to estimate the likelihood that an offender "will re-offend within three years after release" (Motiuk & Blanchette, 2001, p. 139). The segregated and nonsegregated groups were subdivided into risk-level groups and then compared against each other (Motiuk & Blanchette, 2001, p. 139). The five risk-level groups were: very poor, poor, fair, good, and very good (Motiuk & Blanchette, 2001, p. 139). "Among all five SRI-R1 release risk groupings . . . segregated offenders were more

likely to have been returned to federal custody and with a new offence post release" (Motiuk & Blanchette, 2001, p. 140). All of these results indicating the higher re-offense rate of the segregated inmates post-release suggest that Canada's administrative segregation does not have a deterrent effect on its inmates (with regard to post-release offending).

Similarities and Differences Between Solitary Confinement and Disciplinary Segregation

Solitary confinement and disciplinary segregation have many characteristics in common. Some of these characteristics are:

- the prisoners are isolated from the general prison population in a separate cellblock,
- held within their cells for 22-24 hours each day and only permitted one hour of exercise,
- placed in restraints when removed from their cells,
- housed in cells that are continuously lit all day and night by artificial light, with no prisoner control over how brightly their cells are lit, and
- their exposure to physical and social stimulation is severely limited (Arrigo & Bullock, 2008; Briggs, Sundt, & Castellano, 2003; J. Duncan, personal communication, March 13, 2014; Haney & Lynch, 1997; Haney, 2003; Lippke, 2004; Pizarro & Stenius, 2004; Smith, 2006).

The primary differences between solitary confinement and disciplinary segregation (as used within the Oregon DOC system) appear to be:

- the offenders serve their punishment in a two-person cell (and sometimes have a cellmate, depending on the housing situation), whereas in solitary confinement they are housed in one-person cells (J. Duncan, personal communication, March 13, 2014), and
- the length of stay in disciplinary segregation is shorter, with the maximum length being six months (Or. Admin. R. 295-105-0066(10)).

Literature Review Related to Key Variables and Concepts

Certain characteristics and factors have been found to be associated with offending. Moreover, certain characteristics and factors have been found to be associated with those inmates who engage in institutional misbehavior and disruption. Some of these factors include age, length of time incarcerated, LS/CMI risk score, and gender. For this study, these factors can be used to statistically control for possible naturally occurring differences between the treatment cohort and the comparison cohort (i.e., selection bias).

Age and Prison Misconduct

Strong support can be found in the extant literature with regard to age being a predictor of offending (e.g., Farrington, 1986; Gendreau, Goggin, & Law, 1996; Hirschi & Gottfredson, 1983; Nagin & Land, 1993; Sweeten, Piquero, & Steinberg, 2013), including support for age as a predictor of institutional misconduct (Alexander & Austin, 1992, p. 76; Celinska & Sung, 2014, p. 228; Cunningham & Reidy, 1998, pp. 84–85; Cunningham, Sorensen, & Reidy, 2005, p. 44; Flanagan, 1980, pp. 360–361; Flanagan, 1983, pp. 33–34, 36; Gendreau, Goggin, & Law, 1997, para. 19; Jiang & Winfree, 2006,

p. 45; McCorkle, 1995, p. 59; Sorensen & Wrinkle, 1996, pp. 547–549; Toch & Adams, 2002, p. 53–61). Toch and Adams (1989, 2002) evaluated the prison adjustment of inmates within the New York State prison system, with their sampling frame consisting of those who had been released between July 30, 1982, and September 1, 1983 (n = 10,534) (Toch & Adams, 2002, pp. 38–39). The data revealed that younger inmates were more likely to engage in prison misconduct, even when controlling for length of time served (Toch & Adams, 2002, pp. 57–61). In addition, age at prison entry was also a significant predictor of misconduct ($p = .000$) (Toch & Adams, 2002, p. 54, table 3.1). Flanagan (1980) studied the patterns of prison misconduct among long-term and short-term inmates who had been released from custody in a Northeastern state during the years 1973 to 1976 ($n = 1,466$) (p. 359). The study's data showed that age exhibited an important influence on prison misconduct, with older inmates having "far lower misconduct rates" (Flanagan, 1980, pp. 360–361), which confirmed prior research findings (Flanagan, 1980, p. 359). A subsequent investigation of data gathered from the same sample population revealed that age was a statistically significant predictor of prison misconduct ($\phi = .32$, $p < .001$) (Flanagan, 1983, p. 33–34). McCorkle's (1995) research focused on institutional misconduct among male and female inmates while taking into account the inmates' mental health status, using Bureau of Justice Statistics data from several states. McCorkle's data showed that age was a statistically significant predictor of prison misconduct (pp. 58–59). Sorensen and Wrinkle (1996) studied the disciplinary infraction rates of inmates who had been sentenced to life-without-parole, to death, and to life-with-parole. The authors discovered that even among these inmates,

age was significantly related to their infraction rate (Sorensen & Wrinkle, 1996, p. 547). Gendreau, Goggin, and Law (1997) conducted a meta-analysis using 39 studies on prison misconduct (para. 1). Their analysis of data from 12,088 subjects indicated that age was a "significant predictor" of prison misconduct (Gendreau, Goggin, & Law, 1997, para. 19). Cunningham, Sorensen, and Reidy (2005) assessed different variables related to violent prison misconduct in their study of 2,505 Missouri inmates, with the data covering an 11-year span (1991 to 2002). The authors, consistent with prior research, found that age was a significant predictor of violent misconduct, with younger inmates being more at risk of engaging in prison violence than older inmates (Cunningham, Sorensen, & Reidy, 2005, pp. 44–45). In their evaluation of the differences between male and female inmate prison adjustment, Jiang and Winfree (2006) found that for both genders, age was inversely related to inmates' rule violation rate (younger inmates engaged in misconduct at a higher rate than older inmates) (pp. 45–46). Similarly, in another gender-focused study on prison adjustment, Celinska and Sung (2014) also found that age was negatively related to prison misconduct among both male and female inmates (p. 228).

Haun (2007) studied the static and dynamic predictors of institutional misconduct for inmates housed within the Oregon DOC prison system (n = 17,054). Haun found that among Oregon inmates, age was significantly correlated with the overall yearly disciplinary infraction rate (r = .157, p < .001) (p. 152). This pattern held constant across all infraction categories. Haun reported that "as age increased, yearly rates of physically aggressive/violent (r = −.132, p < .001), verbally aggressive/defiant (r = −.129, p < .001), and nonaggressive/nonviolent infractions (r = −.112, p < .001) decreased" (p. 152). After

controlling for the length of time served using hierarchical binary logistic regression analysis, Haun's results showed that age was still a significant predictor of prison misconduct ($b = .037$, $Wald = 747.76$, $p < .001$) (p. 219).

Length of Incarceration and Prison Misconduct

The length of time an inmate has spent on her present sentence can serve as a predictor of institutional misconduct (Cunningham, Sorensen, & Reidy, 2005, p. 47; Gover, Perez, & Jennings, 2008, pp. 389–390; Haun, 2007, p. 159; Reidy, Cunningham, & Sorensen, 2001, pp. 71–73; Sorensen & Wrinkle, 1996, pp. 547, 549; Toch, Adams, & Grant, 1989, pp. 14–22; Toch & Adams, 2002, pp. 55–61; Zamble, 1992, p. 419; *but see* Flanagan, 1980, p. 363; Harer & Langan, 2001, p. 521). Zamble (1992) evaluated the prison adjustment of 25 Canadian inmates (p. 411). He found that the number of prison disciplinary infraction convictions "was significantly lower in the last period" of the inmates' incarceration term than it was "at the beginning of the term" (Zamble, 1992, p. 419). Sorensen and Wrinkle (1996) studied the prison disciplinary infraction rates for three different categories of Missouri inmates: those who were serving life-without-parole sentences, those who had been sentenced to death, and those who were serving life-with-parole sentences (pp. 542–544). Their data spanned the time period of 1977 to 1992 (Sorensen & Wrinkle, 1996, p. 545). The authors found that *time served* was a significant predictor of "overall rates of infractions," and that the type of sentence the inmate was serving was not a significant predictor of institutional infractions (Sorensen & Wrinkle, 1996, p. 547). Similar to Zamble's (1992) data, the data of Sorensen and Wrinkle (1996) suggested that disciplinary infractions rise "during the initial period of

confinement but then [begin] to decline" (p. 549). Toch and Adams's (1989, 2002) evaluation of prison adjustment among New York inmates also investigated patterns of inmate misconduct over the length of the prison term. The authors found that disciplinary infraction rates are typically subject to a sharp rise in the beginning of inmates' sentences, which is then followed by a decline in disciplinary infractions (Toch & Adams, 2002, pp. 55–57). Of note, "the rate of decline is related to the length of the sentence, with short-term inmates showing the steepest drop and long-term inmates displaying the most gradual decline" (Toch & Adams, 2002, p. 57). This pattern is generally characteristic of the younger inmates, whereas the disciplinary infraction rate of the older inmates was "relatively flat" over time (Toch & Adams, 2002, p. 57).

Since *age* has consistently been found to be a significant predictor of disciplinary infractions, Toch and Adams (2002) also investigated whether the predictability of the variable *time served* was simply due to inmates getting older as their length of time served increased (pp. 59–61). They measured the misconduct rates of "age-equivalent groups at various points in the prison term" and found that misconduct rates were still typically "lower at the end of prison sentences than at the beginning" (Toch & Adams, 2002, p. 59). Their data showed that *age* was a stronger predictor of institutional misconduct than length of *time served* (Toch & Adams, 2002, p. 60). Nevertheless, their data also indicated that *time served* was an independent predictor of institutional misconduct apart from *age* (Toch & Adams, 2002, pp. 59–61).

More recently, Gover, Perez, and Jennings (2008) tested the "importation" theory of prison adjustment as well as the "deprivation" theory of prison adjustment, using data

gathered on “a cross-sectional sample of 247 inmates confined to” prison facilities within an unnamed southeastern state (pp. 387, 382). Measuring the level of institutional misconduct was a part of their investigation (Gover, Perez, & Jennings, 2008). They found that for the male inmates (n = 188), “every one standard deviation increase in current length of stay significantly increased the mean number of infractions committed by men by 275%”; whereas “a 1 standard deviation increase in length of stay was associated with a 655% increase in the mean number of institutional infractions” for the female inmates (Gover, Perez, & Jennings, 2008, pp. 389–391).

Haun’s (2007) study on Oregon inmates revealed that the length of time served by an inmate on his or her current sentence term was a statistically significant predictor of prison misconduct (p. 159). The evidence showed that “significant negative associations with the number of days incarcerated were found across each infraction category, such that as the number of days of incarceration increased, the yearly rate of infraction decreased” (Haun, 2007, p. 159).

LS/CMI Risk Score

The Level of Service / Case Management Inventory (LS/CMI) is a risk assessment tool that is administered to Oregon inmates during the initial intake process (J. Hanson, personal communication, February 20, 2015). The LS/CMI is part of a family of risk assessment instruments known collectively as the Level of Service (LS) scales, with the LS/CMI being the most recently developed instrument (Olver, Stockdale, & Wormith, 2014, pp. 156–157). The LS scales are the “most frequently used risk assessment tools” in the world (Olver et al., 2014, p. 156). The LS scales are based upon

the risk-need-responsivity (RNR) framework, which is itself based upon "general personality and cognitive social learning theory" (Olver et al., 2014, p. 157; Andrews, Bonta, & Wormith, 2004, p. 1; Andrews & Bonta, 2010, p. 46; Andrews et al., 2012, p. 115). Three primary principles make up the RNR model: (1) Risk principle: Higher risk offenders should be targeted with higher intensive services (and lower risk offenders should not be targeted with high intensive services), (2) Need principle: Focus on dynamic risk factors ("criminogenic needs") in treatment, (3) Responsivity principle: tailor the intervention delivery in a manner that is compatible with the client's learning ability (Andrews & Bonta, 2010, pp. 44–45; Jung, Daniels, Friesen, & Ledi, 2012, p. 602). The LS scales incorporate the "central eight" risk factors (Olver et al., 2014, p. 156), which have been empirically shown to predict criminal behavior (Andrews et al., 2012, p. 116; Andrews & Bonta, 2010; Gendreau, Little, & Goggin, 1996). These eight risk factors are further divided into the "big four" and the "modest four," with the "big four" having been found to be stronger predictors of criminal recidivism than the "modest four" (Andrews et al., 2012, p. 116; Andrews & Bonta, 2010, p. 46; Andrews et al., 2004, p. 1). The factors comprising the "big four" are: (1) antisocial cognition (e.g., antisocial attitudes and beliefs), (2) antisocial associates, (3) a history of antisocial behavior (criminal behavior), and (4) antisocial personality pattern (Andrews et al., 2004, p. 1; Andrews et al., 2012, p. 116). The factors comprising the "modest four" are: (1) family/marital, (2) school/work, (3) leisure/recreation, and (4) substance abuse (Andrews et al., 2004, p. 1).

The original LS scale was the Level of Service Inventory (LSI; Andrews, 1982), which was then followed by the Youth Level of Service Inventory (YLSI; Andrews, Robinson, & Hoge, 1984). The LSI was improved upon with the release of its second edition, the Level of Service Inventory - Revised (LSI-R; Andrews & Bonta, 1995). Further refinement led to the Level of Service Inventory - Ontario Revision (LSI-OR; Andrews, Bonta, & Wormith, 1995). The LSI-OR then became the Level of Service / Case Management System (LS/CMI; Andrews et al., 2004) – the LSI-OR "served as the pilot version of the LS/CMI" (Olver et al., 2014, p. 157), and the two versions "are virtually identical in their construction" (Andrews et al., 2004, p. 3). The LSI-R is only comprised of one section containing 54 items (Andrews et al., 2004, p. 3). The risk score is calculated by adding up all of the items that apply to the offender (Andrews et al., 2004, p. 3). In contrast, the LS/CMI is comprised of 11 sections, but the numerical risk score is generated only from Section 1 (Andrews et al., 2004, p. 3). The other remaining sections are related to additional factors that may be taken into account when considering the offenders' classification (e.g., overriding the risk classification of the inmate – which initially is based on the offender's numerical risk score taken from Section 1), and case management issues (Andrews et al., 2004). The LSI-R essentially serves as Section 1 of the LS/CMI, although a few changes were made in order to improve the scale (Andrews et al., 2004). The LSI-R's 54 items were grouped into 10 subcomponents, whereas the items that comprise Section 1 of the LS/CMI have been reduced to 43 and they are grouped into only 8 subcomponents (Andrews et al., 2004, p. 3). The purpose of this change was to more closely focus and align the scale with the "central eight" risk factors

that research has shown to be predictive of offending (Andrews et al., 2004, p. 3). In order to accomplish this refinement, the LSI-R's "financial" section and the "accommodation" section were removed, along with the removal of 11 individual items (Andrews et al., 2004, p. 3). There is a high correlation between the LSI-R and the LS/CMI (Andrews et al., 2004, p. 5). For example, Andrews, Bonta, and Wormith (2004) report that Rowe's (1999) data revealed a correlation of .96 between the two scales, and their own research revealed that among U.S. inmates the correlation between the two scales was .97 (Andrews et al., 2004, pp. 5–6).

Research has shown that the LS/CMI is a reliable and valid predictor of offending. Reliability refers to whether the technique or test yields consistent results if administered many times (Field, 2013, p. 12). Validity refers to whether the technique or test truly measures and predicts what it is intended to measure or predict (Field, 2013, p. 12). Andrews et al. (2004) reported the Cronbach's alpha statistic (Cronbach, 1952) for the LS/CMI across several studies and sample populations. The Cronbach's alpha for the LS/CMI's total score ranged overall from .88 to .91 across these studies (Andrews et al., 2004, p. 108, table 5.1). Specifically with regard to the three studies that included male inmate populations, the Cronbach's alpha was .89, .91, and .87 (Andrews et al., 2004, p. 108, table 5.1). These results provide strong evidence of the internal consistency of the LS/CMI, thereby indicating that the LS/CMI is a reliable instrument (Andrews et al., 2004, p. 107).

The "validity" of a scale "refers to the degree of evidence to support the interpretations and uses proposed for the instrument" (Andrews et al., 2004, p. 117). One

of the main purposes of the LS/CMI is to predict offending (Andrews et al., 2004). Andrews et al. (2004) reported that the predictive accuracy of the LS/CMI in predicting general recidivism is strong (r = .41; AUC = .739), and it is also strong with regard to predicting violent recidivism (r = .29; AUC = .666) (Andrews, Bonta, & Wormith, 2006, pp. 13, 15; Jung, Daniels, Friesen, & Ledi, 2012, p. 605; Andrews et al., 2004, pp. 117–121). In relation to the other LS scales, Olver et al.'s (2014) meta-analysis indicated that the LS/CMI has the largest effect size for general recidivism (r = .42, p < .001, random effects; r = .44, p < .001, fixed effects) (p. 168, table 12). This data thus shows that the LS/CMI is a superior predictor of general recidivism than its predecessors the LSI (r = .32, p < .001, random effects; r = .32, p < .001, fixed effects) and the LSI-R (r = .25, p < .001, random effects; r = .24, p < .001, fixed effects) (Olver et al., 2014, p. 168, table 12). The LS/CMI has also been demonstrated to have good construct validity (Jung et al., 2012). This is shown in part by the concurrent validity of the LS/CMI (Trochim, 2006), as indicated by the strong correlation between the LSI-R and the LS/CMI (mentioned above; Andrews et al., 2004). In addition, the research by Jung et al. (2012) showed "moderate to strong" concurrent validity for six of the LS/CMI subscales. Specifically, moderate to strong correlations were found between subscales of the Personality Assessment Inventory (PAI; Morey, 2007) and the Minnesota Multiphasic Personality Inventory-2 (MMPI2; Butcher, Dahlstrom, Graham, Tellegen, & Kaemmer, 1989) and other measures, with the LS/CMI subscales of criminal history, family/marital, companions, alcohol/drug problem, attitudes/orientation, and antisocial pattern (Jung et al., 2012).

There appears to be a lack of studies testing the predictive validity of the LS/CMI with regard to its accuracy in predicting institutional misconduct among male inmates. However, several studies have evaluated the predictive validity of its predecessors the LSI and the LSI-R in predicting institutional misconduct, and Stewart (2011) studied the predictive validity of the LS/CMI in predicting prison misconduct among female inmates. Stewart's analysis of 101 female Canadian federal inmates found that the LS/CMI was indeed statistically significantly correlated with prison misconduct (r = .502, AUC = .798, p < .01) (pp. 63–64, tables 20–21). The field of research on the prior LS scales, especially the research pertaining to the LSI-R, along with Stewart's study on Canadian female inmates, all suggest that the LS/CMI Section 1 risk score is a good predictor of institutional misconduct.

LSI and institutional misconduct. Bonta and Motiuk (1987) conducted two studies that evaluated the relationship between the LSI score of inmates and their misconduct rate. For the first study, they found that the inmates with an LSI score of 14 or less, out of 58 items (i.e., the low-scoring inmates), committed fewer institutional misconducts (F = 7.60 (1, 89), p < .01) (Bonta & Motiuk, 1987, p. 309). With the second study, the data again showed that the LSI score was related to institutional misconduct (r = .19, p < .05) (Bonta & Motiuk, 1987, p. 316). Bonta (1989) again studied the LSI, this time in relation to prison misconducts among Native and non-Native Canadian jail inmates (n = 126) (p. 52). He found correlations of .26 (Native inmates) and .31 (non-Native inmates) between LSI score and prison misconduct (Bonta, 1989, p. 56, table 2). Next, in a study focused on the LSI and halfway-house placement, Bonta and Motiuk's

(1990) data revealed that inmates with a low LSI score (14 and below) committed fewer misconducts, fewer assaults, and had fewer disciplinary reports of any kind, when compared to inmates with an LSI score above 14 (p. 503, table 2). In an extensive study of 510 Canadian inmates, Motiuk (1991) found that the correlation between the inmates' LSI score and prison misconduct was .26 ($p < .001$) (Motiuk, 1991, p. 152, table 21). For assaults, the LSI score correlation was .18 ($p < .01$) (Motiuk, 1991, p. 152, table 21). Controlling for the number of days inmates were incarcerated, the research by Bonta and Motiuk (1992) on over 400 inmates revealed that the LSI was still a significant predictor of prison "occurrences" (i.e., behavior that results in a less formal misconduct process than official "misconducts"; $r = .10$, $p < .05$), prison misconducts ($r = .10$, $p < .01$), and prison assaults ($r = .08$, $p < .05$) (pp. 346–347).

LSI-R and institutional misconduct. Gendreau, Goggin, and Law (1997) conducted a meta-analysis on 39 studies in order to study predictors of prison misconduct (para. 1). In addition to looking at various variables such as age, criminal history, and race, the authors also evaluated the predictive ability of some actuarial measures. The actuarial measures were divided into categories that included the: (1) Minnesota Multiphasic Personality Inventory (MMPI; Hathaway & McKinley, 1943), (2) non-MMPI measures of antisocial personality, (3) LSI-R, and (4) "other risk measures" (Gendreau, Goggin, & Law, 1997, para. 25). Following the method described by Gendreau et al. (1997) in interpreting the data (para. 19), their results concerning the LSI-R data were: The meta-analysis data showed that across the 39 studies sampled, a quantitative relationship between the LSI-R as a predictor and the criterion (i.e.,

misconducts) was reported on 10 occasions involving a total of 2,252 subjects (Gendreau et al., 1997, table 1). The associated mean Pearson's *r* for the LSI-R with the outcome was .23 (SD = .11), and z+, the weighted estimation of Pearson's *r* for the LSI-R with the outcome, was .22 (Gendreau et al., 1997, table 1). Application of Hedges and Olkin's (1985) method for testing the significance of the mean z+ values confirmed the LSI-R as a significant predictor of misconducts (Gendreau et al., 1997, table 1).

A one-way analysis of variance (ANOVA) was performed on the data, which revealed "that there was a significant difference across the actuarial measures $F(3, 65) = 7.32$, $p < .05$" (Gendreau et al., 1997, para. 26). Further analysis "found that the LSI-R was a significantly better predictor of the criterion [i.e., misconducts] than were the other three" (Gendreau et al., 1997, para. 26). Moreover, the data revealed that the LSI-R was superior to the other predictor domains. When compared to the other predictor domains of MMPI, "other risk measures," and non-MMPI, the LSI-R produced higher correlations 82% of the time, 74% of the time, and 86% of the time, respectively (Gendreau et al., 1997, para. 27).

Kroner and Mills (2001) compared the predictive accuracy of five risk appraisal instruments with regard to prison misconducts. The five instruments were the LSI-R, the Psychopathy Checklist-Revised (PCL-R; Hare, 1991), the HCR-20 (Webster, Eaves, Douglas, & Wintrup, 1995), the Violence Risk Appraisal Guide (VRAG; Harris, Rice, & Quinsey, 1993), and the Lifestyle Criminality Screening Form (LCSF; Walters, White, & Denney, 1991). A total of 97 male inmates were involved in the study, and the predictive accuracy of the instruments was evaluated using correlations and receiver operating

characteristics (ROCs) (Kroner & Mills, 2001, pp. 476, 479). For the LSI-R, the data revealed correlations of .27 (AUC = .663) and .20 (AUC = .609) for minor misconducts and major misconducts, respectively (Kroner & Mills, 2001, p. 482, table 3). Although the VRAG was the strongest predictor of both major and minor misconducts, there were no statistically significant differences between the risk scales' correlations for either type of misconduct (Kroner & Mills, 2001, p. 481).

Swoboda (2006) used regression analysis to determine the predictive accuracy of the LSI-R in predicting prison misconduct (p. 52). Swoboda's sample consisted of 129 male inmates at a Pennsylvania prison (pp. 45, 47–48). Swoboda's analysis revealed that the LSI-R total score validly predicted prison misconducts to a statistically significant degree ($r = .21$, $p < .001$). Swoboda further found that the LSI-R individual scales of Criminal History ($r = .12$, $p < .05$), Financial ($r = .13$, $p < .05$), Accommodation ($r = .15$, $p < .05$), Companions ($r = .15$, $p < .05$), and Emotional/Personal ($r = .22$, $p < .001$) were each significantly correlated with prison misconduct (pp. 61, 63).

Campbell, French, and Gendreau (2009) conducted a meta-analysis using 88 studies reporting 76 effect sizes related to institutional violence (p. 572). One of their purposes in conducting the meta-analysis was to evaluate and compare the predictive accuracy of several different risk assessment measures in predicting violent prison misconduct (Campbell, French, & Gendreau, 2009, pp. 571, 572). The risk assessments were divided into six categories, one of which was the LSI/LSI-R category (Campbell et al., 2009, p. 575, table 1). From the 76 studies, there were six effect sizes related to the LSI or the LSI-R, with a total 650 participants (Campbell et al., 2009, p. 575, table 1).

The authors found that LSI/LSI-R category to be moderately predictive of institutional violence (Z^{+} = .24) (Campbell et al., 2009, pp. 575, 581).

Olver, Stockdale, and Wormith (2014) conducted a meta-analysis on the various Level of Service scales, including the LSI, the LSI-R, and the LS/CMI. Their overall evaluation included a total of 128 studies and 137,931 participants, and considered several different outcome measures (e.g., recidivism, institutional misconduct, reincarceration) (p. 160). With regard to general recidivism, the LS/CMI had the largest effect size overall (r_w = .44, p < .001, fixed effects; r_w = .42, p < .001, random effects) when compared to the other LS scales (Olver et al., 2014, p. 168, table 12), thus indicating that it was the strongest predictor of general recidivism. In addition, the meta-analysis further revealed that the LS scales significantly predicted prison misconduct. Across 16 samples and 3,834 inmates, the data showed that the LS scales significantly predicted any institutional misconduct (r_w = .21, p < .05, fixed effects; r_w = .24, p < .05, random effects), whereas across 15 samples and 3,474 inmates, the data also showed that the LS scales significantly predicted serious institutional misconduct (r_w = .20, p < .001, fixed effects; r_w = .21, p < .001, random effects) (Olver et al., 2014, p. 162, table 3). These results are quite robust concerning the LS scales' predictive validity of prison misconduct. In order for these effect sizes to become nonsignificant a large number of studies with null findings would need to be generated (M. Olver, personal communication, December 6, 2014). In fact, "fail safe N analyses demonstrated that 750 missing studies with null findings would be required to reduce the general misconduct prediction effect size to nonsignificance, and 451 missing studies with null findings

would be required to do the same for the serious misconduct effect size" (M. Olver, personal communication, August 14, 2015).

LS/CMI and the Oregon DOC. Beginning in 2007, the LS/CMI was administered to all incoming prison inmates who had scored as medium risk or high risk on the Automated Criminal Risk Score (ACRS) (J. Hanson, personal communication, February 20, 2015). The ACRS is an internal static risk tool developed and used by the Oregon DOC (J. Hanson, personal communication, July 20, 2015). Beginning in 2011, the LS/CMI is now administered to all incoming prison inmates (J. Hanson, personal communication, February 20, 2015).

Gender and Prison Misconduct

The rate of institutional misconduct can differ between male and female inmates (Celinska & Sung, 2014, p. 227; Craddock, 1996, para. 19; Harer & Langan, 2001, pp. 521–523; Haun, 2007, pp. 150–152, 218; *but see*, Gover, Perez, & Jennings, 2008, p. 388; Jiang & Winfree, 2006, p. 43). In addition, male inmates and female inmates may adjust differently to prison life (Warren, Hurt, Loper, & Chauhan, 2004; Jiang & Winfree, 2006).

Craddock (1996) compared and contrasted the violation rate of North Carolina female (n = 1315) and male (n = 3551) prison inmates. She found that male inmates typically committed more rule violations than female inmates (Craddock, 1996, para. 19). Approximately one-half of the male inmates had committed at least one rule violation, compared to approximately one-third of the female inmates (Craddock, 1996, para. 19). In addition, "the median number of infractions was 3 for men and 2 for women"

(Craddock, 1996, para. 19). Similarly, Harer and Langan (2001) found in their study that the prison violence misconduct rate was greater for male inmates than female inmates. Their data revealed that "the average female rate for violence-related misconduct [was] 54.4% of the average male rate" (Harer & Langan, 2001, p. 521). In addition, "the mean female rate for serious violence [was] only 8.14% of the mean male rate" (Harer & Langan, 2001, p. 521). Using Bureau of Justice Statistics data from a large multi-state survey, Celinska and Sung (2014) found in their study that, when other variables were held constant, "being female reduced the likelihood of engaging in [prison] rule-breaking by 24.8%" (p. 227). Their results revealed "that the prevalence rate of infractions among female inmates was 38.3% as compared with the male rate of 47.6% ($p < .001$)" (Celinska & Sung, 2014, p. 227). Furthermore, the evidence showed that "women prisoners were not only less likely to break rules but also did so less frequently than men" (Celinska & Sung, 2014, p. 227).

Haun's (2007) study of Oregon prison inmates also found that gender was a significant predictor of prison misconduct, with total yearly misconduct rates significantly higher for male inmates than female inmates (p. 150). Among the different misconduct categories, the rates were significantly higher among male inmates for physically aggressive/violent infractions and nonaggressive/defiant infractions; but there was no significant difference between male and female inmates on their rate of verbally aggressive/defiant infractions (Haun, 2007, p. 150). A hierarchical binary logistic regression analysis that controlled for the effect of *time served* confirmed that gender was a significant predictor for each of the misconduct categories (p. 218).

Prior Prison Rule Violations and Subsequent Prison Misconduct

Earlier research has demonstrated that prior prison rule violations can be a predictor of subsequent prison misconduct. Some of this research has directly indicated that prior prison misconduct can predict subsequent misconduct (Camp, Gaes, Langan, & Saylor, 2003; Drury & DeLisi, 2010; Gendreau, Goggin, & Law, 1997). Other research provides indirect support that prior prison misconduct can predict future misconduct. For example, there is research that indicates that prior prison violence can predict subsequent prison misconduct (Cunningham & Sorensen, 2007). In addition, there is research that indicates that prior violence can predict future violence (Bonta, Hanson, & Law, 1998; Durose, Cooper, & Snyder, 2014; Sorensen & Pilgrim, 2000), and that prior criminal history can predict subsequent criminal offending (Bonta et al., 1998; Drury & DeLisi, 2010, p. 332; Durose et al., 2014; Gendreau, Little, & Goggin, 1996; Loza, 2003, p. 182). The proposition that prior prison misconduct predicts future prison misconduct fits within the popular notion that "the best predictor of future behavior is past behavior" (e.g., Mischel, 1973, pp. 261–263; Cunningham & Reidy, 1999, p. 32; Ouellette & Wood, 1998; Walters, 1992, pp. 85, 96, 105). It should be noted that the Oregon DOC required that this control variable be added in order to obtain its approval of the study, and its approval was necessary in order to gain access to the data.

Prior prison misconduct and subsequent misconduct. Gendreau, Goggin, and Law's (1997) meta-analysis found past prison adjustment to be "the single best personal domain predictor of [prison] misconducts (r = .21)" (para. 34), surpassing other personal domain predictors such as age, antisocial attitudes and behavior, prior criminal record,

education, and marital status (Table 1). Camp, Gaes, Langan, and Saylor (2003) conducted a study involving more than 120,000 federal inmates, in which they produced 14 multivariate models in order to study the effect of prisons on inmate misconduct. Consistent across all 14 multivariate models, the authors found prior incidents of misconduct to be a significant predictor of subsequent misconduct, and it was the strongest predictor among the individual demographic variables studied, even outperforming the variable *age* (pp. 515–516). This was true "regardless of how misconduct was categorized" (e.g., "prior any misconduct," "prior violent misconduct," "prior drug misconduct") (Camp et al., 2003, pp. 515–516). The "effect" of the variable was positive, in that "individuals with higher counts of prior misconduct were more likely to be involved in a current instance of misconduct" (Camp et al., 2003, pp. 515–516).

Drury and DeLisi (2010) drew a random sample of 1,005 (males, $n = 831$; females, $n = 174$) participants from the 26,869 inmates incarcerated with the Arizona Department of Corrections in 2001. The purpose of the study was to examine the relationship between prison misconduct that occurred during prior prison terms and prison misconduct that occurred in a subsequent prison term (Drury & DeLisi, 2010, pp. 331–333, 336). Drury and DeLisi found "that prior adjustment denoted a strong significant effect across all models in explaining institutional misconduct for both male and female inmates and for both violence and nonviolence types of misconduct" (p. 334). With regard to the total sample (males and females combined), their findings revealed that the inmates who had engaged in misconduct in a prior prison term were significantly more likely to commit minor prison violations (estimate = .601, $z = 8.33$, $p < .01$), and

significantly more likely to commit major prison violations (estimate = .643, $z = 6.24$, $p < .01$) (Drury & DeLisi, 2010, pp. 339–340). When the analysis was limited to only the male inmates in the sample ($n = 831$) it produced similar results. Their findings with regard to the male-only sample revealed that the inmates who had engaged in misconduct in a prior prison term were significantly more likely to commit minor prison violations (estimate = .593, $z = 8.16$, $p < .01$), and significantly more likely to commit major prison violations (estimate = .626, $z = 5.99$, $p < .01$) (Drury & DeLisi, 2010, pp. 340–342).

Prior prison violence and subsequent misconduct. Cunningham and Sorensen (2007) performed a logistic regression analysis on retrospective data concerning 24,514 Florida Department of Corrections inmates who were incarcerated during the year of 2003 (pp. 241, 243). Their analysis revealed that prior prison violence significantly predicted future prison misconduct across all categories of misconduct. The findings showed that prior prison violence was a significant predictor of (1) total prison violations, (2) misconducts of potential violence, (3) all assaults, (4) assaults with injuries, and (5) assaults with serious injuries (Cunningham & Sorensen, 2007, p. 249, Table 5). In fact, "those who had a prior record of committing violent acts in prison were more than twice as likely to commit a violent act in the institution" (Cunningham & Sorensen, 2007, p. 248).

Prior violence and subsequent violence. Sorensen and Pilgrim (2000) examined incarcerated murderers in Texas, and found that previous acts of violence to be a significant predictor of prison violence (p. 1264). Their results showed that, with regard to the incarcerated murderers, those with "an additional previously attempted murder or

assault" were 4.0 percentage points more likely to commit a violent act in prison (Sorensen & Pilgrim, 2000, pp. 1264–1266). Bonta, Hanson, and Law's (1998) meta-analysis on mentally disordered inmates found that a history of violence was a significant predictor of violent recidivism (mean effect size, *Zr* = .16) (pp. 132–133). Durose, Cooper, and Snyder (2014) studied the recidivism of 404,638 U.S. state prisoners released in 2005. Within that sample, of those who had been incarcerated for a violent offense, 33.1% were arrested for another violent offense within 5 years of being released (Durose et al., 2014, p. 9).

It should also be noted that the ability of past violence to predict future violence depends on similar factors being present. As Cunningham and Reidy (2002) explained, "prison is a profoundly different context than the community. It cannot be assumed that factors associated with violence in the community will be predictive in a markedly different context" (p. 525). Violence in the community is not necessarily predictive of violence in prison (Cunningham & Reidy, 2002, p. 528).

Prior criminal history and subsequent criminal behavior. Research indicating that prior criminal history predicts subsequent criminal conduct further supports, albeit indirectly, the idea that prior prison rule violations predict subsequent rule violations. Camp and Gaes (2005) explained:

> In short, although the jury is still out, there appears to be solid support for the notion that inmate misconduct in prison and criminal behavior on the street arise from similar propensities among individuals. Indeed, the very idea of criminal

> trajectories suggests continuity in behavior over time even though settings and institutions may change. (p. 428)

Gendreau, Goggin, and Law (1996) conducted a meta-analysis of 131 studies on recidivism. Across the 131 studies sampled, a quantitative relationship between the predictor "criminal history" and recidivism was reported on 164 occasions and involved a total of 123,940 subjects (Gendreau et al., 1996, pp. 582–583). The associated mean Pearson *r* for "criminal history" with outcome was .18 (S.D. = .13) (Gendreau et al., 1996, pp. 582–583). Application of Hedges and Olkin's (1985) method for testing the significance of the mean z^{+} values confirmed "criminal history" as a significant predictor of recidivism (Gendreau et al., 1996, pp. 582–583). "Criminal history" was a broad category, defined as "adult-prior arrest, probation, jail, conviction, incarceration, [and] prison misconducts" (Gendreau et al., 1996, p. 597).

Similarly, Bonta et al. (1998) conducted a meta-analysis focused on mentally disordered offenders. In analyzing the results of 22 studies examining criminal history and recidivism involving a total of 4,312 participants, Bonta et al. found criminal history to be significantly correlated with recidivism among mentally disordered offenders (p. 129, Table 4).

A recent Bureau of Justice Statistics study affirmed the relationship between prior criminal history and recidivism. Data gathered on "404,638 state prisoners released in 2005 from 30 states" revealed that 67.8% were arrested within 3 years of being released (Durose et al., 2014, p. 1). Within 5 years of being released, 76.6% had been arrested (Durose et al., 2014, p. 1). Durose et al. (2014) also measured recidivism as a conviction

for a new crime (as opposed to just an arrest) (p. 14). The data gathered from the 29 states that had such information showed that 45.2% of the released prisoners were convicted of a new crime within 3 years of being released, and over half (55.4%) were convicted of a new crime within 5 years of being released (Durose et al., 2014, p. 15, Table 16).

Furthermore, the results demonstrated that increases in an inmate's criminal history negatively predicted recidivism (Durose et al., 2014, p. 10). Durose et al. (2014) reported that "a year after release from prison, about a quarter (26.4%) of released inmates with 4 or fewer arrests in their prior criminal record had been arrested, compared to over half (56.1%) of released inmates who had 10 or more prior arrests" (p. 10). This trend held over time. Within five years of release from prison, 60.8% of the released inmates who had 4 or fewer arrests in their prior criminal record had been arrested, whereas 86.5% of released inmates who had 10 or more prior arrests (Durose et al., 2014, p. 10). In addition, this trend held across offense categories as well (e.g., violent offenders with a more extensive criminal history were more likely be arrested than those violent offenders with a less extensive criminal history) (Durose et al., 2014, p. 10).

LS/CMI and prior misconduct. It should be noted that the LS/CMI also includes a *criminal history* component, which takes into account both prior criminal convictions, and prior institutional misconduct or behavior reports (Andrews, Bonta, & Wormith, 2004, pp. 12–13). In addition, it should be further noted that since the LS/CMI is administered to Oregon prison inmates during intake (i.e., upon their initial admission into the prison system for their current sentence), for the purposes of this study the

control variables *LS/CMI risk score* and *prior prison misconduct* would not be measuring the same thing. The *LS/CMI risk score* control variable would be taking into account prior misconduct that occurred during a prior sentence, whereas the *prior prison misconduct* control variable would be taking into account only previous misconduct that occurred during the present sentence that the defendant was serving (i.e., the present sentence associated with the inmate's incarceration during the years 2011–2014).

Prior Time Spent in Disciplinary Segregation and Subsequent Prison Misconduct

There appears to be a lack of research studies that demonstrate that prior time spent in disciplinary segregation is predictive of subsequent prison misconduct. Therefore, it appears that there is very little scientific support for such a proposition. However, the addition of this variable may contribute to a deeper understanding of the effect prior disciplinary segregation may have on subsequent prison misconduct. It should be noted that the Oregon DOC required that this control variable be added in order to obtain its approval of the study, and its approval was necessary in order to gain access to the data.

Index Offense, Gang Affiliation, and Prison Misconduct

In addition to age, length of incarceration, and gender, Haun (2007) also found that index offense (pp. 156–157, 223) and gang affiliation (pp. 160–161, 222) were significant predictors of misconduct within the Oregon correctional system. The literature also suggests that an inmate's "index offense" (i.e., the most serious crime that provides the basis for the defendant's current prison sentence, Haun, 2007, p. 37) may serve as a predictor of offending (Cunningham, Sorensen, & Reidy, 2005, p. 44;

Flanagan, 1983, p. 34; Toch & Adams, 2002, p. 54). However, the evidence supporting an inmates' index offense as a predictor of institutional misconduct is relatively weak. Furthermore, the extant literature reveals that gang affiliation may also predict institutional misconduct (Fischer, 2001, pp. ii, v; Gaes et al., 2002, pp. 370, 373, 381). However, gang affiliation cannot be included as a key variable in this study since the Oregon DOC no longer keeps data on inmate gang affiliation.

Variables related to Disciplinary Segregation

Coid et al. (2003) examined the characteristics of inmates who spent time in disciplinary segregation in the England and Wales prison system. Although their study was done in England and Wales instead of the United States, thus limiting its generalizability, it still offers insight into the factors associated with prisoners who engage in institutional misbehavior. Consistent with prior research, Coid et al. found that the prisoners that had experienced disciplinary segregation, in contrast to those inmates who did not report having been subjected to disciplinary segregation, were generally younger and had a previous conviction for certain types of crimes. Coid et al. also discovered that those reporting having been subjected to disciplinary segregation were "more likely to have spent a year or longer in prison at the time of the interview" (p. 302). This was inconsistent with prior research that had indicated that the length of the sentence being served was inversely related to the commission of institutional infractions (Coid et al., 2003, p. 299). However, this difference may have had something to do with how the concept was operationalized and measured, since "1 year + in prison" (p. 303, Table 1) does not necessarily exclude the possibility that length of time incarcerated was

still inversely related to spending time in disciplinary segregation. In addition, their research found that inmates who experienced disciplinary segregation were not more likely to suffer from "severe mental illness", although they did find that "segregated men . . . were more likely to have a personality disorder" (Coid et al., 2003, p. 310).

Literature Review of Research Design

The purpose of this study was to examine the effect of disciplinary segregation on subsequent prison behavior among those subjected to it. Specifically, the aim of the study was to examine whether disciplinary segregation is effective in deterring subsequent prison misconduct among those inmates subjected to it. In addition, the study sought to examine, in the alternative, whether disciplinary segregation has a criminogenic effect on the subsequent behavior of those subjected to it, or whether disciplinary segregation has a null effect on the behavior of those subjected to it.

The ideal research design for exploring these issues would be a true experiment, where individuals are randomly assigned to a treatment group and control group (Nagin, Cullen, & Jonson, 2009, p. 131; Zimring & Hawkins, 1973, p. 253). For this study, such a design could take the form of drawing its sample from a population of offenders who, during a certain time period, were found to be in violation of the prison rules and subject to disciplinary segregation as punishment for the commission of that rule violation. Those individuals could then be randomly assigned disciplinary segregation punishment. The subsequent misconduct rate of the two groups could then be compared and contrasted. Unfortunately, such a design cannot be employed in the present study, due to

ethical and operational considerations. Instead, this study used archival and administrative data from the past. The use of such data calls for an observational study.

The fact that a true experiment with random assignment is not feasible for this study is fairly typical in criminal justice research (Zimring & Hawkins, 1973, pp. 249–250). Zimring and Hawking (1973) averred, "ordinarily, a controlled experiment is beyond the reach of a criminologist seeking to investigate punishment policy, because he is unable to randomly assign regimes of punishment between individuals in the same jurisdiction" (pp. 253–254). The authors opined that "in all but a few instances it will not be possible" to form treatment and control groups of randomly assigned individuals, and then alter between the groups the threat and punishment for misconduct (Zimring & Hawkins, 1973, p. 294). Zimring and Hawkins further asserted, "it is difficult to conceive of an acceptable experiment in which, after random assignment, the severity of sanctions threatened for a violation of a particular criminal law was varied between the two groups" (p. 294). Although they were discussing punishment and crime, the same logic holds true for punishment and prison rule violations.

When a true experiment is not feasible, as in this case, then the resulting study should aim to emulate the design features of a true experiment (Rosenbaum, 2010, p. 4; Nagin, Cullen, & Jonson, 2009). One type of research design that can be employed when using nonexperimental data (i.e., randomization will not be employed) is an observational study (Nagin, Cullen, & Jonson, 2009, p. 132; Mann, 2003, p. 59; Rosenbaum, 1989, p. 1024). In an observational study a researcher does not implement an intervention; instead, the researcher "simply observes" (Mann, 2003, p. 54). In this case, the

"observation" was the collection, collation, and analysis of the archival and administrative data held by the Oregon DOC. Specifically, the data that were available for this study called for a "retrospective" observational study, since the data had already been gathered prior to this study taking place (Mann, 2003, p. 55).

The intent of this study, and many other criminal justice and deterrence studies, was to investigate the impact of punishment on those subjected to it (Nagin, Cullen, & Jonson, 2009, p. 131). That is, the intent of the study was to discover the difference between the behavior of the individuals after being subjected to the treatment in comparison with their behavior had they not been subjected to the treatment (i.e., the "counterfactual") (Nagin, Cullen, & Jonson, 2009, p. 131). One way to perform this investigation and generate data from which an inference can be made is to compare two groups, with one comprised of individuals subjected to the treatment, and one comprised of those not subjected to the treatment (Nagin, Cullen, & Jonson, 2009, p. 131). Such a design must account for the fact that the random assignment process applied in true experiments will not be serving to isolate the effect of the treatment variable and control for bias (Nagin, Cullen, & Jonson, 2009, p. 135). Therefore, criminal justice researchers performing an observational study often utilize regression analysis to control for variables that may bias the results (Bales & Piquero, 2012, p. 80; Nagin, Cullen, & Jonson, 2009, pp. 133, 138). Regression analysis is a statistical tool that can measure the influence of variables other than the treatment variable in order to better isolate and partial-out the effect of the treatment variable. These control variables should be selected based upon evidence showing them to be "strongly related" to the outcome variable (i.e.,

prison rule violations) (Nagin, Cullen, & Jonson, 2009, p. 136) and the treatment variable (i.e., selection into the treatment or comparison cohort) (Steiner, Cook, Shadish, & Clark, 2010, pp. 250, 262).

The selection of the covariates may very well be more important in obtaining results that more closely resemble a true experiment than the selection of the data analysis technique. Furthermore, different data analysis techniques, although utilizing divergent approaches and tactics, may yield quite similar results. Steiner, Cook, Shadish, and Clark (2010) conducted a within-study comparison of a randomized experiment and a quasi-experiment. The quasi-experimental data were analyzed using propensity score analysis methods and analysis of covariance (ANCOVA) (Steiner, Cook, Shadish, & Clark, 2010, p. 254). In their study, the researchers found that "the choice of covariates has a much stronger impact on bias reduction than the choice of a specific adjustment method" (Steiner, Cook, Shadish, & Clark, 2010, p. 256). None of the data analysis methods appeared to be superior to the others, but the selection of covariates did appear to impact the results in an important way (Steiner, Cook, Shadish, & Clark, 2010, pp. 256–258). The study also revealed that more covariates do not necessarily improve the accuracy of the results – the addition of some covariates can actually increase bias (Steiner, Cook, Shadish, & Clark, 2010, p. 258). Therefore, the covariates should be selected in a critical manner based upon their correlation with the treatment and outcome variables (Steiner, Cook, Shadish, & Clark, 2010; see also Warner, 2013, Chapter 14; Field, 2013, p. 321). Using the same data, the researchers also found that analyzing the data either through ordinary linear regression or through propensity score methods

yielded similar results (Steiner, Cook, Shadish, & Clark, 2010, pp. 1339, 1342), and both types of data analysis techniques substantially reduced selection bias (Steiner, Cook, Shadish, & Clark, 2010, pp. 1339, 1341). Bales and Piquero (2012) arrived at a similar result. Bales and Piquero assessed the effect of imprisonment on reoffending using three data analysis techniques: logistic regression, precision matching, and propensity score matching (p. 73). Each of the techniques indicated that imprisonment had a criminogenic effect, when compared to noncustodial alternatives (Bales & Piquero, 2012, p. 98). The authors opined that they were "hesitant to say which method is superior, as they are all different ways of getting at the issue" (p. 98). This sentiment was echoed by Schafer and Kang (2008) when they stated "ANCOVA, regression, and propensity scores share a common goal: to eliminate biases due to confounding. However, they attack the problem from different sides" (pp. 280–281). They went on to explain, "ANCOVA and regression model relationships between confounders and the outcome, whereas propensity scores model relationships between the confounders and treatment status" (Schafer & Kang, 2008, p. 280).

Multiple regression is a data analysis technique in which an outcome is predicted by a model containing two or more predictor variables (Field, 2013, p. 880; Warner, 2013, pp. 547–610). Multiple regression using *k* predictor variables is expressed as:

$$\hat{Y} = b_0 + b_1X_1 + b_2X_2 + \ldots + b_kX_k$$

(Warner, 2013, p. 547). In the present study, the equation becomes:

$$\hat{Y} = b_0 + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5X_5 + b_6X_6 + b_7X_7$$

Here, Y represents the dependent/outcome variable *prison misconduct*, X_1 represents the independent/predictor variable *cohort* (which will be dummy coded, with 1 = treatment cohort, and 0 = comparison cohort), X_2 represents the control/predictor variable *age*, X_3 represents the control/predictor variable *length of time spent incarcerated on current sentence*, X_4 represents the control/predictor variable *LS/CMI risk score*, X_5 represents the control/predictor variable *prior major rule violations in 2011-2012*, X_6 represents the control/predictor variable *prior minor rule violations in 2011-2012*, and X_7 represents the control/predictor variable *length of time spent in disciplinary segregation in 2011-2012*. The b_1 slope represents the predicted change in Y for a one-unit change in cohort membership (X_1), controlling for *age* (X_2), *length of time spent incarcerated* (X_3), *LS/CMI risk score* (X_4), *prior major rule violations in 2011-2012* (X_5), *prior minor rule violations in 2011-2012* (X_6), and *length of time spent in disciplinary segregation in 2011-2012* (X_7) (Warner, 2013, p. 547).

Multiple regression can be used to assess the ability of a variable to predict the outcome, while at the same time controlling for other variables (Warner, 2013, p. 548). In other words, multiple regression can be used to partial-out the predictive ability of a variable of interest while excluding the influence of other observed variables (Warner, 2013, Chapter 14). Therefore, multiple regression is a statistical technique that can be utilized to isolate and better understand the impact of a treatment variable while simultaneously controlling for other extraneous variables (Warner, 2013, pp. 547–610). Discovering that a treatment variable is a significant predictor of the outcome while

controlling for other variables can suggest that the treatment variable may have a relationship with the outcome, although the inference is a limited one (Warner, 2013, p. 556).

There are three different types of multiple regression: (1) standard or simultaneous regression, (2) sequential or hierarchical regression, and (3) statistical regression (Field, 2013, pp. 321–322; Warner, 2013, p. 559). Their differences center on how the variables are entered into the model (Field, 2013, p. 321). In the standard or simultaneous regression method, each of the predictor variables is entered at the same time (Field, 2013, p. 322; Warner, 2013, p. 559). Of the three methods, simultaneous regression is often the preferred method, although hierarchical regression may be preferable in specific circumstances (Field, 2013, pp. 322–324; Warner, 2013, pp. 560–564). The advantage of simultaneous regression is that when compared to the other two methods it is the simplest method, it typically provides a more conservative estimate of the impact of each predictor variable, and the variance explained by each predictor variable is unique to that specific predictor variable (i.e., the variance attributed to one predictor variable will not overlap with the variances attributed to other predictor variables) (Warner, 2013, pp. 560–561). In contrast, the variance attributed to a predictor variable as a result of hierarchical or statistical regression may be the result of an arbitrary process; due to how the variables were entered into the model, the variance attributed to the X_1 predictor variable might also be explained by the X_2 predictor variable, and hence such variance would not be unique to the X_1 predictor variable (Warner, 2013, p. 561).

The timeframe for the study spans 48 months. The first 24 months (the years 2011–2012) were for categorization purposes, in that the participants were selected for either the treatment cohort or the comparison cohort based upon their behavior during those two years. The next 24 months (the years 2013–2014) comprised the follow-up period, from which the rule violation rate of the two cohorts was compared and contrasted in order to test the hypotheses. Walters (2007) explained that "prior experience suggest[s]" that when examining institutional (i.e., prison) adjustment, "a 2-year follow-up offers the best balance in terms of maximizing the number of IRs [institutional reports, e.g., rule violation convictions] available for analysis while minimizing the number of participants lost to analysis because of release" (p. 73).

Summary and Conclusions

The research on deterrence theory indicates that it still remains a viable criminal justice theory, but the evidence supporting it is moderate at best. Furthermore, the research suggests, for the purposes of deterrence, that the certainty of punishment may be more important than the severity of punishment or the swiftness of punishment. The weight of the extant research on prison isolation and segregation indicates that it can have negative psychological and physiological effects on inmates, although these effects might only be associated with longer periods of isolation. However, inmates who experience disciplinary segregation may be at risk of suffering from those negative consequences. The research that has been performed regarding prison segregation and its relationship with deterrence appears inconclusive thus far and has not directly examined the deterrent effect of disciplinary segregation, so it is unclear whether the experience of prison

segregation (especially disciplinary segregation) has a deterrent effect on inmate prison misconduct. The empirical research on variables associated with prison misconduct has revealed that age, gender, and length of time incarcerated on current sentence are strong predictors of prison misconduct. In addition, the research on the LS scales indicates that the LS/CMI section 1 total score should serve as a useful predictor of prison misconduct as well. Furthermore, research suggests that prior prison rule violations may also be a useful predictor of subsequent prison inmate misconduct.

The present study intended to fill the current gap in the literature by investigating whether the practice of disciplinary segregation in the Oregon DOC system has a deterrent, criminogenic, or null effect on those subjected to it. It attempted to do so by evaluating the prison misconduct rates of two cohorts. The first cohort was comprised of inmates who spent time in disciplinary segregation during the years 2011 or 2012. The second cohort was comprised of those inmates who, as of January 1, 2013, had not spent any time in disciplinary segregation. Then, the prison misconduct data of the two cohorts were examined for the years 2013 and 2014. This was accomplished through the use of multiple regression, controlling for the effects of the key variables gender, age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation in 2011-2012.

Chapter 3: Research Method

Introduction

The purpose of this quantitative retrospective observational study was to examine the effectiveness of disciplinary segregation in deterring prison misconduct on those subjected to it within the Oregon DOC system. The prison rule violation rates of two cohorts were compared using multiple regression analysis. The treatment cohort was comprised of prison inmates who had spent time in disciplinary segregation during the years 2011–2012. The comparison cohort was comprised of prison inmates who had not, as of January 1, 2013, spent any time in disciplinary segregation. Control variables were used to help isolate the effect of the experience of disciplinary segregation on prison inmate misconduct. Three models were tested using multiple regression analysis, each with the outcome variable operationalized in a different way: overall total rule violations in 2013-2014, total major rule violations in 2013-2014, and total minor rule violations in 2013-2014.

Research Design and Rationale

The primary research question that this study was designed to address was whether or not deterrence theory explains the relationship between disciplinary segregation and prison misconduct among inmates. The study was additionally designed to address whether or not disciplinary segregation has a criminogenic effect on inmates who are subjected to it, and alternatively whether disciplinary segregation has no significant effect on inmates who are subjected to it. The ideal method for investigating such questions is a true randomized experiment (Nagin, Cullen, & Jonson, 2009, p. 131;

Zimring & Hawkins, 1973, p. 253). However, in this study randomly assigning participants to treatment and control groups would have raised ethical concerns, and was not feasible for this specific study. Instead, this study relied on administrative and archival (i.e., secondary) data. Therefore, I selected a retrospective observational study using multiple regression analysis to address the research questions.

A retrospective observational study was especially appropriate for this dissertation because random selection was not feasible and prisoners are considered a vulnerable population. A retrospective observational study is a method that can be used to compare two preformed groups, where the formation of the groups is not amenable to randomization (Rudestam & Newton, 2007, p. 28; Nagin, Cullen, & Jonson, 2009; Rosenbaum, 1989). Random assignment, if utilized for this study, could have possibly been structured in two ways: (1) randomly assigning inmates to disciplinary segregation without requiring a justification, simply to give the inmates a "taste" of what they could be subjected to if they committed a rule violation, or (2) subsequent to being found guilty of committing a prison rule violation, and pursuant to a finding that their rule violation merited punishment in the form of disciplinary segregation, the inmate would then be randomly assigned disciplinary segregation as punishment. Both of these forms of random selection would result in unfair treatment of the inmates, and such treatment may be unethical. Furthermore, such a random selection process could violate the rights of the inmates. The use of a retrospective observational study avoided these ethical dilemmas and potential infringements upon the rights of the inmates.

Inmates are members of a vulnerable population (Creswell, 2009, p. 89). The study of vulnerable populations is accompanied by heightened ethical concerns, such as whether any actual or perceived coercion may affect the individuals' consent to participate in the study (Creswell, 2009, p. 89; Frankfort-Nachmias & Nachmias, 2008, pp. 72–76). Ethical concerns associated with vulnerable populations can pose a barrier to obtaining approval for a study. Since a retrospective observational study such as this one is limited to collecting archival and administrative data, it avoids and minimizes many of the ethical concerns that would be associated with other different types of studies done in the same context. For example, studies that employ the administration of surveys to the prisoners, or those that involve interviewing the prisoners, would be accompanied by stronger ethical concerns than those that accompany the use of administrative and archival data.

The use of a retrospective observational study enabled this research to be performed without having to utilize a random selection process when generating the sample and placing the participants into the treatment and comparison cohorts. For this study, two cohorts were formed, a treatment cohort and a comparison cohort. Both cohorts were limited to those who were incarcerated as of January 1, 2011, and who remained incarcerated through December 31, 2014. In addition, the participants were limited to those who were continuously incarcerated (during that time period) at a facility (or facilities) that had at least a medium-security component. That is, inmates who spent any time incarcerated in a minimum-security-only facility during 2011 through 2014 were excluded from the study. The treatment cohort was comprised of all those inmates

who spent time in disciplinary segregation during the years 2011 and/or 2012. The inmates comprising the comparison cohort were drawn from those inmates who, as of January 1, 2013, had not spent any time in disciplinary segregation. Then, the data were analyzed using multiple regression in order to determine whether the experience of disciplinary segregation significantly predicted subsequent prison misconduct for the years 2013 and 2014.

A retrospective observational study using multiple regression analyses provided an avenue through which archival and administrative data were analyzed and controlled for in a manner that addressed the research questions. After generating these two cohorts, I used multiple regression analyses to examine the predictive ability of the experience of disciplinary segregation with regard to prison misconduct for 2013 and 2014. Specifically, standard/simultaneous multiple regression was employed. This is because, of the three methods of multiple regression (standard/simultaneous regression, sequential/hierarchical regression, and statistical regression) the standard/simultaneous regression method is the simplest, it typically supplies a more conservative estimate of the impact of each predictor variable, and the variance attributed to each predictor variable will not overlap with the variances of the other predictor variables (Warner, 2013, pp. 560–561). The multiple regression analysis method was used to control for the influence of key variables on the rule violation rate. Controlling for key variables helped isolate the unique influence of disciplinary segregation on the behavior of the prisoners. These key control variables used in the multiple regression analyses were: age, length of time incarcerated on current sentence, LS/CMI risk score, prior major rule violations in

2011-2012, prior minor rule violations in 2011-2012, and time spent in disciplinary segregation in 2011-2012. In addition, gender served as another control variable, in that only males were selected as participants in this study. These control variables were chosen based upon prior research indicating that they are related to prison misconduct. In essence, controlling for these variables using multiple regression analyses enhanced the comparability of the two cohorts.

Variables

Several variables were involved in this study. In general, the dependent/outcome variable was *prison misconduct*, the independent/predictor variable was *cohort* (i.e., the grouping variable), the control/predictor variables were *age*, *length of time served on current sentence*, *LS/CMI risk score*, *prior prison misconduct in 2011-2012*, and *length of time spent in disciplinary segregation in 2011-2012*. In addition, *gender* also served as a control variable. The specific definitions and the operationalization of each of these variables are described below.

The dependent/outcome variable for this study was *prison misconduct*, defined as an official finding by the prison authorities of an inmate rule violation in 2013-2014. This could be operationalized in several different ways. First, it could be defined as the overall total of official findings of rule violations in 2013-2014. It could also be further broken down into the six different levels of rule violations. Additionally, it could also be defined as total official findings of major rule violations (levels 1-4), and as total official findings of minor rule violations (levels 5 and 6). These different measurement definitions would contribute to developing both a broad overview of the relationship

between the experience of disciplinary segregation and prison misconduct, and to a deeper and more detailed understanding of that relationship, but these methods of measurement also come with drawbacks. Running multiple significance tests requires lowering the level of significance set for each of the individual models (Warner, 2013, pp. 89–99, 101, 565). One method of finding the appropriate level of significance for individual models is the Bonferroni correction method. The equation for the Bonferroni correction method is:

$$PC_{\alpha} = EW_{\alpha}/k$$

where EW_{α} is the α-level for the whole study (here, $\alpha = .05$), "and k is the number of significance tests performed in the entire [] study" (Warner, 2013, pp. 89–99, 101, 565). Applying this method reveals that it is important to limit the number of significance tests within the study, in order that the level of significance set for each individual model is not set so low as to make the study highly susceptible to rejecting the null hypothesis even when the null hypothesis is incorrect (Type II error; Warner, 2013, p. 1122). For example, if six significance tests were run (one for each of the six rule-violation-level outcome variables), then the significance level for each model should be set at .008 using the Bonferroni correction, in order to keep the overall experiment-wise significance level at the standard .05 α-level (Field, 2013, pp. 67, 70, 870; Warner, 2013, pp. 89, 92, 98–99). However, if three significance tests were run, then the significance level for each model should be set at .017 using the Bonferroni correction. The appropriate resolution appears to be a compromise where a limited number of models are employed. Therefore,

for this study, three models were employed, each using a different definition for the outcome variable *prison misconduct*: (1) overall total rule violation findings in 2013-2014, (2) total major rule violation findings in 2013-2014, and (3) total minor rule violation findings in 2013-2014.

Cohort was a dummied variable, with 1 = treatment cohort, and 0 = the comparison cohort. *Age* was a continuous variable measured as the participants' chronological (biological) age in years as of January 1, 2011. *Length of time spent incarcerated on current sentence* was a continuous variable, measured by the number of days the participant had spent incarcerated on the current sentence as of January 1, 2011. *LS/CMI risk score* was a continuous variable, and measured as the participant's numerical total score from section 1 of the LS/CMI. *Gender* also served as a control variable, in that the sample was limited to only male participants.

Length of time spent in disciplinary segregation during 2011-2012 was a continuous variable and was measured as the number of days the participant spent in disciplinary segregation in 2011 through 2012. In addition, the data provided by the Oregon DOC on this variable included within its parameters all the days the participant continuously spent in disciplinary segregation, as long as one of those days was within the treatment window (the treatment window being January 1, 2011, through December 31, 2012). In such an instance, the days would be counted from the date the participant moved out of disciplinary segregation (or December 31, 2012) minus the date the participant moved into disciplinary segregation; plus any other additional days spent by the participant in disciplinary segregation in 2011 and 2012. If a participant was in

disciplinary segregation in 2011 and/or 2012, and did not move out of disciplinary segregation until January 1, 2013 or later, those days after December 31, 2012 were not counted; in other words, December 31, 2012, was the stop-date for counting the number of days the participant spent in disciplinary segregation.

Prior rule violations was a continuous variable and could have been operationalized several different ways. Prior rule violations could be operationalized by creating a separate control variable for each level of rule violation (six levels total). Prior rule violations could also be collapsed from those six levels/variables into two variables: one variable for major rule violations (levels 1 through 4), and one variable for minor rule violations (levels 5 and 6). Model fit was used to determine which operationalization to apply in this study.

Methodology

Population

The population for the study is the group of individuals that the study is aimed at investigating, and to which the results of the study will be generalized (Trochim, 2006). For this study, the population can be described along three different levels. First, on a broad level, the population for the study was the United States prison population. On a more narrow level, the population for the study was the Oregon prison population. More specifically, the population for this study consisted of all the male prisoners that were housed within the Oregon DOC's prison facilities during the years 2011 through 2014, and who were incarcerated throughout that whole time in a facility or facilities that had a

medium-security component. This was also the specific sampling frame from which the sample was drawn, per Trochim (2006).

Only male inmates were used for this study, in part because the Oregon DOC had a significantly larger proportion of male inmates than female inmates incarcerated during the period of study, and because gender can serve as a predictor of prison misconduct (Celinska & Sung, 2014, p. 227; Craddock, 1996, para. 19; Harer & Langan, 2001, pp. 521–523; Haun, 2007, pp. 150–152, 218). The average total combined monthly male prison population for the five Oregon DOC prison facilities used in this study, for the years 2011 through 2014, was approximately 9,164 total inmates (Oregon DOC, n.d.a). The average monthly Oregon DOC female prison population during those years was only approximately 1,192 inmates (Oregon DOC, n.d.a). Since gender can serve as a predictor of prison misconduct, the influence of gender on prison misconduct should be partialed-out from the influence of disciplinary segregation. Also, female inmates are not housed within the five prison facilities that will be used in this study. There is only one female facility with a medium-security component (Coffee Creek Correctional Facility), and its male population is very small; its average monthly male population for the years 2012 through 2014 was between approximately zero to three inmates (Oregon DOC, n.d.a). The average monthly female population for that facility for the years 2011 through 2014 was approximately only 655 inmates (Oregon DOC, n.d.a).

The five prison facilities from which the two cohorts were drawn were the Oregon State Penitentiary (OSP), the Eastern Oregon Correctional Institute (EOCI), the Oregon State Correctional Institute (OSCI), the Snake River Correctional Institute (SRCI), and

the Two Rivers Correctional Institute (TRCI). These facilities were selected because they were either a medium-security facility, or a multi-security facility with a medium-security component (Oregon DOC, n.d.b). Inmates who spent time during the study period in a strictly minimum-security facility or facilities (i.e., those facilities that did not also have a medium-security component) were excluded from the study. This is because the disciplinary segregation unit at a strictly minimum-security prison facility is structurally different from a disciplinary segregation unit at a medium-security or multi-security facility. At a minimum-security facility, the disciplinary segregation unit resembles more of a holding cell unit, with only a few cells and the inmates generally do not serve significant amounts of time in disciplinary segregation. When longer disciplinary segregation punishments are imposed, the inmates are sent to serve the punishment at a facility with a higher-rated level of security (i.e., a medium or multi-security facility).

Sampling

For this study, the sample selection process utilized a purposive sampling strategy. Purposive sampling entails selecting the sampling units with the intent to fulfill a specific purpose (Trochim, 2006). Here, the intent of the sampling process was to generate two cohorts that were representative of the population and which could be studied in order to better understand the effect of the experience of disciplinary segregation on inmate behavior within the Oregon prison system. In order to achieve this purpose, the participants were drawn from the Oregon DOC prison populations of those facilities with a medium-security component. The participants were limited to those who

were incarcerated as of January 1, 2011, and who remained incarcerated through December 31, 2014. From this sampling frame, two cohorts were formed. Specifically, the treatment cohort was comprised of all of those inmates who spent time in disciplinary segregation during the years 2011 and 2012. The comparison cohort was comprised of all of those inmates who had not, as of January 1, 2013, spent any time in disciplinary segregation. This could also be considered a "convenience" sample (Frankfort-Nachmias & Nachmias, 2008, p. 168), since the sampling frame was selected in part based upon its availability.

Data Collection

The data that were used in this study was archival and administrative data held by the Oregon DOC. The study underwent an application and approval process with the Oregon DOC, and official approval for the study was received. Once the Oregon DOC and the Walden University Institutional Review Board (IRB) approved the study, data were then gathered on all the Oregon DOC inmates who were incarcerated from January 1, 2011, through December 31, 2014 in a facility or facilities with a medium-security component. In particular, data regarding the inmates' gender, age, length of time spent incarcerated on current sentence, LS/CMI section 1 risk score, prison misconduct history in 2011-2012, and their disciplinary segregation history were gathered. The Oregon DOC Research and Evaluation Unit helped me collect and organize the necessary data. The Research and Evaluation Unit developed and ran computer codes that were then deployed to collect the data (J. Duncan, personal communication, January 28, 2015). After compiling the data, the Research and Evaluation Unit then delivered it to me in a

Microsoft Excel spreadsheet format, which I then uploaded into the SPSS computer program.

Data Screening and Cleaning

Prior to using statistical analysis techniques to examine the data, steps were taken in SPSS to screen and clean the data. First, the data were examined for any missing values. Cases with missing data were dealt with using listwise deletion (Field, 2013, pp. 187, 332; Tabachnick & Fidell, 2014, pp. 97, 105, 160; Warner, 2013, p. 134). Second, frequency tables were used to get an overview of the data and determine the number of participants in the treatment and comparison cohorts (Warner, 2013, p. 135). Third, histograms depicting each of the variables were generated in order to check for outliers and assess whether the assumption of normally distributed errors had been violated (Field, 2013, pp. 176–179, 311, 349; Warner, 2013, pp. 142–147, 573).

After examining the histograms, additional steps were taken to check for outliers. The next step was to generate boxplots that were used to examine the data for outliers (Field, 2013, pp. 176–178; Warner, 2013, pp. 153–156). This was followed by an additional step in which standardized residuals (*z*- scores) were generated and examined (Field, 2013, pp. 179–180, 304–306). In a sample where the data are normally distributed, no more than 5% of the *z*-scores should be greater than the absolute value of 1.96 (i.e., "potential outliers"), no more than 1% of the *z*-scores should be greater than the absolute value of 2.58 ("probable outliers"), and no more than 0.1% of the *z*-scores should be greater than the absolute value of 3.29 (i.e., "extreme outliers") (Field, 2013, pp. 179–180, 306; Tabachnick & Fidell, 2014, pp. 105–107). These steps identified any

outliers, and such values were then deleted. Where more than 0.1% of the cases had a *z*-score with an absolute value greater than 3.29, then those values were deleted, beginning with the most extreme, until only 0.1% of the *z*-scores had an absolute value greater than 3.29 (Field, 2013, p. 306; Warner, 2013, p. 153). Where more than 1% of the cases had a *z*-score with an absolute value greater than 2.58, then those values were deleted, until only 1% of the *z*-scores had an absolute value greater than 2.58 (Field, 2013, p. 306; Warner, 2013, p. 153). Where more than 5% of the cases had a *z*-score with an absolute value greater than 1.96, then those values were deleted, until only 5% of the *z*-scores had an absolute value greater than 1.96 (Field, 2013, p. 306; Warner, 2013, p. 153). Since outliers were detected, the multiple regression analyses were run twice; once with the outlier cases deleted, and once with the outlier cases included (Warner, 2013, pp. 156–157). This was done in order to facilitate a transparent analysis of the data and develop a better understanding of the effect of the outliers, if any, on the analysis (Warner, 2013, pp. 156–157).

Next, the assumption of normally distributed errors was further assessed in addition to the examination of the initial histograms. The data were divided into two groups, where the data pertaining to the treatment cohort were delineated from the data pertaining to the comparison cohort (Field, 2013, pp. 188–189). A descriptive statistics table was then generated, which provided values that described the skewness and kurtosis of the data (Field, 2013, pp. 181–188; Warner, 2013, pp. 150–153). In addition, the data were examined using the Shapiro-Wilk test and the Kolmogorov-Smirnov test, which if significant ($\alpha = .05$) would indicate that the sample is not normally distributed (Field,

2013, pp. 184–185, 191). Most importantly to the determination of whether errors were normally distributed, histograms depicting the dependent/outcome variables and the standardized residuals were evaluated, and normal probability-probability plots were inspected as well (Field, 2013, pp. 179–182, 311, 329–331, 348–351; Williams, Grajales, & Kurkiewicz, 2013). Since these examinations indicated that the assumption of normality was violated, the bootstrapping method was used in the multiple regression analyses in order to correct for the violation (Field, 2013, pp. 198–200, 311, 350–352).

Multiple regression also assumes homogeneity of variances (homoscedasticity) when comparing the means of quantitative variables across groups (Field, 2013, pp. 172–176, 311; Warner, 2013, pp. 163–164, 573). First, the data were checked for homoscedasticity through the use of scatterplots that depicted the standardized predicted values and the standardized residuals (Field, 2013, pp. 192–193, 330; Warner, 2013, p. 573). A scatterplot will indicate heteroscedasticity if the values form the shape of a funnel (Field, 2013, pp. 192, 348–349). In addition, the Levene's test ($\alpha = .05$) was used to further determine whether the assumption had been violated (Field, 2013, pp. 193–195; Warner, 2013, pp. 163–164). Since the assumption was violated, the bootstrapping method was used in the multiple regression analyses in order to correct for the violation (Field, 2013, pp. 198–202, 350–352).

Multiple regression also requires linearity, in that the outcome variable should be linearly related to the predictor variables (Field, 2013, pp. 167–168, 192; Warner, 2013, p. 573). Checking the data for linear relations was done by using the same scatterplots used to check for homoscedasticity (Field, 2013, pp. 192, 326; Warner, 2013, p. 573). A

scatterplot will indicate that the assumption of linearity has been violated if the values form the shape of a curve (Field, 2013, p. 192; Warner, 2013, pp. 168–172). If the assumption of linearity had been violated, then nonparametric tests would have been used to evaluate the data instead of multiple regression analysis. The nonparametric tests that would have been used were the Mann-Whitney test and the Wilcoxon's rank-sum test (Field, 2013, pp. 219, 228; Mann & Whitney, 1947; Wilcoxon, 1945).

Independence of observations is another assumption underlying the multiple regression analysis (Field, 2013, pp. 176, 311; Warner, 2013, pp. 163, 573). The data were examined for a violation of this assumption using the Durbin-Watson test (Field, 2013, pp. 176, 311, 329, 337; Durbin & Watson, 1951; Tabachnick & Fidell, 2014, p. 164).

Multicollinearity is another issue that must be addressed. Strong collinearity (correlation) between two or more of the predictor variables can interfere with the analysis (Field, 2013, pp. 312, 324). The variance inflation factor (VIF) and the tolerance statistic were used to assess multicollinearity (Field, 2013, pp. 325, 329, 342). If a VIF value is greater than 10, and/or a tolerance statistic is less than 0.2, it is likely that the multicollinearity assumption has been violated (Field, 2013, pp. 325, 342, 795). If there had been strong collinearity between two or more predictor variables, then one of the variables would have been removed from the study, or principal component analysis (PCA) would have been used to combine those highly-correlated variables into one predictor variable (Field, 2013, pp. 343, 666, 674, 797).

Data Analysis

After the data had gone through the screening and cleaning process, the data were analyzed with a statistical analysis technique using SPSS. Since the data met the necessary assumptions, multiple regression analysis was the statistical technique used to examine the data. Specifically, the standard/simultaneous regression method with bootstrapping was employed to analyze the data.

The multiple regression analyses were performed with the purpose of analyzing the ability of the independent/predictor variable *cohort* (the grouping variable dividing the participants into the treatment and comparison cohorts) to predict prison misconduct. The aim of this analysis was to investigate the following three research questions and three null hypotheses:

RQ1: Does deterrence theory explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among inmates, after controlling for the effects of gender, age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and time spent in disciplinary segregation in 2011-2012?

RQ2: Does the experience of disciplinary segregation reduce subsequent prison misconduct among inmates who are subjected to it, and if so, to what extent?

RQ3: Does the experience of disciplinary segregation have a criminogenic effect on inmates who are subjected to it?

NH1: There is no significant difference between the treatment cohort and the comparison cohort on prison misconduct.

NH2: The experience of disciplinary segregation does not significantly reduce subsequent prison misconduct among inmates who are subjected to it.

NH3: The experience of disciplinary segregation does not have a significant criminogenic effect on inmates who are subjected to it.

In essence, each of these hypotheses asks whether the experience of disciplinary segregation significantly predicts whether an inmate will engage in prison misconduct. Since multiple regression analysis examines the predictive ability of a variable, the three null hypotheses must be transformed into hypotheses that use that language of prediction. Therefore, the multiple regression analyses directly tested the following null hypotheses:

NH1: The independent/predictor variable *cohort* does not significantly predict prison misconduct.

NH2: The level of the independent/predictor variable *cohort* that indicates membership in the cohort comprised of those who have spent time in disciplinary segregation (the treatment cohort) does not significantly negatively predict prison misconduct.

NH3: The level of the independent/predictor variable *cohort* that indicates membership in the cohort comprised of those who have spent time in disciplinary segregation (the treatment cohort) does not significantly positively predict prison misconduct.

For this study, three models were employed, each using a different definition for the outcome variable *prison misconduct*: (1) overall total rule violation findings in 2013-2014, (2) total major rule violation findings in 2013-2014, and (3) total minor rule violation findings in 2013-2014. For each of these models, a specific α-level was not preset; rather, exact *p* values are reported, and the results were to be discussed in terms of

whether they were significant at the .05 α-level, and additionally whether they were significant at the .017 α-level, and then additionally whether they were significant at the .008 α-level. These alpha-levels were obtained using the Bonferroni correction method explained earlier in this chapter. The .017 α-level was based on the fact that three total models were used in this study. The .008 α-level was based on the fact that six total multiple regression analyses were run, each model being run twice (i.e., once with the outliers excluded and then once with the full data set). Where $p < .05$, the results were described simply as nonsignificant, without the additional discussion that the results were also clearly not significant at the even lower levels of .017 and .008.

If the independent/predictor variable *cohort* is significantly predictive of prison misconduct, then NH1 should be rejected. If *cohort* is significantly predictive of prison misconduct and its slope is negative, this would indicate that higher scores on *cohort* (i.e., being in the treatment cohort) predicted lower rates of misconduct – and therefore NH1 and NH2 should be rejected (Field, 2013, pp. 319, 338; Warner, 2013, pp. 567–568, 578). If *cohort* is significantly predictive of prison misconduct and its slope is positive, this would indicate that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher rates of prison misconduct – and therefore NH1 and NH3 should be rejected (Field, 2013, pp. 319, 338; Warner, 2013, pp. 567–568, 578). If the independent/predictor variable *cohort* is not significantly predictive of prison misconduct (either negatively or positively), then none of the null hypotheses should be rejected.

Limiting the participants to males and excluding females helped control for the effects of gender on prison misconduct. Furthermore, limiting the participants to only

those inmates incarcerated in a facility with a medium-security component helped control for and exclude the possible effects of being incarcerated in a minimum-security facility where the disciplinary segregation unit is qualitatively different than the disciplinary segregation units in facilities with a medium-security component. The multiple regression analyses helped control for the effects of age, length of time spent incarcerated on current sentence, LS/CMI risk score, and prior rule violation history, since prior research indicates that these variables can influence prison misconduct. In addition, the multiple regression analysis also included the control/predictor variable length of time spent in disciplinary segregation in 2011-2012. All of these measures helped increase the comparability of the two cohorts and isolate the effect of the experience of disciplinary segregation on subsequent prison inmate misconduct.

Threats to Validity

There were several apparent threats to the study's validity, particularly threats to its internal validity and external validity. First, the internal validity of the study was threatened by a possible selection bias. Internal validity refers to the ability to draw a causal connection between two or more variables based upon the results of the study (Creswell, 2009, p. 162; Warner, 2013, pp. 16–20, 1093). Selection bias occurs when members of a group have a predisposition towards a certain outcome (Creswell, 2009, p. 163). Selection bias is a common issue in retrospective observational studies (e.g., Campbell & Stanley, 1963, p. 12). Here, the cohort that was comprised of inmates who had previously spent time in disciplinary segregation may have been more predisposed to committing rule violations, whereas the cohort that was comprised inmates who, as of

January 1, 2013, had not been sent to disciplinary segregation may have been more predisposed to not committing prison rule violations. Either of these circumstances would compromise the accuracy of any inferences that could be drawn from the results. In order to reduce the chance of selection bias interfering with the study, the statistical technique of multiple regression analysis was used to partial out the impact of certain variables that research has shown to influence prison misconduct. These measures should have increased the comparability of the two cohorts, helped isolate the effect of the experience of disciplinary segregation, and reduced the threat to internal validity posed by selection bias.

External validity refers to the ability of the results of the study to be generalized to a broader group or environment beyond the study's specific sample and circumstances (Creswell, 2009, p. 162; Warner, 2013, pp. 17–20, 1086). Since this study utilized administrative/archival data, as opposed to being conducted in a tightly controlled laboratory setting, the external validity of the study should be relatively strong. However, the sample selection process may have resulted in reducing the external validity of the study. That is, limiting the sample to only those inmates incarcerated throughout the entire study period (2011 through 2014) within a facility containing a medium-security component may have decreased the representativeness of the sample with regard to the whole Oregon prison population. However, keeping that limitation on the sample in place was important for other reasons, including issues related internal validity and allotting a timeframe long enough to collect enough data to compile meaningful results.

Ethical Procedures

It is important to ensure that the expected benefits of a study outweigh any potential risks, particularly risks to human participants. It is also important that any potential risks are minimized. To that end, this study went through Walden University's IRB process, and no data collection occurred until after this study had received official IRB approval. The IRB approval number for this study was 04-20-15-0366739. Prior to receiving IRB approval, the study also underwent a formal application process with the Oregon DOC, after which the Oregon DOC granted official approval to gather the data and perform the study.

This study used de-identified archival/administrative data from the years 2011 through 2014 provided by the Oregon DOC. The data were anonymous, in that the data set that I received from the Oregon DOC did not contain any of the participants' personal identifying information, other than the information directly related to the variables that were being studied (e.g., age, gender, prison misconduct). In addition, measures were taken to keep the data secure. These measures included storing the data on a single password-protected personal computer and an external hard drive. The external hard drive was kept in a locked fire-resistant safe, except for the times when it was briefly removed to back-up the computer. The safe and the computer remained in a limited-access home office, and only I had access to the computer and the safe. After the study had been completed, the data were deleted from the computer, but the data remains stored on the external hard drive locked in the safe. The data set will not be shared with anyone unless the Oregon DOC grants specific consent.

Summary

This study utilized a quantitative retrospective observational research design. First, archival and administrative data were collected with the help of the Oregon DOC. Then, using SPSS, the data were screened and cleaned. Next, multiple regression analysis was used to analyze the data, in order to determine whether the experience of disciplinary segregation significantly predicted subsequent prison misconduct, and if so then whether it significantly negatively or positively predicted prison misconduct. The intent of this examination was to help reveal the effect that the experience of disciplinary segregation may have on subsequent prison inmate misconduct.

Chapter 4: Results

Introduction

The purpose of this quantitative study was to examine the effectiveness of disciplinary segregation in deterring prison inmate misconduct within the Oregon DOC prison system. This retrospective observational study was specifically designed to test the theory of deterrence that relates disciplinary segregation to prison misconduct. The following three research questions were addressed by this study:

RQ1: Does deterrence theory explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among inmates, after controlling for the effects of gender, age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation in 2011-2012?

RQ2: Does the experience of disciplinary segregation reduce subsequent prison misconduct among inmates who are subjected to it, and if so, to what extent?

RQ3: Does the experience of disciplinary segregation have a criminogenic effect on inmates who are subjected to it?

In order to address these questions, multiple regression analyses were used to test the following null hypotheses:

NH1: The independent/predictor variable *cohort* does not significantly predict prison misconduct.

NH2: The level of the independent/predictor variable *cohort* that indicates membership in the cohort comprised of those who have spent time in disciplinary segregation (the treatment cohort) does not significantly negatively predict prison misconduct.

NH3: The level of the independent/predictor variable *cohort* that indicates membership in the cohort comprised of those who have spent time in disciplinary segregation (the treatment cohort) does not significantly positively predict prison misconduct.

Three separate analyses were performed. The primary analysis featured overall total rule violations as the dependent/outcome variable. Then, two more analyses were performed in order to develop a more refined understanding of the effect of disciplinary segregation on subsequent prison misconduct. The second analysis featured total major rule violations as the outcome variable. The third analysis featured total minor rule violations as the outcome variable.

This chapter begins with a brief description of the characteristics of the sample. Then, two different models are examined with regard to the control/predictor variable prior prison misconduct in 2011-2012, in order to determine which was the most appropriate to apply in this study. This is followed by a discussion of the data in relation to the necessary assumptions. Lastly, the results of the multiple regression analyses are reported.

Population and Sample

The population for the study consisted of 3,375 inmates that had been incarcerated from January 1, 2011 through December 31, 2014 in an Oregon DOC facility or facilities that had a medium-security component. The descriptive statistics for this population are set out in Tables 1 and 2; of the 3,375 participants, one individual was excluded from the study due to missing data on a few key variables, and 1,263 were excluded from the study due to meeting neither the comparison cohort criteria nor the treatment cohort criteria. These 1,263 individuals had spent time in disciplinary segregation at some point prior to January 1, 2011 (thus excluding them from the comparison cohort) and did not serve any time in disciplinary segregation during the years 2011 or 2012 (thus excluding them from the treatment cohort). Of the remaining 2,111 participants, 853 qualified for the comparison cohort and 1,258 qualified for the treatment cohort. However, of those 2,111 participants, only 228 had scores on the LS/CMI. These 228 participants comprised the final sample used for the study. The descriptive statistics for this sample are contained in Tables 3 and 4.

Table 1

Descriptive Statistics for the Population

	Age	Length of Time Spent on Current Sentence	LS/CMI Score	Length of Time Spent in DSU in 2011-2012
n Valid	3374	3374	307	3374
n Missing	1	1	3068	1
M	41.646	2558.406	24.023	43.996
Mdn	41.000	1760.500	24.000	0.000
SD	12.5468	2487.6625	7.6741	119.1519
Minimum	18.0	4.0	6.0	0.0
Maximum	81.0	14165.0	40.0	2421.0

Table 2

Descriptive Statistics for the Population

	Total Major Rule Violations in 2011-2012	Total Minor Rule Violations in 2011-2012	Overall Total Rule Violations in 2013-2014	Total Major Rule Violations in 2013-2014	Total Minor Rule Violations in 2013-2014
n Valid	3375	3375	3375	3375	3375
n Missing	0	0	0	0	0
M	1.9446	0.2770	1.677	1.4607	0.2160
Mdn	0.0000	0.0000	0.000	0.0000	0.0000
SD	4.52028	0.94483	4.7312	4.36096	0.70386
Minimum	0.00	0.00	0.0	0.00	0.00
Maximum	58.00	19.00	116.0	111.00	9.00

Table 3

Descriptive Statistics for the Sample

	Age	Length of Time Spent on Current Sentence	LS/CMI Score	Length of Time Spent in DSU in 2011-2012
n Valid	228	228	228	228
n Missing	0	0	0	0
M	33.588	1120.496	24.877	130.939
Mdn	31.000	590.500	25.000	89.500
SD	10.2916	1334.8228	7.6580	143.4899
Minimum	18.0	46.0	6.0	0.0
Maximum	66.0	7270.0	40.0	806.0

Table 4

Descriptive Statistics for the Sample

	Total Major Rule Violations in 2011-2012	Total Minor Rule Violations in 2011-2012	Overall Total Rule Violations in 2013-2014	Total Major Rule Violations in 2013-2014	Total Minor Rule Violations in 2013-2014
n Valid	228	228	228	228	228
n Missing	0	0	0	0	0
M	6.5658	0.9649	5.193	4.5132	0.6798
Mdn	5.0000	0.0000	2.000	2.0000	0.0000
SD	7.05361	1.81005	7.9585	7.18079	1.37252
Minimum	0.00	0.00	0.0	0.00	0.00
Maximum	43.00	13.00	49.0	47.00	8.00

Model Fit

Assessment of the model fit was necessary in order to determine the appropriate method of controlling for prior rule violations and determining the best way to operationalize prior prison misconduct. I focused on two options for operationalizing prior prison misconduct. There were six different rule violation levels (levels of seriousness), with levels 1-4 being categorized as "major" violations, and levels 5-6 being categorized as "minor" violations (Or. Admin. R. 291-105-005 *et seq.*, exhibit 1, exhibit 2). Therefore, the prior rule violations could be applied as two control/predictor variables, with one variable being *prior major violations in 2011-2012*, and the other being *prior*

minor rule violations in 2011-2012. However, the prior rule violations could also have been applied with further specificity by being operationalized as six different control/predictor variables, with one control/predictor variable for each level of misconduct.

I ran regression analyses on the data in order to decide which version of operationalization to use. The specific data used for this analysis utilized the overall total rule violations in 2013-2014 as the outcome variable, with no data points deleted (i.e., outliers were not deleted). When including the six different rule violation level predictor variables (for a total of 11 predictors) in the model, the adjusted $R^2 = .449$, $F = 17.821$, $p < .001$. When those six rule violation level predictor variables were collapsed into just two variables, (one for major violations, one for minor violations, for a total of seven predictors), the adjusted $R^2 = .455$, $F = 28.059$, $p < .001$. Since the adjusted R^2 for the seven-predictor model was higher than the adjusted R^2 for the 11-predictor model, it appeared that the seven-predictor model was a better "fit." Therefore, the prior rule violations were operationalized as two predictor variables: *prior major misconducts in 2011-2012* and *prior minor misconducts in 2011-2012*.

Assumptions

As outlined in Chapter 3, I examined the study data to determine whether any of the necessary assumptions were violated and to see if any remedies or alterations to the data analysis process were necessary. First, the data were examined for outliers. Then, the assumption of normally distributed errors was assessed. Next, the assumption of

homogeneity of variances was tested, which was then followed by an examination of the data for linearity. Independence of observations and multicollinearity were also assessed.

Outliers

The data were first examined for outliers. This was done through a review of the histograms and boxplots of each of the different variables, as well as the generation of standardized *z* scores. When examining the data for outliers, (1) no more than 5% of the *z* scores should be greater than the absolute value of 1.96, (2) no more than 1% of the *z* scores should be greater than the absolute value of 2.58, and (3) no more than 0.1% of the *z* scores should be greater than the absolute value of 3.29 (Field, 2013, p. 306). Applying these rules to the data within each of the variables meant that, since there were a total of 228 participants:

1. no more than eleven of the *z* scores should be greater than the absolute value of 1.96,
2. no more than two of the *z* scores should be greater than the absolute value of 2.58, and
3. zero of the *z* scores should be greater than the absolute value of 3.29.

Data points whose equivalent *z* scores were outside the necessary range were deleted. Frequency tables depicting the number and percentages of extreme outliers, probable outliers, and potential outliers within the data set are contained in Appendix A. When there were multiple data points outside the necessary range, data points were chosen for deletion based upon whether the case would be removed anyway from the analysis (e.g., the case was an extreme outlier for another variable), otherwise the highest score would

be deleted first. The purpose of this method was to limit the number of participants that would be eliminated from the study due to outliers.

For example, the variable *length of time spent in disciplinary segregation during 2011-2012* had three extreme outliers, and each of these data points were deleted (which later resulted in those participants being deleted when listwise deletion was used during the multiple regression analyses). The variable *prior major rule violations in 2011-2012* had three "probable" outliers. Of these three probable outliers (participants #138, #89, #226), one had to be deleted in order for the data to be within the necessary range. Since participant #138 was already going to be removed from the data set due to having an extreme outlier on the other variable *length of time spent in disciplinary segregation in 2011-2012,* his outlier data point for *prior major rule violations in 2011-2012* was chosen as the one to be deleted.

Table 5

Deleted Outliers

Predictor Variable	Total Extreme Outliers Deleted	Total Probable Outliers Deleted
Age	0	0
Length of time spent on current sentence	5	2
LS/CMI score	0	0
Length of time spent in DSU in 2011-2012	3	1
2011-2012 total major rule violations	2	1
2011-2012 total minor rule violations	4	0
2013-2014 overall total rule violations	7	0
2013-2014 total major rule violations	7	0
2013-2014 total minor rule violations	5	0

The process of screening for outliers revealed that there were outliers within the data, and some of these outliers were deleted from the data set (as described above). Because of this, the multiple regression analysis was run twice with each of the three models (one model for each of the three different outcome variables): once with the outliers included, and once with the outliers excluded. This was done in order to understand any effect the outliers may have had on the analysis, and to facilitate a transparent assessment of the data (Warner, 2013, pp. 156–157).

Normally Distributed Errors

First, I examined histograms of the variables to see whether the scores were normally distributed. Then the data were delineated between the comparison cohort and

the treatment cohort. Descriptive statistics were then generated and examined to further examine whether the data was normally distributed. The descriptive statistics concerning the dependent/outcome variables are depicted in Appendix B. Then, continuing on with the examination of the distribution shape of the scores on the variables, and with the data still split by cohort, the Shapiro-Wilk test and the Kolmogorov-Smirnov test were applied to the variables. The results of these two tests are set out in Appendix C.

Most importantly in the assessment of the assumption of normally distributed errors, histograms depicting the dependent/outcome variables and the standardized residuals were evaluated, and normal probability-probability plots were inspected. These histograms are set out in Appendix D, and the normal probability-probability plots are set out in Appendix E. The information revealed by these different examinations suggested that the data violated the assumption of normal distribution of errors. Therefore, the bootstrapping procedure was utilized in the multiple regression analyses because of the appearance of nonnormally distributed errors. It should also be noted that regardless of whether the assumption of normally distributed errors was violated or not, the bootstrapping method would have still needed to be employed in order to deal with the violation of the assumption of homogeneity of variances.

Homogeneity of Variance

The assumption of homogeneity of variances was checked in three different phases. First, this assumption was evaluated using the outcome variable *overall total rule violations in 2013-2014*. Second, this assumption was checked using the outcome variable *total major rule violations in 2013-2014*. Thirdly, this assumption was checked

using the outcome variable *total minor rule violations in 2013-2014*. Within each of these three phases, the assumption was checked with the outliers removed from the data set, and then again using the complete data set without the outliers removed.

Homoscedasticity and Overall Total Rule Violations

Homoscedasticity and overall total rule violations with outliers removed. First, a scatterplot generated from the data set with the outliers removed and depicting the standardized predicted values (*x* axis) and standardized residuals (*y* axis) for the outcome variable *overall total rule violations in 203-2014* was examined (Field, 2013, p. 330). This scatterplot showed some clustering of the data. Partial plots were also examined, several of which also showed some clustering of the data, especially with the partial plots relating to the variables *length of time spent on current sentence*, *length of time spent in disciplinary segregation during 2011-2012*, *total major rule violations in 2011-2012*, and *total minor rule violations in 2011-2012*. Next, a Levene's test was conducted using the outcome variable *overall total rule violations in 2013-2014*, and *cohort* (i.e., the grouping variable) as the factor variable. This Levene's test (using the median scores) revealed unequal variances, $F(1, 219) = 34.158$, $p < .001$. The information from the scatterplot, partial plots, and the Levene's test indicated that the assumption of homogeneity of variances had been violated. Therefore, the bootstrapping method was used with this data set. The scatterplots and partial plots used for these analyses can be found in Appendix F.

Homoscedasticity and overall total rule violations with full data set. The results from the following exercise were very similar with the results from the previous

exercise using the data set that excluded outliers. First, a scatterplot generated from the full data set and depicting the standardized predicted values (*x* axis) and standardized residuals (*y* axis) for the outcome variable *overall total rule violations in 2013-2014* was examined (Field, 2013, p. 330). This scatterplot showed some clustering of the data. Partial plots were also examined, several of which also showed some clustering of the data, especially with the partial plots relating to the variables *length of time spent on current sentence, length of time spent in disciplinary segregation during 2011-2012, total major rule violations in 2011-2012,* and *total minor rule violations in 2011-2012*. Next, a Levene's test was conducted using the outcome variable *overall total rule violations in 2013-2014,* and *cohort* as the factor variable. This Levene's test (using the median scores) revealed unequal variances, $F(1, 226) = 19.316$, $p < .001$. The information from the scatterplot, partial plots, and the Levene's test indicated that the assumption of homogeneity of variances had been violated. Therefore, the bootstrapping method was used with this data set. The scatterplots and partial plots used for these analyses can be found in Appendix G.

Homoscedasticity and Total Major Rule Violations

Homoscedasticity and total major rule violations with outliers removed. First, a scatterplot generated from the data set with the outliers removed and depicting the standardized predicted values (*x* axis) and standardized residuals (*y* axis) for the outcome variable *total major rule violations in 2013-2014* was examined (Field, 2013, p. 330). This scatterplot showed some clustering of the data. Partial plots were also examined, several of which also showed some clustering of the data, especially with the partial plots

relating to the variables *length of time spent on current sentence, length of time spent in disciplinary segregation during 2011-2012*, *total major rule violations in 2011-2012*, and *total minor rule violations in 2011-2012*. Next, a Levene's test was conducted using the outcome variable *total major rule violations in 2013-2014*, and *cohort* as the factor variable. This Levene's test (using the median scores) revealed unequal variances, $F(1, 219) = 41.579$, $p < .001$. The information from the scatterplot, partial plots, and the Levene's test indicated that the assumption of homogeneity of variances had been violated. Therefore, the bootstrapping method was used with this data set. The scatterplots and partial plots used for these analyses can be found in Appendix H.

Homoscedasticity and total major rule violations with full data set. The results from the following exercise were very similar with the results from the previous exercise using the data set that excluded outliers. First, a scatterplot generated from the full data set and depicting the standardized predicted values (*x* axis) and standardized residuals (*y* axis) for the outcome variable *overall total rule violations in 2013-2014* was examined (Field, 2013, p. 330). This scatterplot showed clustering of the data. Partial plots were also examined, several of which also showed some clustering of the data. The partial plots relating to the variables *cohort*, *age*, and *LS/CMI risk score* showed some clustering. The partial plots relating to the variables *length of time spent on current sentence, length of time spent in disciplinary segregation during 2011-2012*, *total major rule violations in 2011-2012*, and *total minor rule violations in 2011-2012* showed a more clearly defined pattern of clustering. Next, a Levene's test was conducted using the outcome variable *total major rule violations in 2013-2014*, and *cohort* as the factor

variable. This Levene's test (using the median scores) revealed unequal variances, $F(1, 226) = 18.161$, $p < .001$. The information from the scatterplot, partial plots, and the Levene's test indicated that the assumption of homogeneity of variances had been violated. Therefore, the bootstrapping method was used with this data set. The scatterplots and partial plots used for these analyses can be found in Appendix I.

Homoscedasticity and Total Minor Rule Violations

Homoscedasticity and total minor rule violations with outliers removed. First, a scatterplot generated from the data set with the outliers removed and depicting the standardized predicted values (*x* axis) and standardized residuals (*y* axis) for the outcome variable *total minor rule violations in 2013-2014* was examined (Field, 2013, p. 330). This scatterplot showed some clustering of the data. Partial plots were also examined, several of which also showed some clustering of the data. The partial plots relating to the variables *cohort* and *age* showed some clustering. The partial plots relating to the variables *length of time spent on current sentence*, *length of time spent in disciplinary segregation during 2011-2012, total major rule violations in 2011-2012,* and *total minor rule violations in 2011-2012* showed a more clearly defined pattern of clustering. Next, a Levene's test was conducted using the outcome variable *total minor rule violations in 2013-2014*, and *cohort* as the factor variable. This Levene's test (using the median scores) revealed unequal variances, $F(1, 221) = 10.885$, $p = .001$. The information from the scatterplot, partial plots, and the Levene's test indicated that the assumption of homogeneity of variances had been violated. Therefore, the bootstrapping method was

used with this data set. The scatterplots and partial plots used for these analyses can be found in Appendix J.

Homoscedasticity and total minor rule violations with full data set. The results from the following exercise were very similar with the results from the previous exercise using the data set that excluded outliers. First, a scatterplot generated from the full data set and depicting the standardized predicted values (*x* axis) and standardized residuals (*y* axis) for the outcome variable *total minor rule violations in 2013-2014* was examined (Field, 2013, p. 330). This scatterplot showed clustering of the data. Partial plots were also examined, several of which also showed some clustering of the data. The partial plots relating to the variables *cohort*, *age*, and *LS/CMI risk score* showed some clustering. The partial plots relating to the variables *length of time spent on current sentence*, *length of time spent in disciplinary segregation in 2011-2012*, *total major rule violations in 2011-2012*, and *total minor rule violations in 2011-2012* showed a more clearly defined pattern of clustering. Next, a Levene's test was conducted using the outcome variable *total minor rule violations in 2013-2014*, and *cohort* as the factor variable. This Levene's test (using the median scores) revealed unequal variances, $F(1, 226) = 9.526$, $p = .002$. The information from the scatterplot, partial plots, and the Levene's test indicated that the assumption of homogeneity of variances had been violated. Therefore, the bootstrapping method was used with this data set. The scatterplots and partial plots used for these analyses can be found in Appendix K.

Linearity

The same scatterplots and partial plots used to check for homoscedasticity were also used to check the assumption of linearity for each of the three models (both with and without outliers included). In viewing these graphs, it did not appear that the assumption of linearity had been violated. The scatterplots and partial plots used for these analyses can be found in Appendices B–G.

Independence of Observations

The Durbin-Watson test was conducted on each of the three models (with and without outliers) to determine whether the assumption of independence of errors was violated. For the primary model with the outcome variable *overall total rule violations in 2013-2014*, the Durbin-Watson statistic = 1.974 (outliers excluded), and 1.997 (full data set). For the model with the outcome variable *total major rule violations in 2013-2014*, the Durbin-Watson statistic = 1.994 (outliers excluded), and 2.016 (full data set). For the model with the outcome variable *total minor rule violations in 2013-2014*, the Durbin-Watson statistic = 1.718 (outliers excluded), and 1.905 (full data set). Since these values are so close to 2.0, it appears that the assumption of independence of observations was not violated.

Multicollinearity

Each of the models was examined to check for whether the assumption of multicollinearity was violated. Tables 6–11 depict the collinearity diagnostics for the different models. The collinearity diagnostics seen in Tables 6–11 indicated that the assumption of multicollinearity was not violated.

Table 6

Collinearity Statistics for Model With Outcome Variable Overall Total Rule Violations in 2013-2014 (Outliers Excluded From the Data Set)

Predictor Variable	Tolerance	VIF
Cohort	.703	1.422
Age	.736	1.359
Length of time spent on current sentence	.951	1.051
LS/CMI score	.826	1.210
Length of time spent in DSU in 2011-2012	.559	1.790
Total major rule violations in 2011-2012	.469	2.130
Total minor rule violations in 2011-2012	.788	1.269

Table 7

Collinearity Statistics for Model With Outcome Variable Overall Total Rule Violations in 2013-2014 (Full Data Set)

Predictor Variable	Tolerance	VIF
Cohort	.731	1.367
Age	.713	1.403
Length of time spent on current sentence	.917	1.090
LS/CMI score	.811	1.234
Length of time spent in DSU in 2011-2012	.595	1.681
Total major rule violations in 2011-2012	.432	2.315
Total minor rule violations in 2011-2012	.704	1.421

Table 8

Collinearity Statistics for Model With Outcome Variable Total Major Rule Violations in 2013-2014 (Outliers Excluded From the Data Set)

Predictor Variable	Tolerance	VIF
Cohort	.703	1.422
Age	.736	1.359
Length of time spent on current sentence	.951	1.051
LS/CMI score	.826	1.210
Length of time spent in DSU in 2011-2012	.559	1.790
Total major rule violations in 2011-2012	.469	2.130
Total minor rule violations in 2011-2012	.788	1.269

Table 9

Collinearity Statistics for Model With Outcome Variable Total Major Rule Violations in 2013-2014 (Full Data Set)

Predictor Variable	Tolerance	VIF
Cohort	.731	1.367
Age	.713	1.403
Length of time spent on current sentence	.917	1.090
LS/CMI score	.811	1.234
Length of time spent in DSU in 2011-2012	.595	1.681
Total major rule violations in 2011-2012	.432	2.315
Total minor rule violations in 2011-2012	.704	1.421

Table 10

Collinearity Statistics for Model With Outcome Variable Total Minor Rule Violations in 2013-2014 (Outliers Excluded From the Data Set)

Predictor Variable	Tolerance	VIF
Cohort	.707	1.414
Age	.737	1.357
Length of time spent on current sentence	.953	1.050
LS/CMI score	.810	1.234
Length of time spent in DSU in 2011-2012	.570	1.755
Total major rule violations in 2011-2012	.456	2.192
Total minor rule violations in 2011-2012	.735	1.361

Table 11

Collinearity Statistics for Model With Outcome Variable Total Minor Rule Violations in 2013-2014 (Full Data Set)

Predictor Variable	Tolerance	VIF
Cohort	.731	1.367
Age	.713	1.403
Length of time spent on current sentence	.917	1.090
LS/CMI score	.811	1.234
Length of time spent in DSU in 2011-2012	.595	1.681
Total major rule violations in 2011-2012	.432	2.315
Total minor rule violations in 2011-2012	.704	1.421

Data Collection

The data that were used in this study were archival and administrative data held by the Oregon DOC. Data were gathered on all the Oregon DOC inmates who were incarcerated from January 1, 2011, through December 31, 2014 in a facility or facilities with a medium-security component. In particular, data regarding the inmates' gender, age, length of time spent incarcerated on current sentence, LS/CMI section 1 risk score, prison misconduct history in 2011-2012, and their disciplinary segregation history were gathered. The Oregon DOC Research and Evaluation Unit helped me collect and organize the necessary data. The Research and Evaluation Unit developed and ran computer codes that were then deployed to collect the data (J. Duncan, personal

communication, January 28, 2015). After compiling the data, the Research and Evaluation Unit then delivered it to me in a Microsoft Excel spreadsheet format in July of 2015. I then uploaded the data into the SPSS computer program.

Results

A multiple regression analysis was run on each of the three models. The primary model that operationalized the dependent/outcome variable *prison misconduct* as the overall total rule violations in 2013-2014 was analyzed first. The multiple regression analysis was run twice using this model: once with the outliers removed from the data set, and then a second time with the full data set (i.e., no outliers removed). This was done in order to develop a fuller picture of the effect of disciplinary segregation on subsequent prison misconduct and to facilitate a transparent analysis of the data. This same process was then applied to the second model, which operationalized the dependent/outcome variable *prison misconduct* as the total major rule violations in 2013-2014; and then again with the third model that operationalized the dependent/outcome variable *prison misconduct* as the total minor rule violations in 2013-2014. Multiple regression analysis with bootstrapping using bias corrected and accelerated confidence intervals was used on each of the models to deal with the violations of the assumptions of normality and homogeneity of variances (Field, 2013, pp. 198–200, 350–352; Warner, 2013, pp. 657–662).

Steps were carried out to discover the global effect size and the power of each of the models. In addition, the local effect size for the independent/predictor variable *cohort*

was determined for each of the models. The global effect size was computed using the Cohen's f^2 statistic. The equation for that statistic is:

$$f^2 = (R^2) / (1 - R^2)$$

(Selya, Rose, Dierker, Hedeker, & Mermelstein, 2012, p. 2; G*Power 3.1 Manual, 2014, pp. 34–35; Cohen, 1988). In calculating this global effect size, the adjusted R^2 value was used. After determining the global effect size, the appropriate values were then inputted into the G*Power application (version 3.1) (effect size f^2, α-level, sample size, and number of predictors) and a posthoc power analysis was conducted (Faul, Erdfelder, Lang, & Buchner, 2007; Faul, Erdfelder, Buchner, & Lang, 2009). Because multiple significance tests were performed, the α-level was set at 0.008 using the Bonferroni corrected method, in order to obtain a conservative power estimate. The equation for the Bonferroni corrected method is:

$$PC_\alpha = EW_\alpha / k$$

where EW_α is the α-level for the whole study (here, $\alpha = .05$), "and k is the number of significance tests performed in the entire [] study" (Warner, 2013, pp. 89–99, 101, 565). Here, there were three models tested, and each model was tested once with outliers removed and then once without the outliers removed, for a total of six multiple regression analyses.

For each of the analyses, the local effect size of the predictor *cohort* was also calculated (sr^2). This was calculated by squaring the part correlation associated with that specific variable (Warner, 2013, pp. 569, 579).

Multiple Regression and Overall Total Rule Violations

Multiple regression and overall total rule violations with outliers removed.

The number of overall total rule violations in 2013-2014 were predicted from the following variables: cohort (coded 0 = comparison cohort, 1 = treatment cohort), age, length of time spent on current sentence, LS/CMI risk score, total major rule violations in 2011-2012, total minor rule violations in 2011-2012, and length of time spent in disciplinary segregation during 2011-2012. The total *N* for this sample was 228; 21 outlier cases were removed and, therefore, for this analysis, $N = 207$.

Standard multiple regression was performed; that is, all predictor variables were entered in one step. Results for this standard multiple regression are summarized in Table 12. The overall regression, including all seven predictors, was statistically significant, $R = 0.646$, $R^2 = 0.417$, adjusted $R^2 = 0.397$, $F(7, 199) = 20.365$, $p < .001$. The positive slope ($b = 0.720$) for *cohort* indicated that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher numbers of overall total rule violations; however, since the bootstrap confidence interval included zero (–0.249, 1.647), this "positive" predictive relationship between disciplinary segregation and overall total rule violations may not be genuine. More importantly, the predictive relation of *cohort* to overall total rule violations was not significant, $p = .156$. The results of this model indicated that disciplinary segregation is not a significant predictor of subsequent misconduct. This result therefore suggests that the experience of disciplinary segregation does not affect subsequent overall prison misconduct. In other words, this result suggests that

disciplinary segregation neither decreases nor increases subsequent overall prison misconduct among those inmates subjected to it (i.e., it has a null effect).

The global effect size for this model was $f^2 = 0.658$. An analysis of the model using the G*Power application revealed that the power for this test was about 1.000.

The local effect size of the independent/predictor variable *cohort* was also calculated. For this model, the proportion of variance uniquely explained by the predictor *cohort* was $sr^2 = .002$.

Table 12

Linear Model of Predictors of Overall Total Rule Violations in 2013-2014 (Outliers Excluded From the Data Set)

	b	*SE B*	*β*	*p*	Part Correlation
Constant	1.226 (–0.830, 3.150)	1.176		$p = .309$	
Cohort	0.720 (–0.249, 1.647)	0.502	.054	$p = .156$	.046
Age	–0.059 (–0.109, 0.001)	0.028	–.119	$p = .039$	–.102
Length of time spent on current sentence	7.376E-005 (0.000, 0.001)	0.000	.015	$p = .752$	.015
LS/CMI score	0.066 (–0.011, 0.150)	0.038	.098	$p = .092$	.089
Days spent in DSU in 2011-2012	–0.001 (–0.009, 0.007)	0.004	–.027	$p = .783$	–.020
Total major rule violations in 2011-2012	0.363 (0.181, 0.549)	0.095	.416	$p < .001$	.285
Total minor rule violations in 2011-2012	0.832 (0.234, 1.424)	0.290	.207	$p = .003$	.184

Note. 95% bias corrected and accelerated confidence intervals reported in parentheses; Confidence intervals and standard errors based on 2000 bootstrap samples.

Multiple regression and overall total rule violations with full data set. The number of overall total rule violations in 2013-2014 were predicted from the following variables: cohort (coded 0 = comparison cohort, 1 = treatment cohort), age, length of time spent on current sentence, LS/CMI risk score, total major rule violations in 2011-2012,

total minor rule violations in 2011-2012, and length of time spent in disciplinary segregation during 2011-2012. The total *N* for this sample was 228; no outliers were removed for this analysis.

Standard multiple regression was performed; that is, all predictor variables were entered in one step. Results for this standard multiple regression are summarized in Table 13. The overall regression, including all seven predictors, was statistically significant, $R = 0.687$, $R^2 = 0.472$, adjusted $R^2 = 0.455$, $F(7, 220) = 28.059$, $p < .001$. The positive slope ($b = 0.610$) for *cohort* indicated that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher numbers of overall total rule violations; however, since the bootstrap confidence interval included zero (–0.625, 1.910), this "positive" predictive relationship between disciplinary segregation and overall total rule violations may not be genuine. More importantly, the predictive relation of *cohort* to overall total rule violations was not significant, $p = .364$. The results of this model indicated that disciplinary segregation is not a significant predictor of subsequent misconduct. This result therefore suggests that the experience of disciplinary segregation does not affect subsequent overall prison misconduct. In other words, this result suggests that disciplinary segregation neither decreases nor increases overall subsequent overall prison misconduct among those inmates subjected to it (i.e., it has a null effect).

The global effect size for this model was $f^2 = 0.455$. An analysis of the model using the G*Power application revealed that the power for this test was about 1.000.

The local effect size of the independent/predictor variable *cohort* was also calculated. For this model, the proportion of variance uniquely explained by the predictor *cohort* was sr^2 = 5.76E-4.

Table 13

Linear Model of Predictors of Overall Total Rule Violations in 2013-2014 (Full Data Set)

	b	*SE B*	*β*	*p*	Part Correlation
Constant	–2.217 (–8.137, 2.507)	2.508		*p* = .405	
Cohort	0.610 (–0.625, 1.910)	0.672	.028	*p* = .364	.024
Age	–0.014 (–0.094, 0.073)	0.045	–.018	*p* = .757	–.015
Length of time spent on current sentence	0.000 (–0.001, 0.001)	0.000	.024	*p* = .744	.023
LS/CMI score	0.136 (0.036, 0.252)	0.053	.131	*p* = .017	.118
Days spent in DSU in 2011-2012	–0.008 (–0.019, 0.001)	0.005	–.139	*p* = .109	–.107
Total major rule violations in 2011-2012	0.566 (0.304, 0.871)	0.139	.502	*p* = .001	.330
Total minor rule violations in 2011-2012	1.157 (0.585, 1.743)	0.314	.263	*p* = .002	.221

Note. 95% bias corrected and accelerated confidence intervals reported in parentheses; Confidence intervals and standard errors based on 2000 bootstrap samples.

Multiple Regression and Total Major Rule Violations

Multiple regression and total major rule violations with outliers removed.

The number of total major rule violations in 2013-2014 were predicted from the following variables: cohort (coded 0 = comparison cohort, 1 = treatment cohort), age, length of time spent on current sentence, LS/CMI risk score, total major rule violations in 2011-2012, total minor rule violations in 2011-2012, and length of time spent in disciplinary segregation during 2011-2012. The total *N* for this sample was 228; 21 outlier cases were removed and, therefore, for this analysis, $N = 207$.

Standard multiple regression was performed; that is, all predictor variables were entered in one step. Results for this standard multiple regression are summarized in Table 14. The overall regression, including all seven predictors, was statistically significant, $R = 0.632$, $R^2 = 0.400$, adjusted $R^2 = 0.379$, $F(7, 199) = 18.939$, $p < .001$. The positive slope ($b = 0.499$) for *cohort* indicated that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher numbers of overall total rule violations; however, since the bootstrap confidence interval included zero (–0.423, 1.390), this "positive" predictive relationship between disciplinary segregation and total major rule violations may not be genuine. More importantly, the predictive relation of *cohort* to total major rule violations was not significant, $p = .279$. The results of this model indicate that disciplinary segregation is not a significant predictor of subsequent major misconduct. This result therefore suggests that the experience of disciplinary segregation does not affect subsequent major-level prison misconduct. In other words, this result suggests that

disciplinary segregation neither decreases nor increases subsequent major-level prison misconduct among those inmates subjected to it (i.e., it has a null effect).

The global effect size for this model was $f^2 = 0.379$. An analysis of the model using the G*Power application revealed that the power for this test was about 1.000.

The local effect size of the independent/predictor variable *cohort* was also calculated. For this model, the proportion of variance uniquely explained by the predictor *cohort* was $sr^2 = .001$.

Table 14

Linear Model of Predictors of Total Major Rule Violations in 2013-2014 (Outliers Excluded From the Data Set)

	b	*SE B*	*β*	*p*	Part Correlation
Constant	1.585 (–0.423, 3.389)	1.095		*p* = .153	
Cohort	0.499 (–0.423, 1.390)	0.461	.043	*p* = .279	.036
Age	–0.063 (–0.111, –0.005)	0.026	–.143	*p* = .018	–.122
Length of time spent on current sentence	0.000 (0.000, 0.001)	0.000	.028	*p* = .559	.027
LS/CMI score	0.051 (–0.018, 0.128)	0.036	.086	*p* = .160	.078
Days spent in DSU in 2011-2012	4.045E-005 (–0.006, 0.007)	0.003	.001	*p* = .990	.001
Total major rule violations 2011-2012	0.315 (0.163, 0.466)	0.083	.407	*p* < .001	.279
Total minor rule violations in 2011-2012	0.611 (0.056, 1.190)	0.265	.172	*p* = .020	.152

Note. 95% bias corrected and accelerated confidence intervals reported in parentheses; Confidence intervals and standard errors based on 2000 bootstrap samples.

Multiple regression and total major rule violations with full data set. The number of total major rule violations in 2013-2014 were predicted from the following variables: cohort (coded 0 = comparison cohort, 1 = treatment cohort), age, length of time

spent on current sentence, LS/CMI risk score, total major rule violations in 2011-2012, total minor rule violations in 2011-2012, and length of time spent in disciplinary segregation during 2011-2012. The total *N* for this sample was 228; no outliers were removed for this analysis.

Standard multiple regression was performed; that is, all predictor variables were entered in one step. Results for this standard multiple regression are summarized in Table 15. The overall regression, including all seven predictors, was statistically significant, $R = 0.655$, $R^2 = 0.429$, adjusted $R^2 = 0.411$, $F(7, 220) = 23.621$, $p < .001$. The positive slope ($b = 0.473$) for *cohort* indicated that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher numbers of overall total rule violations; however, since the bootstrap confidence interval included zero (–0.664, 1.638), this "positive" predictive relationship between disciplinary segregation and total major rule violations may not be genuine. More importantly, the predictive relation of *cohort* to total major rule violations was not significant, $p = .445$. The results of this model indicate that the disciplinary segregation is not a significant predictor of subsequent misconduct. This result therefore suggests that the experience of disciplinary segregation does not affect subsequent major-level prison misconduct. In other words, this result suggests that the experience of disciplinary segregation neither decreases nor increases subsequent major-level prison misconduct among those inmates subjected to it (i.e., it has a null effect).

The global effect size for this model was $f^2 = 0.411$. An analysis of the model using the G*Power application revealed that the power for this test was about 1.000.

The local effect size of the independent/predictor variable *cohort* was also calculated. For this model, the proportion of variance uniquely explained by the predictor *cohort* was sr^2 = 4.41E-4.

Table 15

Linear Model of Predictors of Total Major Rule Violations in 2013-2014 (Full Data Set)

	b	*SE B*	*β*	*p*	Part Correlation
Constant	−1.746 (−7.535, 2.675)	2.425		*p* = .497	
Cohort	0.473 (−0.664, 1.638)	0.626	.024	*p* = .445	.021
Age	−0.019 (−0.098, 0.065)	0.043	−.027	*p* = .662	−.023
Length of time spent on current sentence	0.000 (0.000, 0.001)	0.000	.036	*p* = .659	.034
LS/CMI score	0.119 (0.025, 0.230)	0.051	.127	*p* = .025	.114
Days spent in DSU in 2011-2012	−0.007 (−0.017, 0.001)	0.005	−.130	*p* = .155	−.100
Total major rule violations in 2011-2012	0.513 (0.273, 0.800)	0.129	.504	*p* = .001	.331
Total minor rule violations in 2011-2012	0.847 (0.321, 1.433)	0.300	.213	*p* = .007	.179

Note. 95% bias corrected and accelerated confidence intervals reported in parentheses; Confidence intervals and standard errors based on 2000 bootstrap samples.

Multiple Regression and Total Minor Rule Violations

Multiple regression and total minor rule violations with outliers removed.

The number of total minor rule violations in 2013-2014 were predicted from the following variables: cohort (coded 0 = comparison cohort, 1 = treatment cohort), age, length of time spent on current sentence, LS/CMI risk score, total major rule violations in 2011-2012, total minor rule violations in 2011-2012, and length of time spent in disciplinary segregation during 2011-2012. The total *N* for this sample was 228; 19 outlier cases were removed and, therefore, for this analysis, $N = 209$.

Standard multiple regression was performed; that is, all predictor variables were entered in one step. Results for this standard multiple regression are summarized in Table 16. The overall regression, including all seven predictors, was statistically significant, $R = 0.505$, $R^2 = 0.255$, adjusted $R^2 = 0.229$, $F(7, 201) = 9.847$, $p < .001$. The positive slope ($b = 0.138$) for *cohort* indicated that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher numbers of overall total rule violations; however, since the bootstrap confidence interval included zero (–0.102, 0.366), this "positive" predictive relationship between disciplinary segregation and total minor rule violations may not be genuine. More importantly, the predictive relation of *cohort* to total minor rule violations was not significant, $p = .246$. The results of this model indicate that disciplinary segregation is not a significant predictor of subsequent misconduct. This result therefore suggests that the experience of disciplinary segregation does not affect subsequent minor-level prison misconduct. In other words, this result suggests that the

experience of disciplinary segregation neither increases nor decreases subsequent minor-level prison misconduct among those inmates subjected to it (i.e., it has a null effect).

The global effect size for this model was $f^2 = 0.229$. An analysis of the model using the G*Power application revealed that the power for this test was about 0.9999.

The local effect size of the independent/predictor *cohort* was also calculated. For this model, the proportion of variance uniquely explained by the predictor *cohort* was $sr^2 = .002$.

Table 16

Linear Model of Predictors of Total Minor Rule Violations in 2013-2014 (Outliers Excluded From the Data Set)

	b	*SE B*	*β*	*p*	Part Correlation
Constant	–0.303 (–0.935, 0.354)	0.312		*p* = .333	
Cohort	0.138 (–0.102, 0.366)	0.119	.054	*p* = .246	.045
Age	0.005 (–0.008, 0.018)	0.007	.056	*p* = .443	.048
Length of time spent on current sentence	–2.724E-005 (0.000, 6.903E-005)	4.833E-005	–.029	*p* = .582	–.028
LS/CMI score	0.007 (–0.006, 0.022)	0.008	.057	*p* = .341	.052
Days spent in DSU in 2011-2012	8.068E-005 (–0.002, 0.002)	0.001	.010	*p* = .925	.008
Total major rule violations in 2011-2012	0.027 (–0.013, 0.062)	0.019	.168	*p* = .157	.113
Total minor rule violations in 2011-2012	0.264 (0.139, 0.403)	0.069	.372	*p* = .001	.319

Note. 95% bias corrected and accelerated confidence intervals reported in parentheses; Confidence intervals and standard errors based on 2000 bootstrap samples.

Multiple regression and total minor rule violations with full data set. The number of total minor rule violations in 2013-2014 were predicted from the following

variables: cohort (coded 0 = comparison cohort, 1 = treatment cohort), age, length of time spent on current sentence, LS/CMI risk score, total major rule violations in 2011-2012, total minor rule violations in 2011-2012, and length of time spent in disciplinary segregation during 2011-2012. The total *N* for this sample was 228; no outliers were removed for this analysis.

Standard multiple regression was performed; that is, all predictor variables were entered in one step. Results for this standard multiple regression are summarized in Table 17. The overall regression, including all seven predictors, was statistically significant, $R = 0.591$, $R^2 = 0.349$, adjusted $R^2 = 0.328$, $F(7, 220) = 16.843$, $p < .001$. The positive slope ($b = 0.137$) for *cohort* indicated that higher scores on *cohort* (i.e., being in the treatment cohort) predicted higher numbers of overall total rule violations; however, since the bootstrap confidence interval included zero (–0.104, 0.440), this "positive" predictive relationship between disciplinary segregation and total minor rule violations may not be genuine. More importantly, the predictive relation of *cohort* to total minor rule violations was not significant, $p = .314$. The results of this model indicate that disciplinary segregation is not a significant predictor of subsequent misconduct. This result therefore suggests that the experience of disciplinary segregation does not affect subsequent minor-level prison misconduct. In other words, this result suggests that the experience of disciplinary segregation neither increases nor decreases subsequent minor-level prison misconduct among those inmates subjected to it (i.e., it has a null effect).

The global effect size for this model was $f^2 = 0.328$. An analysis of the model using the G*Power application revealed that the power for this test was about 1.000.

The local effect size of the independent/predictor variable *cohort* was also calculated. For this model, the proportion of variance uniquely explained by the predictor *cohort* was $sr^2 = .001$.

Table 17

Linear Model of Predictors of Total Minor Rule Violations in 2013-2014 (Full Data Set)

	b	*SE B*	*β*	*p*	Part Correlation
Constant	–0.471 (–1.099, 0.156)	0.296		*p* = .113	
Cohort	0.137 (–0.104, 0.440)	0.137	.037	*p* = .314	.032
Age	0.005 (–0.008, 0.017)	0.007	.039	*p* = .439	.033
Length of time spent on current sentence	–4.642E-005 (0.000, 3.250E-005)	4.134E-005	–.045	*p* = .251	–.043
LS/CMI score	0.017 (0.001, 0.036)	0.010	.096	*p* = .082	.086
Days spent in DSU in 2011-2012	–0.001 (–0.003, 0.000)	0.001	–.127	*p* = .159	–.098
Total major rule violations in 2011-2012	0.053 (0.011, 0.102)	0.023	.271	*p* = .018	.178
Total minor rule violations in 2011-2012	0.310 (0.182, 0.416)	0.067	.409	*p* < .001	.343

Note. 95% bias corrected and accelerated confidence intervals reported in parentheses; Confidence intervals and standard errors based on 2000 bootstrap samples.

Summary

For each of the models, the variable *cohort* did not significantly predict subsequent prison misconduct. This was the case for each of the models regardless of whether outliers were included in the data set or excluded from the data set. Specifically, the variable *cohort* did not significantly predict overall total rule violations, nor did it significantly predict total major rule violations, nor did it significantly predict total minor rule violations.

These results can be analyzed in light of the following research questions and null hypotheses that were addressed in this study:

RQ1: Does deterrence theory explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among inmates, after controlling for the effects of gender, age, length of time spent incarcerated on current sentence, LS/CMI risk score, prior major rule violations in 2011-2012, prior minor rule violations in 2011-2012, and length of time spent in disciplinary segregation in 2011-2012?

- The results suggest that deterrence theory does not explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among inmates.

RQ2: Does the experience of disciplinary segregation reduce subsequent prison misconduct among inmates who are subjected to it, and if so, to what extent?

- The results suggest that the experience of disciplinary segregation does not reduce subsequent prison misconduct among those inmates subjected to it.

RQ3: Does the experience of disciplinary segregation have a criminogenic effect on inmates who are subjected to it?

- The results suggest that the experience of disciplinary segregation does not increase subsequent prison misconduct among those inmates subjected to it.

NH1: The independent/predictor variable *cohort* does not significantly predict prison misconduct.

- The results indicate that this hypothesis should not be rejected. The variable *cohort* did not significantly predict prison misconduct.

NH2: The level of the independent/predictor variable *cohort* that indicates membership in the cohort comprised of those who have spent time in disciplinary segregation (the treatment cohort) does not significantly negatively predict prison misconduct.

- The results indicate that this hypothesis should not be rejected. The variable *cohort* did not significantly negatively predict prison misconduct.

NH3: The level of the independent/predictor variable *cohort* that indicates membership in the cohort comprised of those who have spent time in disciplinary segregation (the treatment cohort) does not significantly positively predict prison misconduct.

- The results indicate that this hypothesis should not be rejected. The variable *cohort* did not significantly positively predict prison misconduct.

Chapter 5: Discussion, Conclusions, and Recommendations

Introduction

The purpose of this quantitative study was to examine the effectiveness of disciplinary segregation in deterring prison inmate misconduct within the Oregon DOC prison system. Specifically, the purpose of this retrospective observational study was to test the theory of deterrence that relates disciplinary segregation to prison misconduct.

The results of the multiple regression analyses for this study showed that the variable *cohort* did not significantly predict prison misconduct. The results suggested that the experience of disciplinary segregation does not decrease or increase subsequent prison misconduct among those inmates subjected to it in a statistically significant manner, showing that it has a null effect. This result was consistent throughout the testing of the different models.

In general, the study results were not statistically significant. The primary model operationalized the outcome variable as *overall total prison rule violations in 2013-2014*, and the analyses revealed that the variable *cohort* did not significantly predict prison misconduct. This was the result regardless of whether outliers were excluded or included within the data set. The model that operationalized the outcome variable as *total major rule violations in 2013-2014* also found that the variable *cohort* did not significantly predict prison misconduct, regardless of whether outliers were included in the data or excluded from the data. The model that operationalized the outcome variable as *total minor rule violations in 2013-2014* also found that the variable *cohort* did not significantly predict prison misconduct, regardless of whether outliers were included or

excluded from the data set. All of these results suggest that the experience of disciplinary segregation does not significantly affect subsequent prison misconduct among those inmates subjected to it. Put another way: These results suggest that the experience of disciplinary segregation does not significantly increase or decrease subsequent prison misconduct among those inmates subjected to it.

Interpretation of Findings

The results of this study suggest that deterrence theory does not explain the relationship between the experience of disciplinary segregation and subsequent prison misconduct among those inmates subjected to it. In essence, the findings of this study do not support the deterrence hypothesis. Just as Lynch (1999), Kovandzic and Vieraitis (2006), and DeFina and Arvanites (2002) found "little or no significant relationship" (Kovandzic & Vieraitis, 2002, p. 227) between incarceration rates and crime rates, this study similarly found no significant relationship between the experience of disciplinary segregation and subsequent prison misconduct among those subjected to it. This is in contrast to other studies such as Levitt (1996, 2004) and Marvell and Moody (1994), who found a negative relationship between the incarceration rate and the crime rate (e.g., an increase in the incarceration rate decreased the crime rate).

The findings of Nagin, Cullen, and Jonson (2009) and Bales and Piquero (2012) indicated that custodial sanctions (e.g., incarceration) may have a criminogenic effect when compared to noncustodial sanctions. In contrast, the results of this present study did not indicate that the experience of disciplinary segregation had a criminogenic effect on subsequent behavior. Dölling, Entorf, Hermann, and Rupp's (2009) meta-analysis of 700

deterrence studies found that for 50.5% of those studies, the deterrent effect estimate was not significant, whereas for 41.7% of the studies the deterrent effect estimate was significant and supported the deterrence hypothesis, and for 7.8% of the studies the deterrent effect estimate was significant and did not support the deterrence hypothesis. Similarly to those studies in the 50.5% category in which the deterrent effect estimate was not significant, here in this study the effect of the experience of disciplinary segregation on subsequent prison misconduct was also not significant. Dölling, Entorf, Hermann, and Rupp's meta-analysis also revealed that deterrent effects are more pervasive for property and administrative-type offenses, as opposed to serious and violent crimes. This suggests that even if the experience of disciplinary segregation does not decrease subsequent major rule violations, it may however decrease subsequent minor rule violations. However, the results of this dissertation study suggested that the experience of disciplinary segregation does not actually decrease minor rule violations to a significant extent.

The results of this present study are generally aligned with some of the previous research on segregation and its relationship with deterrence. Barak-Glantz (1983) found that solitary confinement had only a "minimal" deterrent effect on inmates (p. 36). The overall results of the Briggs, Sundt, and Castellano (2003) study on whether supermax prisons reduced prison violence did not support the deterrence hypothesis. The research by Mears and Bales (2009) indicated that supermax incarceration did not reduce recidivism. In addition, the results of the study by Motiuk and Blanchette (2001) on Canadian segregation and recidivism also did not support the deterrence theory (in fact,

their results suggested that segregation may have a criminogenic effect on subsequent offending). The results of these studies did not support the deterrence hypothesis in the segregation context. Similarly, the present study also failed to reveal evidence supporting the hypothesis that segregation has a deterrent effect on those subjected to it. Overall, the results of this study on segregation and prison inmate misconduct did not support the deterrence hypothesis, just as the findings of several other types of studies, including some focused on incarceration and crime, custodial sanctions, and segregation and recidivism, also did not support the deterrence hypothesis.

Alternative Criminal Behavior Theories

This study was specifically designed to explore whether deterrence theory explained the relation between the experience of disciplinary segregation and subsequent misconduct among those subjected to it. It was not designed to test other criminal behavior theories. However, since the findings of this study suggest that deterrence theory does not explain the relation between the experience of disciplinary segregation and subsequent prison misconduct, another different criminal behavior theory or theories may instead explain these results. A brief discussion of some of these other theories of criminal behavior that could potentially explain the results of this study is provided as an aid to future research.

Disciplinary segregation may have a null effect due to certain influencing factors, or alternatively its deterrent effect might be masked by other stronger influencing factors. Some of these potential factors influencing the subsequent behavior of inmates after spending time in disciplinary segregation may be explained by social learning theory.

Social learning theory holds that an individual may engage in criminal behavior due to receiving rewards for engaging in criminal behavior, acting in imitation of others, and/or acting in accordance with a certain beliefs or attitudes that they hold (Akers & Jennings, 2009). In the disciplinary segregation/prison misconduct context, disciplinary segregation may not have an effect on subsequent misconduct because inmates are acting pursuant to an overriding anti-institutional attitude. Another possibility under social learning theory is that the inmate is acting in imitation of another when committing rule violations. Or, the inmate may have received some reward (e.g., recognition and esteem) in the past for committing rule violations and/or spending time in disciplinary segregation.

Other theories offer alternate explanations. Under self-control theory, subsequent prison misconduct following disciplinary segregation may simply be due to an inmate's lack of self-control (Gottfredson & Hirschi, 1990; Pratt & Cooper, 2009). Under social-control theory, subsequent behavior following disciplinary segregation may be primarily influenced by the strength of the social bond between the inmate and society, and a weak bond with society may lead to subsequent misconduct (Schreck & Hirschi, 2009). Under strain theory, subsequent behavior following disciplinary segregation may be primarily influenced by the presence or nonpresence of certain stressors, and therefore subsequent prison misconduct may be due to certain stressors being present in an inmate's life (Agnew, 2009). Defiance theory could also explain subsequent behavior following disciplinary segregation, especially if the inmate viewed the punishment as unfair (Bales & Piquero, 2012, p. 72, n. 1; Bouffard & Piquero, 2010; Sherman, 1993). Furthermore,

under the biosocial perspective, the subsequent behavior of an inmate following spending time in disciplinary segregation may be due to a biological predisposition towards certain behavior, or may be due to a combination of biological predisposition and other sociological and environmental factors (Boisvert, 2015; Crews, 2009). These are just some of the theories that could potentially explain the results of this study.

Limitations of the Study

The conclusions that may be drawn from this present study are circumscribed by several limitations. These limitations include possible selection bias, the design of the study, and the possible influence of external variables. Selection bias refers to a situation where two cohorts have pretreatment differences that result in them being noncomparable groups (e.g., comparing apples to oranges) (Rosenbaum, 1989, p. 1024). Here, selection bias may have been present within this study in that the treatment group may have been more naturally predisposed to committing rule violations than the comparison group. The study attempted to reduce the effect selection bias may have had on the results by using multiple regression analysis to control for other factors that have been found to be related to prison misconduct, such as age, LS/CMI risk score, prior prison misconduct, and length of time spent on current sentence.

The design of the study is also an issue. The study utilized multiple regression analyses, which limits the conclusions that can be drawn from the results of the study. Results from multiple regression analyses do not provide "proof of causality" (Warner, 2013, p. 556). Instead, the results can only suggest the possibility of causality. In other words, the strength of the inferences that may be drawn from the findings is limited.

Another limitation with regard to the design of the study was the sample. There were 37 participants in the comparison cohort and 191 in the treatment cohort. Perhaps the results of the analyses would have been different if the two cohorts were more balanced in terms of the number of participants in each cohort.

Another design issue may be the choice of the control/predictor variables that were included in the study. Only major rule violations may be punished by disciplinary segregation, minor rule violations by themselves and unaccompanied by major rule violations may not be punished by disciplinary segregation (Or. Admin. R. 291-105-005 *et seq.*). Given this fact, the control/predictor variable *prior major rule violations in 2011-2012* and the independent/predictor variable *cohort* may have been targeting the same phenomenon. This is because each of the participants in the treatment cohort was placed in that cohort because they had spent time in disciplinary segregation in 2011-2012, as punishment for a major rule violation within that same timeframe. However, in response to this critique, it should be noted that not all major rule violations are punished by disciplinary segregation. The choice of disciplinary segregation as punishment is based upon the severity of the rule violation and the inmate's prior misconduct history, among other considerations (Or. Admin. R. 291-105-005 *et seq.*).

The inclusion of the variable *length of time spent in disciplinary segregation in 2011-2012* may also be a weakness in the study. This is because it could be argued that there is no research or theoretical basis upon which to justify its inclusion (Field, 2013, p. 321; Warner, 2013, p. 548). Furthermore, with regard to the selection of variables, of the seven predictor variables used in the study, only a few significantly predicted the prison

misconduct outcome variable. Out of the six multiple regression analyses, only age, LS/CMI risk score, total major rule violations in 2011-2012, and total minor rule violations in 2011-2012 sometimes significantly predicted the prison misconduct outcome variable. Age significantly predicted *overall total rule violations in 2013-2014 (outliers deleted)* at the .05 α-level (p = .039), and *total major rule violations in 2013-2014 (outliers deleted)* at the .05 α-level (p = .018). LS/CMI risk score significantly predicted *overall total rule violations in 2013-2014 (full data set)* at the .017 α-level (p = .017), and *total major rule violations in 2013-2014 (outliers deleted)* at the .05 α-level (p = .025). Total major rule violations in 2011-2012 significantly predicted the prison misconduct outcome variable at the $p < .008$ α-level for each of the multiple regression analyses except for *total minor rule violations in 2013-2014 (outliers deleted)* and *total minor rule violations in 2013-2014 (full data set)*. Total major rule violations in 2011-2012 did not significantly predict *total minor rule violations in 2013-2014 (outliers deleted)*, but did significantly predict *total minor rule violations in 2013-2014 (full data set)* at the .05 α-level ($p < .018$). For each of the six multiple regression analyses, total minor rule violations in 2011-2012 significantly predicted the prison misconduct outcome variable at the $p < .008$ α-level; except for the outcome variable *total major rule violations in 2013-2014 (outliers deleted)*, although it significantly predicted that outcome variable at the .05 α-level (p = .020).

Another limitation is that external variables not accounted for within the study may have influenced the results. The aim of the design of the study was to control for

variables that may influence prison misconduct and isolate the effect the experience of disciplinary segregation may have had on subsequent misconduct. However, there may be variables that influenced the results that were not included in the study. In essence, the design of the study may have failed to isolate the effect, if any, of the experience of disciplinary segregation on subsequent misconduct.

It should also be noted that it appears that this might be the first study to directly examine whether the experience of disciplinary segregation affects subsequent prison inmate misconduct. The findings and results should be interpreted within that context. More research is needed in order to develop a full and accurate assessment of the effect that the experience of disciplinary segregation may have on subsequent prison inmate misconduct.

Other Considerations

Regardless of whether or not the experience of disciplinary segregation is effective at reducing the subsequent misconduct among those subjected to it, it may serve other beneficial purposes that should be kept in mind. For example, disciplinary segregation could serve the purposes of retribution and just deserts. Furthermore, and perhaps more importantly, the institution of disciplinary segregation may serve as a general deterrent to other prisoners, and may prevent them from engaging in prison misconduct (or at least limit their misconduct). Disciplinary segregation could hypothetically have both a deterrent effect on the general population, and a criminogenic effect on those directly subjected to it; the two possible results are not mutually exclusive (Nagin, Cullen, & Jonson, 2009, p. 132). Even if later studies show that the experience

of disciplinary segregation may increase subsequent prison inmate misconduct, a fair evaluation of disciplinary segregation should involve an analysis of both the potential costs (e.g., a criminogenic effect on those subjected to it), with the potential benefit (e.g., a general deterrent effect on the general inmate population). Zimring and Hawkins (1973) point out that "some methods of punishment . . . may themselves be criminogenic. Insofar as this is the case the preventive effect of punishment on other potential offenders has to be weighed against the possible criminogenic effect on the offender" (p. 43). In addition, the potential benefits of disciplinary segregation are not necessarily limited to behavior modification. Disciplinary segregation might be used to remove a dangerous or troublesome inmate from the general inmate population in order to promote the well-being and safety of the inmate population and correctional staff, as well as facilitate the smooth operation of the institution. Arguably, these benefits could also be achieved through the use of administrative segregation, but nonetheless such benefits may be realized through the practice of disciplinary segregation as well.

Recommendations

More research should be performed in order to develop a full and accurate picture of the effect of disciplinary segregation on prison misconduct. It is especially important to understand whether disciplinary segregation has a general deterrent effect. To that end, two similar prisons could be examined, one that practices disciplinary segregation and one that does not, and then the prison rule violation rates of the two prisons could be compared and contrasted. This could also take the form of a longitudinal study, where a single prison's rule violation rates for multiple years are compared; for example, where

the rule violation rate of a past year when disciplinary segregation was practiced is compared to the rule violation rate of a year when disciplinary segregation was not practiced.

Another potential avenue for future research is that of obtaining information on disciplinary segregation directly from the inmates themselves. This could take the form of administering surveys to the inmates, or even conducting interviews. The information could be gathered from both the general inmate population, as well as from inmates who were recently released from disciplinary segregation. The inmates who had spent time in disciplinary segregation could be asked how they felt their experience will affect their subsequent behavior, and the general population inmates could be asked how they felt the possibility of being sent to disciplinary segregation impacted their behavior.

It is further recommended that this research be replicated in other jurisdictions. For example, similar studies could be carried out in other states, or within the United States Federal system, or in other countries such as Canada. In addition, in a few years a follow-up study could be conducted again on the Oregon prison system, which could yield a larger sample size since the LS/CMI began to be administered to all incoming inmates beginning in 2011. Given the results of the current study, it is possible that these future studies may reveal that the use of disciplinary segregation solely for the purpose of subsequent behavioral modification may not be justified.

Implications

The results of this study suggest that the experience of disciplinary segregation does not reduce subsequent prison misconduct among those inmates subjected to it.

Given these findings, especially in light of the possibility that the experience of disciplinary segregation may place inmates at risk of physiological and psychological harm, it would be prudent to critically evaluate the practice of disciplinary segregation within the Oregon DOC system and elsewhere. Such a critical evaluation should take into account: (1) the findings of this study, which indicate that the experience of disciplinary segregation does not effect the subsequent behavior of those subjected to it, (2) the practice of disciplinary segregation may place those inmates subjected to it at risk of negative psychological and physiological effects, (3) the other potential costs associated with the practice of disciplinary segregation (e.g., monetary costs), (4) the potential benefits of disciplinary segregation (e.g., a potential general deterrent effect). Such a cost-benefit analysis could lead to positive social change, in that it could lead to the practice of disciplinary segregation being exercised in a more judicious and informed manner, and thereby reduce the possibility of unnecessarily placing inmates at risk of psychological and physiological harm.

Conclusion

The purpose of this quantitative study was to examine the effectiveness of disciplinary segregation in deterring prison inmate misconduct within the Oregon DOC prison system. Specifically, the purpose of this retrospective observational study was to test the theory of deterrence that relates disciplinary segregation to prison misconduct. The findings of this study suggest that the experience of disciplinary segregation does not significantly affect subsequent prison inmate misconduct. These findings were consistent regardless of whether the outcome variable was overall total prison rule violations, or

total major rule violations, or total minor rule violations, and regardless of whether outliers were included or excluded from the data set. In addition, the results suggested that deterrence theory does not explain the relationship between the experience of disciplinary segregation and subsequent prison inmate misconduct. Since the findings suggest that the experience of disciplinary segregation does not decrease prison misconduct, a critical evaluation of the practice of disciplinary segregation would be prudent to undertake.

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Appendix A: Tables of Frequencies and Percentages of Outliers within the Data

Table 18

Z-Scores: Outliers Within the Data on the Variable Age

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Probable Outliers (z > 2.58)	2	.9	.9	.9
	Potential Outliers (z > 1.96)	11	4.8	4.8	5.7
	Normal range	215	94.3	94.3	100.0
	Total	228	100.0	100.0	

Table 19

Z-Scores: Outliers Within the Data on the Variable Length of Time Spent on Current Sentence

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	5	2.2	2.2	2.2
	1.95644	1	.4	.4	2.6
	Probable Outliers (z > 2.58)	4	1.8	1.8	4.4
	Potential Outliers (z > 1.96)	6	2.6	2.6	7.0
	Normal range	212	93.0	93.0	100.0
	Total	228	100.0	100.0	

Table 20

Z-Scores: Outliers Within the Data on the Variable LS/CMI Score

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Potential Outliers (z > 1.96)	9	3.9	3.9	3.9
	Normal range	219	96.1	96.1	100.0
	Total	228	100.0	100.0	

Table 21

Z-Scores: Outliers Within the Data on the Variable Total Major Rule Violations in 2011-2012

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	2	.9	.9	.9
	Probable Outliers (z > 2.58)	3	1.3	1.3	2.2
	Potential Outliers (z > 1.96)	8	3.5	3.5	5.7
	Normal range	215	94.3	94.3	100.0
	Total	228	100.0	100.0	

Table 22

Z-Scores: Outliers Within the Data on the Variable Total Minor Rule Violations in 2011-2012

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	4	1.8	1.8	1.8
	Probable Outliers (z > 2.58)	2	.9	.9	2.6
	Potential Outliers (z > 1.96)	8	3.5	3.5	6.1
	Normal range	214	93.9	93.9	100.0
	Total	228	100.0	100.0	

Table 23

Z-Scores: Outliers Within the Data on the Variable Days Spent in DSU During 2011-2012

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	3	1.3	1.3	1.3
	1.95179	1	.4	.4	1.8
	Probable Outliers (z > 2.58)	3	1.3	1.3	3.1
	Potential Outliers (z > 1.96)	2	.9	.9	3.9
	Normal range	219	96.1	96.1	100.0
	Total	228	100.0	100.0	

Table 24

Z-Scores: Outliers Within the Data on the Variable Overall Total Rule Violations in 2013-2014

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	7	3.1	3.1	3.1
	Probable Outliers (z > 2.58)	1	.4	.4	3.5
	Potential Outliers (z > 1.96)	1	.4	.4	3.9
	Normal range	219	96.1	96.1	100.0
	Total	228	100.0	100.0	

Table 25

Z-Scores: Outliers Within the Data on the Variable Total Major Rule Violations in 2013-2014

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	7	3.1	3.1	3.1
	Potential Outliers (z > 1.96)	1	.4	.4	3.5
	Normal range	220	96.5	96.5	100.0
	Total	228	100.0	100.0	

Table 26

Z-Scores: Outliers Within the Data on the Variable Total Minor Rule Violations in 2013-2014

		Frequency	Percent	Valid Percent	Cumulative Percent
Valid	Extreme Outliers (z > 3.29)	5	2.2	2.2	2.2
	Probable Outliers (z > 2.58)	1	.4	.4	2.6
	Potential Outliers (z > 1.96)	6	2.6	2.6	5.3
	Normal range	216	94.7	94.7	100.0
	Total	228	100.0	100.0	

Appendix B: Normality Statistics Concerning Dependent/Outcome Variables

Table 27

Normality Diagnostics: Full Data Set, Divided by Cohort

OUTCOME VARIABLE	COHORT			Statistic	Std. Error
Overall Total Rule Violations in 2013–2014	comparison cohort	Mean		.162	.0823
		95% Confidence Interval for Mean	Lower Bound	-.005	
			Upper Bound	.329	
		5% Trimmed Mean		.069	
		Median		.000	
		Variance		.251	
		Std. Deviation		.5008	
		Minimum		.0	
		Maximum		2.0	
		Range		2.0	
		Interquartile Range		.0	
		Skewness		3.146	.388
		Kurtosis		9.169	.759
	treatment cohort	Mean		6.168	.6043
		95% Confidence Interval for Mean	Lower Bound	4.976	
			Upper Bound	7.359	

		5% Trimmed Mean		4.916	
		Median		4.000	
		Variance		69.740	
		Std. Deviation		8.3511	
		Minimum		.0	
		Maximum		49.0	
		Range		49.0	
		Interquartile Range		7.0	
		Skewness		2.611	.176
		Kurtosis		8.187	.350
Total major rule violations in 2013-2014	comparison cohort	Mean		.1081	.06465
		95% Confidence Interval for Mean	Lower Bound	-.0230	
			Upper Bound	.2392	
		5% Trimmed Mean		.0345	
		Median		.0000	
		Variance		.155	
		Std. Deviation		.39326	
		Minimum		.00	
		Maximum		2.00	
		Range		2.00	
		Interquartile Range		.00	
		Skewness		3.934	.388
		Kurtosis		16.055	.759

	treatment cohort	Mean		5.3665	.54660
		95% Confidence Interval for Mean	Lower Bound	4.2883	
			Upper Bound	6.4447	
		5% Trimmed Mean		4.2182	
		Median		3.0000	
		Variance		57.065	
		Std. Deviation		7.55414	
		Minimum		.00	
		Maximum		47.00	
		Range		47.00	
		Interquartile Range		7.00	
		Skewness		2.778	.176
		Kurtosis		9.621	.350
Total minor rule violations in 2013-2014	comparison cohort	Mean		.0541	.05405
		95% Confidence Interval for Mean	Lower Bound	-.0556	
			Upper Bound	.1637	
		5% Trimmed Mean		.0000	
		Median		.0000	
		Variance		.108	
		Std. Deviation		.32880	
		Minimum		.00	
		Maximum		2.00	
		Range		2.00	

	Interquartile Range		.00	
	Skewness		6.083	.388
	Kurtosis		37.000	.759
treatment cohort	Mean		.8010	.10583
	95% Confidence Interval for Mean	Lower Bound	.5923	
		Upper Bound	1.0098	
	5% Trimmed Mean		.5748	
	Median		.0000	
	Variance		2.139	
	Std. Deviation		1.46259	
	Minimum		.00	
	Maximum		8.00	
	Range		8.00	
	Interquartile Range		1.00	
	Skewness		2.605	.176
	Kurtosis		7.650	.350

Table 28

Normality Diagnostics: Data Set with Outliers Deleted, Divided by Cohort

OUTCOME VARIABLE	COHORT			Statistic	Std. Error
Overall total rule violations in 2013-2014	comparison cohort	Mean		.162	.0823
		95% Confidence Interval for Mean	Lower Bound	-.005	
			Upper Bound	.329	
		5% Trimmed Mean		.069	
		Median		.000	
		Variance		.251	
		Std. Deviation		.5008	
		Minimum		.0	
		Maximum		2.0	
		Range		2.0	
		Interquartile Range		.0	
		Skewness		3.146	.388
		Kurtosis		9.169	.759
	treatment cohort	Mean		4.913	.3919
		95% Confidence Interval for Mean	Lower Bound	4.140	
			Upper Bound	5.686	
		5% Trimmed Mean		4.371	

		Median		3.000	
		Variance		28.255	
		Std. Deviation		5.3155	
		Minimum		.0	
		Maximum		26.0	
		Range		26.0	
		Interquartile Range		6.8	
		Skewness		1.339	.179
		Kurtosis		1.533	.356
Total major rule violations in 2013-2014	comparison cohort	Mean		.1081	.06465
		95% Confidence Interval for Mean	Lower Bound	-.0230	
			Upper Bound	.2392	
		5% Trimmed Mean		.0345	
		Median		.0000	
		Variance		.155	
		Std. Deviation		.39326	
		Minimum		.00	
		Maximum		2.00	
		Range		2.00	
		Interquartile Range		.00	
		Skewness		3.934	.388
		Kurtosis		16.055	.759
	treatment cohort	Mean		4.2174	.34351
		95% Confidence Interval	Lower Bound	3.5396	

		for Mean	Upper Bound	4.8951	
		5% Trimmed Mean		3.7826	
		Median		3.0000	
		Variance		21.712	
		Std. Deviation		4.65962	
		Minimum		.00	
		Maximum		23.00	
		Range		23.00	
		Interquartile Range		6.00	
		Skewness		1.269	.179
		Kurtosis		1.299	.356
Total minor rule violations in 2013-2014	comparison cohort	Mean		.0541	.05405
		95% Confidence Interval for Mean	Lower Bound	-.0556	
			Upper Bound	.1637	
		5% Trimmed Mean		.0000	
		Median		.0000	
		Variance		.108	
		Std. Deviation		.32880	
		Minimum		.00	
		Maximum		2.00	
		Range		2.00	
		Interquartile Range		.00	
		Skewness		6.083	.388

	Kurtosis		37.000	.759
treatment cohort	Mean		.6344	.07759
	95% Confidence Interval for Mean	Lower Bound	.4813	
		Upper Bound	.7875	
	5% Trimmed Mean		.4904	
	Median		.0000	
	Variance		1.120	
	Std. Deviation		1.05815	
	Minimum		.00	
	Maximum		5.00	
	Range		5.00	
	Interquartile Range		1.00	
	Skewness		1.883	.178
	Kurtosis		3.202	.355

Appendix C: Results of Kolmogorov-Smirnov and Shapiro-Wilk Normality Tests

Table 29

Tests of Normality on the Data Set With Outliers Deleted

	Cohort	Kolmogorov-Smirnov[a]			Shapiro-Wilk		
		Statistic	df	Sig.	Statistic	df	Sig.
Overall total rule violations in 2013-2014	comparison cohort	.519	37	.000	.363	37	.000
	treatment cohort	.178	184	.000	.846	184	.000
Total major rule violations in 2013-2014	comparison cohort	.527	37	.000	.307	37	.000
	treatment cohort	.183	184	.000	.844	184	.000
Total minor rule violations in 2013-2014	comparison cohort	.538	37	.000	.155	37	.000
	treatment cohort	.371	186	.000	.655	186	.000
Age	comparison cohort	.084	37	.200*	.962	37	.237
	treatment cohort	.113	191	.000	.950	191	.000
Length of time spent incarcerated on current sentence	comparison cohort	.214	37	.000	.796	37	.000
	treatment cohort	.185	184	.000	.778	184	.000
LS/CMI score	comparison cohort	.100	37	.200	.973	37	.499
	treatment cohort	.063	191	.062	.983	191	.019

Length of time spent in DSU during 2011-2012[b]	treatment cohort	.121	187	.000	.917	187	.000
Total major rule violations in 2011-2012[c]	treatment cohort	.122	188	.000	.913	188	.000
Total minor rule violations in 2011-2012	comparison cohort	.538	37	.000	.155	37	.000
	treatment cohort	.296	187	.000	.697	187	.000

Note. df = degrees of freedom; Sig. = significance.

*. This is a lower bound of the true significance.

a. Lilliefors Significance Correction.

b. Length of time spent in DSU during 2011-2012 is constant when cohort = comparison cohort. It has been omitted.

c. Total major rule violations in 2011-2012 is constant when cohort = comparison cohort. It has been omitted.

Table 30

Tests of Normality on the Full Data Set

	Cohort	Kolmogorov-Smirnov[a]			Shapiro-Wilk		
		Statistic	df	Sig.	Statistic	df	Sig.
Overall total rule violations in 2013-2014	comparison cohort	.519	37	.000	.363	37	.000
	treatment cohort	.230	191	.000	.704	191	.000
Total major rule violations in 2013-2014	comparison cohort	.527	37	.000	.307	37	.000
	treatment cohort	.239	191	.000	.688	191	.000

Total minor rule violations in 2013-2014	comparison cohort	.538	37	.000	.155	37	.000
	treatment cohort	.336	191	.000	.608	191	.000
Age	comparison cohort	.084	37	.200*	.962	37	.237
	treatment cohort	.113	191	.000	.950	191	.000
Length of time spent on current sentence	comparison cohort	.214	37	.000	.796	37	.000
	treatment cohort	.215	191	.000	.714	191	.000
LS/CMI score	comparison cohort	.100	37	.200*	.973	37	.499
	treatment cohort	.063	191	.062	.983	191	.019
Length of time spent in DSU during 2011-2012[b]	treatment cohort	.140	191	.000	.881	191	.000
Total major rule violations in 2011-2012[c]	treatment cohort	.132	191	.000	.875	191	.000
Total minor rule violations in 2011-2012	comparison cohort	.538	37	.000	.155	37	.000
	treatment cohort	.295	191	.000	.644	191	.000

Note. df = degrees of freedom; Sig. = significance.

*. This is a lower bound of the true significance.

a. Lilliefors Significance Correction.

b. Length of time spent in DSU during 2011-2012 is constant when cohort = comparison cohort. It has been omitted.

c. Total major rule violations in 2011-2012 is constant when cohort = comparison cohort. It has been omitted.

Appendix D: Histograms Depicting the Dependent/Outcome Variables and the Standardized Residuals

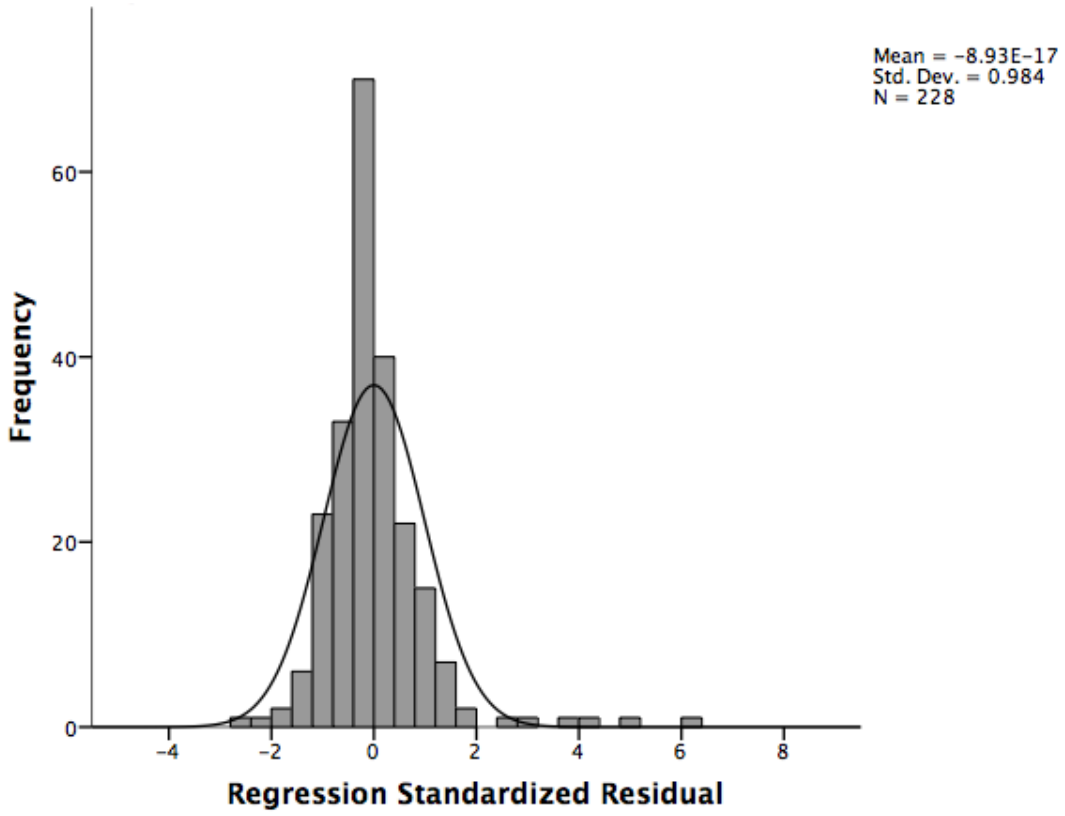

Figure 1. A histogram of the outcome variable of Overall Total Rule Violations in 2013-2014, using the full data set.

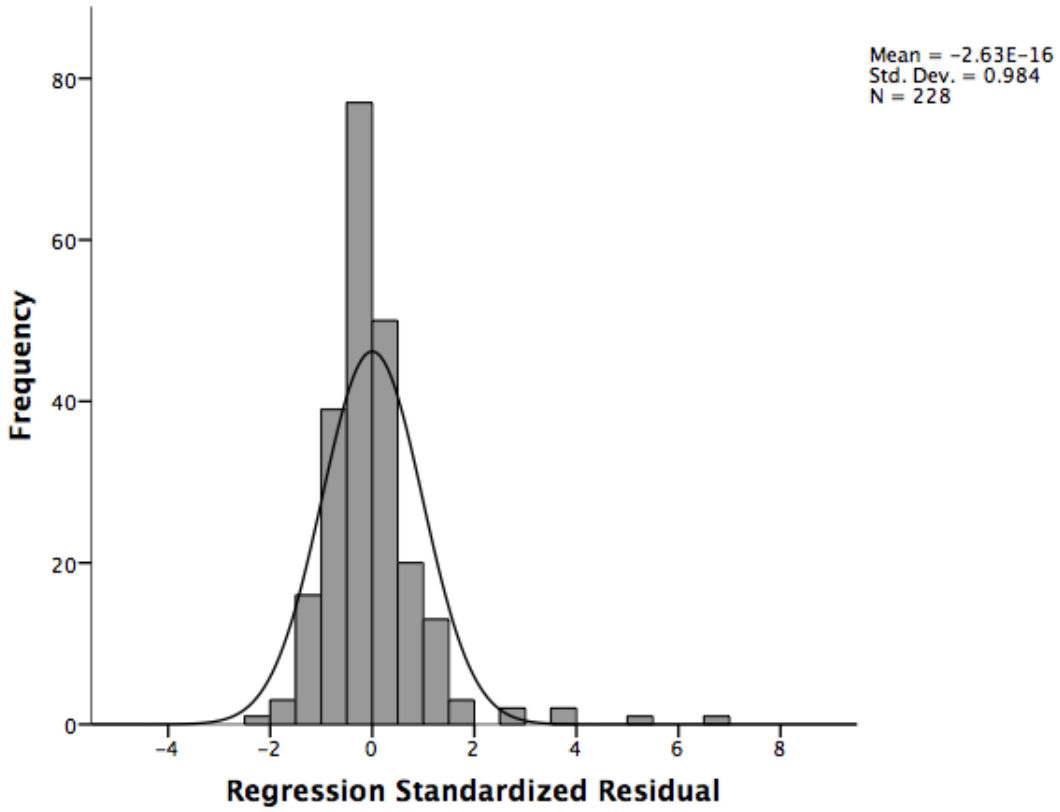

Figure 2. A histogram of the outcome variable of Total Major Rule Violations in 2013-2014, using the full data set.

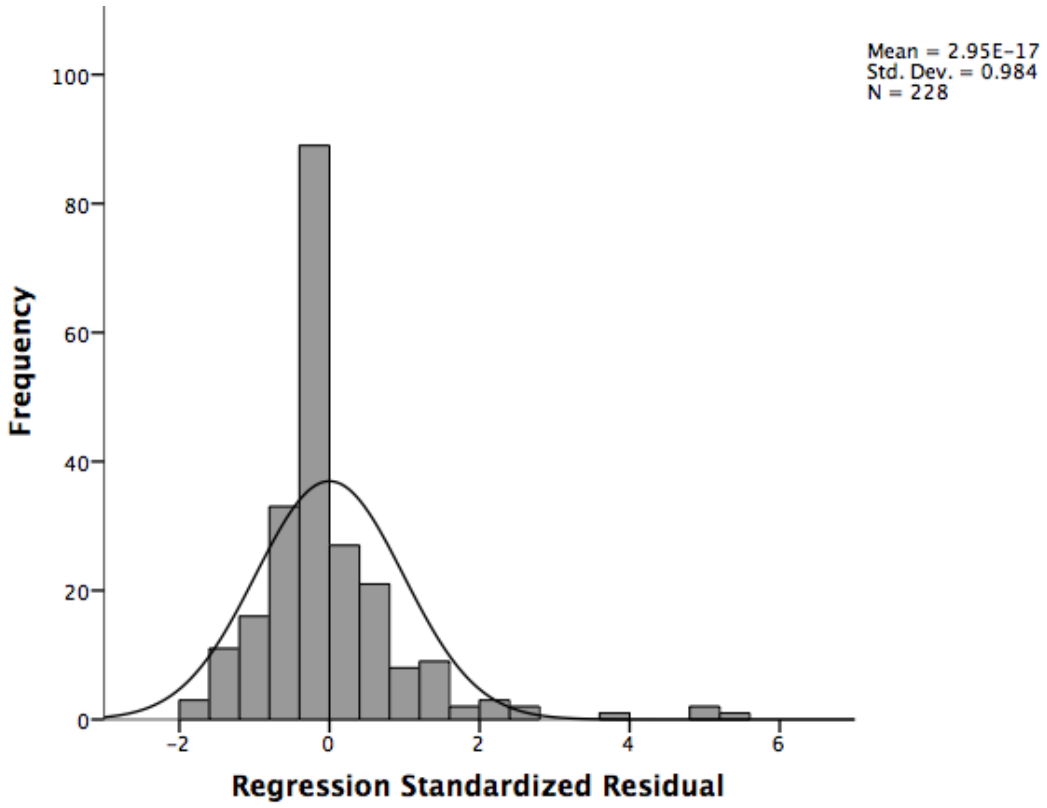

Figure 3. A histogram of the outcome variable of Total Minor Rule Violations in 2013-2014, using the full data set.

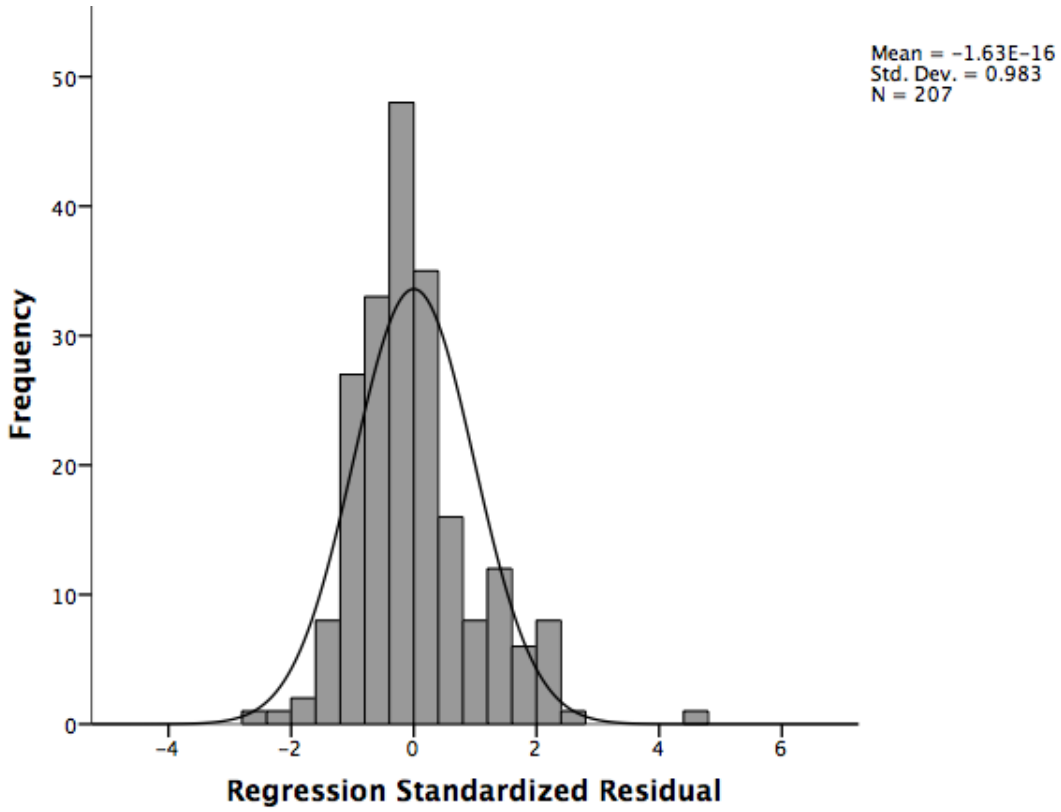

Figure 4. A histogram of the outcome variable of Overall Total Rule Violations in 2013-2014, using the data set with outliers removed.

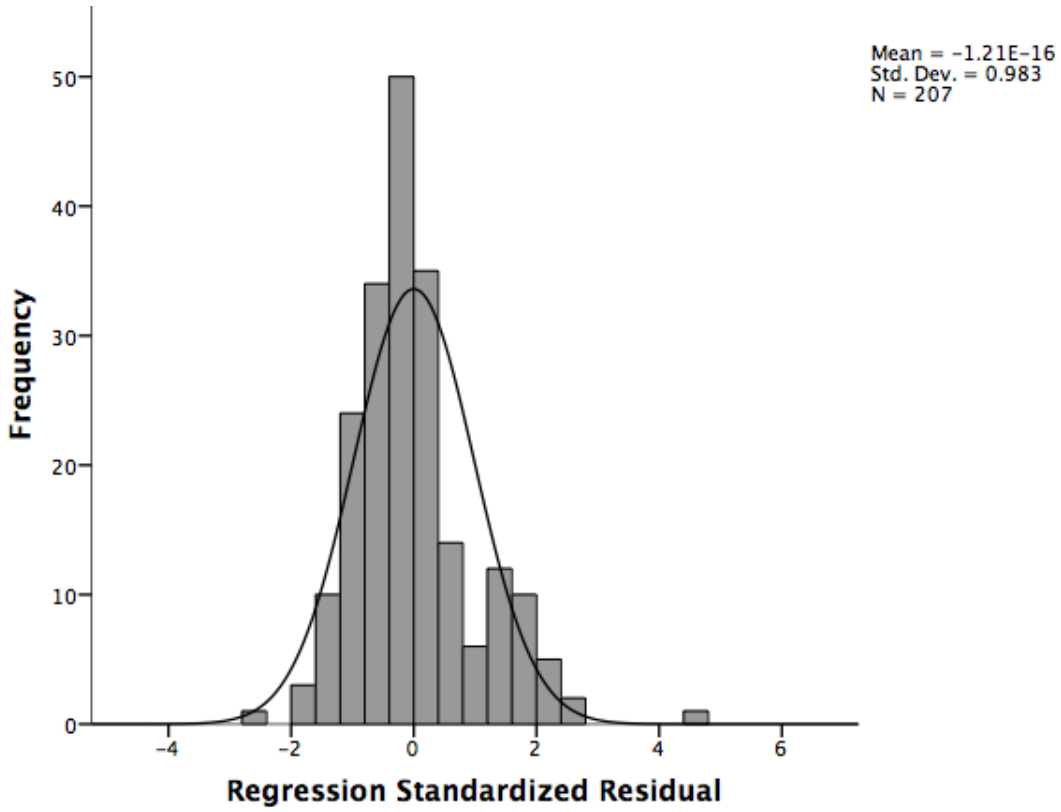

Figure 5. A histogram of the outcome variable of Total Major Rule Violations in 2013-2014, using the data set with outliers removed.

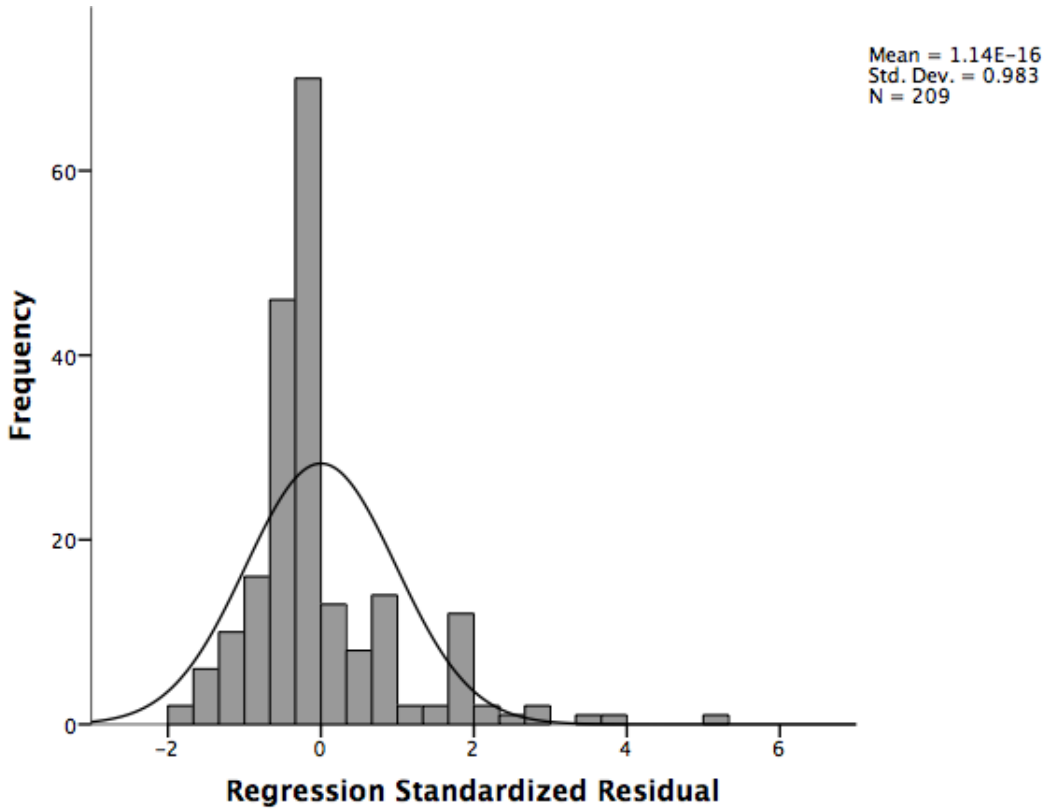

Figure 6. A histogram of the outcome variable of Total Minor Rule Violations in 2013-2014, using the data set with outliers removed.

Appendix E: Normal Probability-Probability Plots of Regression Standardized Residuals

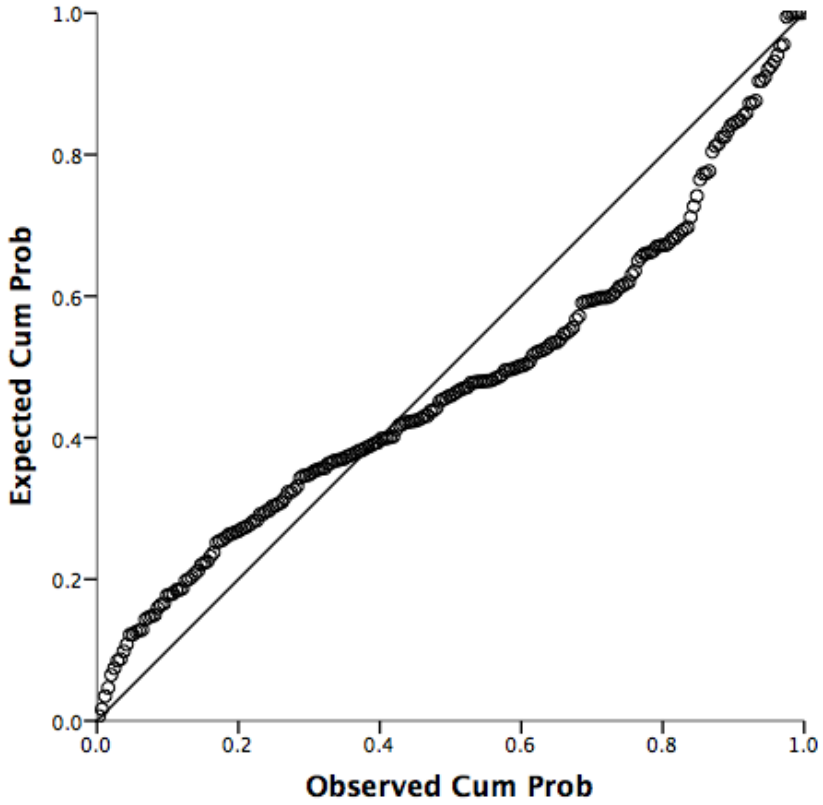

Figure 7. A normal P-P plot of the regression standardized residual with the outcome variable of Overall Total Rule Violations in 2013-2014, using the full data set.

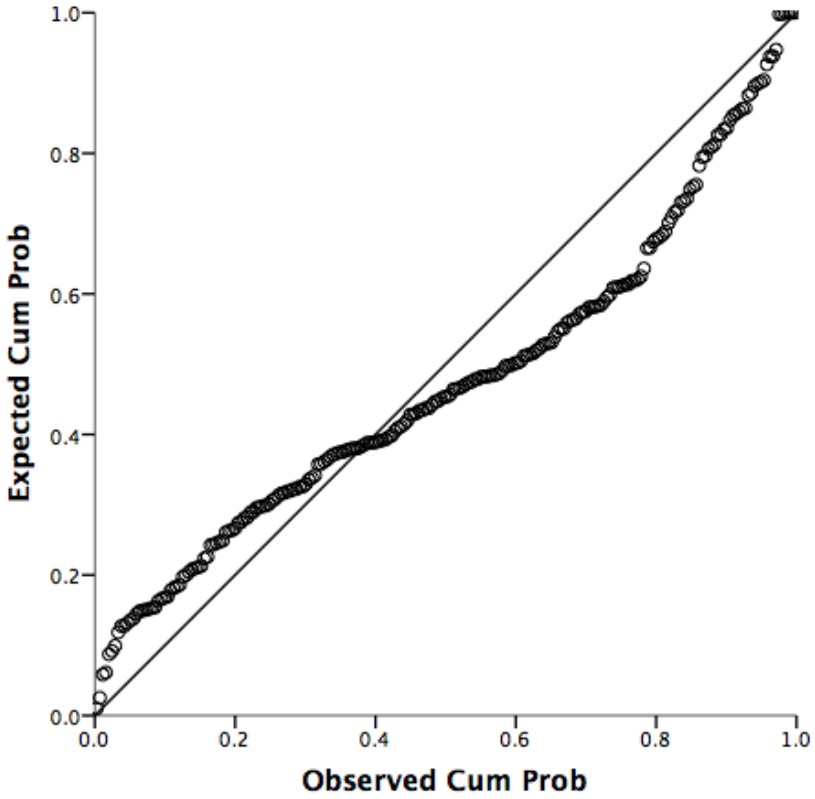

Figure 8. A normal P-P plot of the regression standardized residual with the outcome variable of Total Major Rule Violations in 2013-2014, using the full data set.

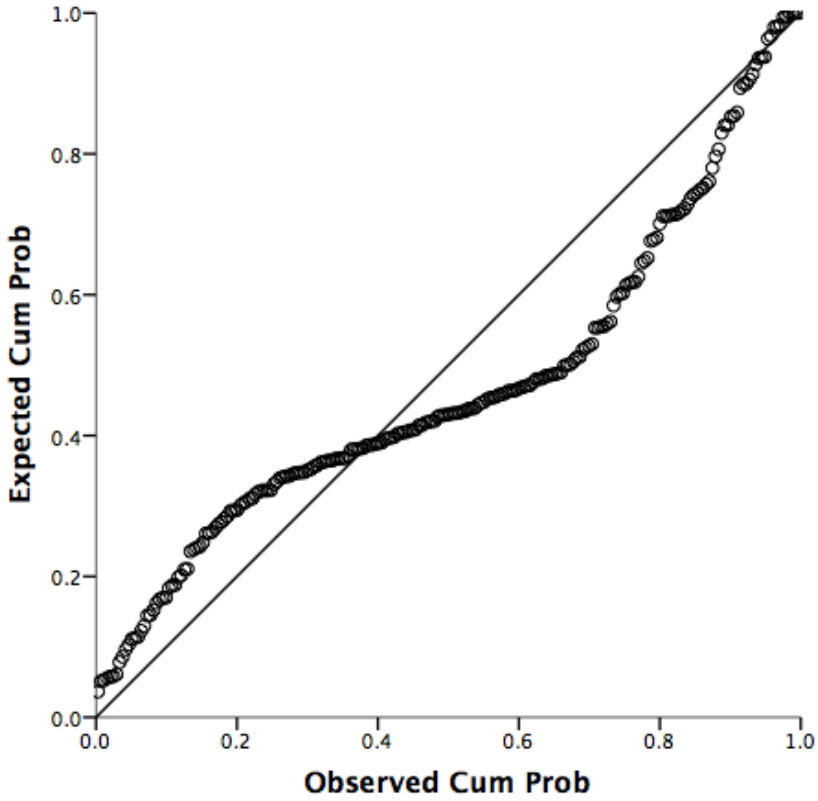

Figure 9. A normal P-P plot of the regression standardized residual with the outcome variable of Total Minor Rule Violations in 2013-2014, using the full data set.

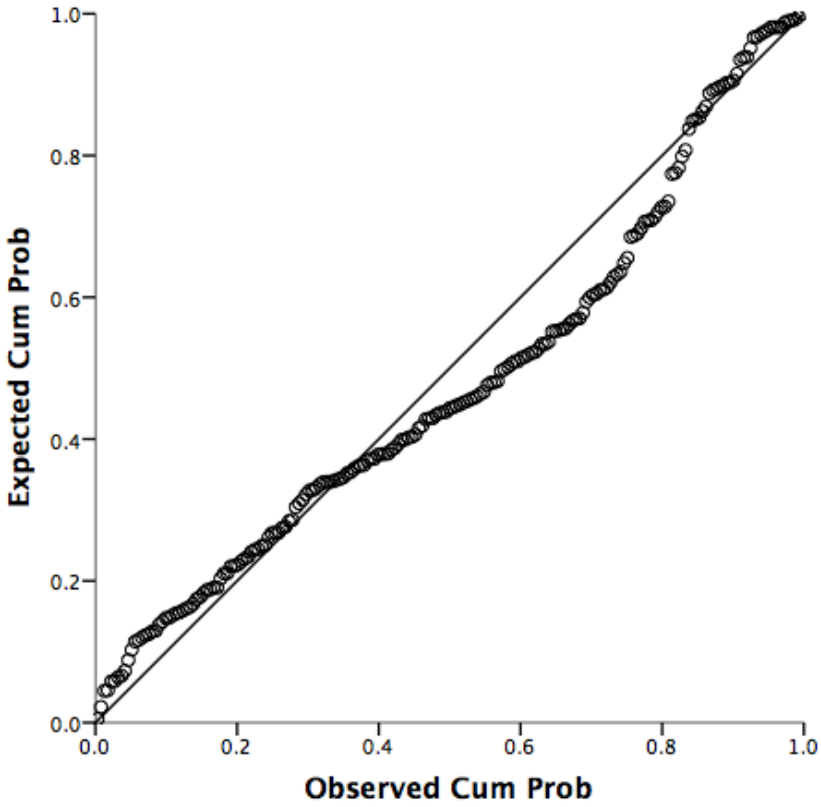

Figure 10. A normal P-P plot of the regression standardized residual with the outcome variable of Overall Total Rule Violations in 2013-2014, using the data set with outliers removed.

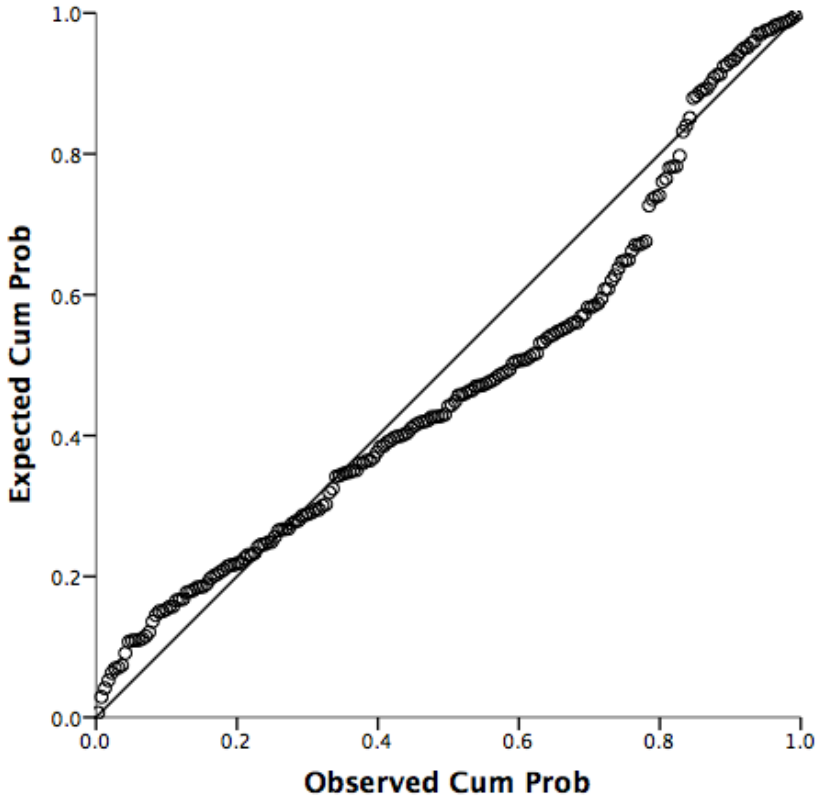

Figure 11. A normal P-P plot of the regression standardized residual with the outcome variable of Total Major Rule Violations in 2013-2014, using the data set with outliers removed.

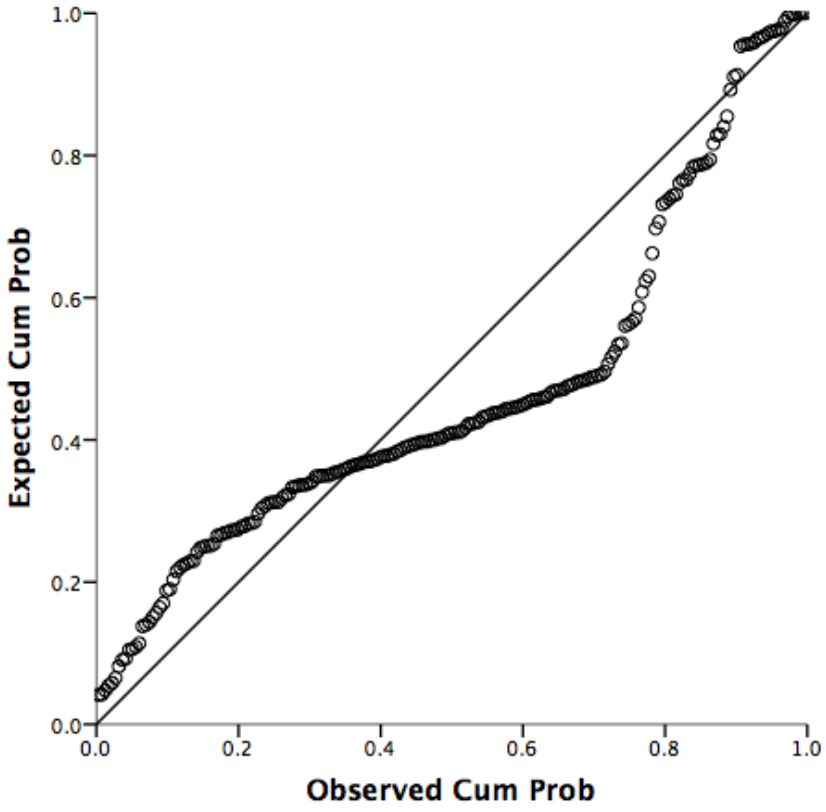

Figure 12. A normal P-P plot of the regression standardized residual with the outcome variable of Total Minor Rule Violations in 2013-2014, using the data set with outliers removed.

Appendix F: Scatterplot and Partial Plots of Data With Outcome Variable Overall Total Rule Violations in 2013-2014, Outliers Removed

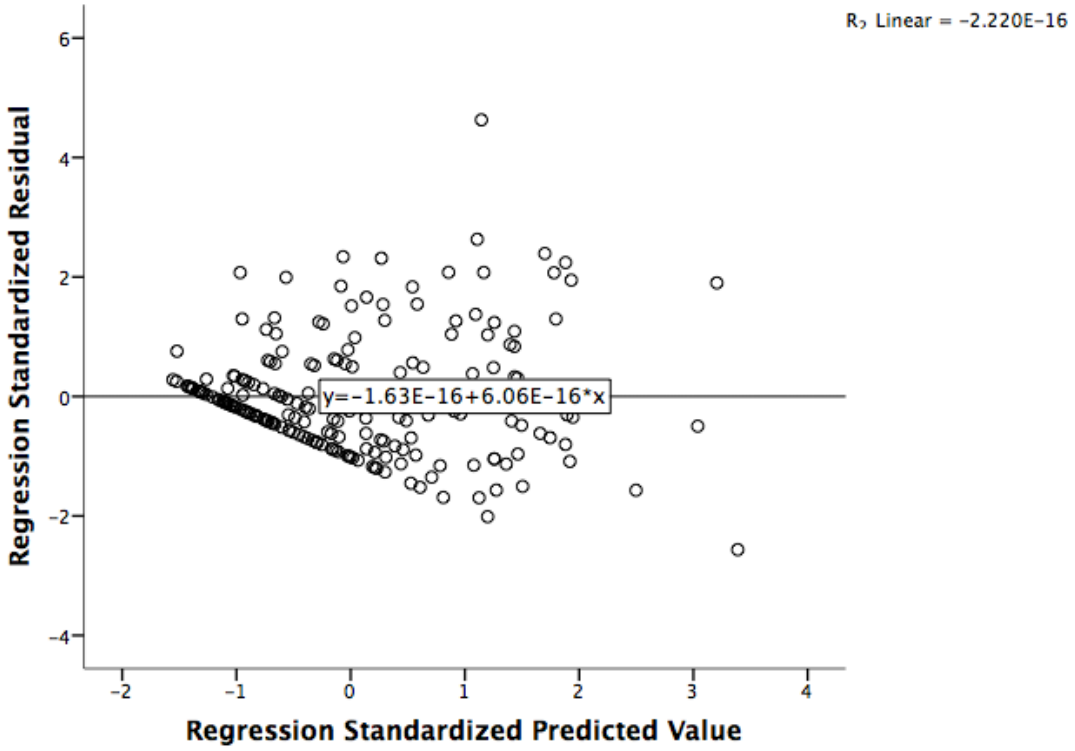

Figure 13. A scatterplot with the outcome variable of Overall Total Rule Violations in 2013-2014, using the data set with outliers removed.

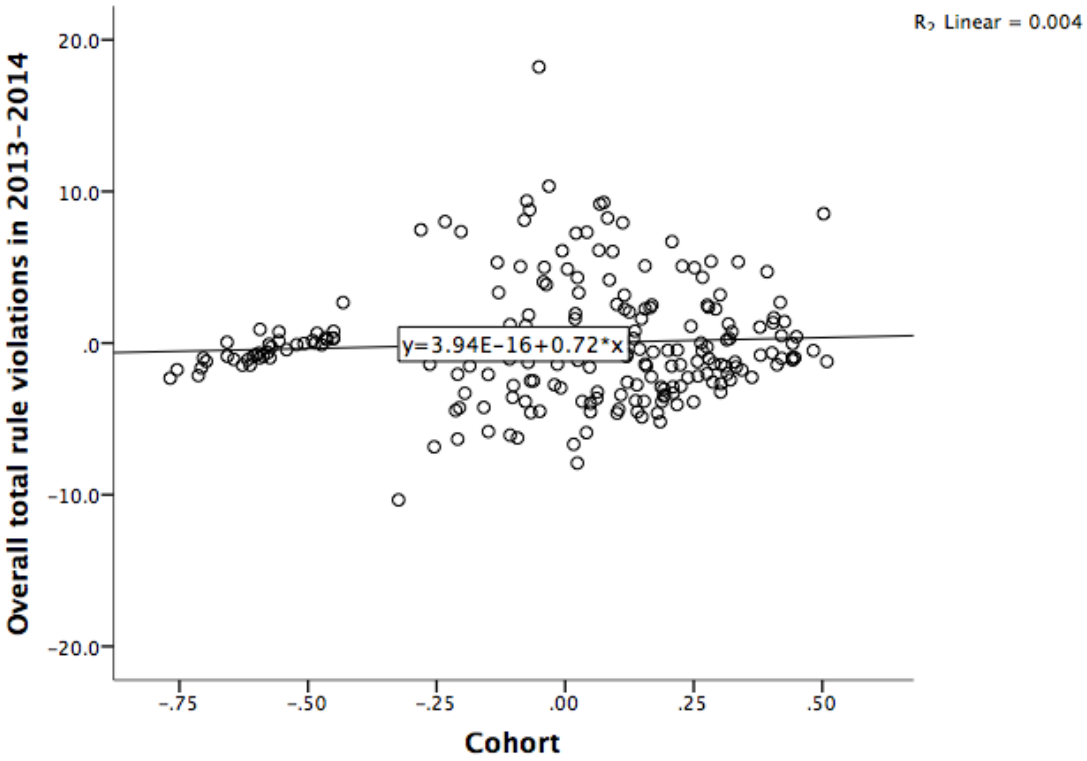

Figure 14. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Cohort, using the data set with outliers removed.

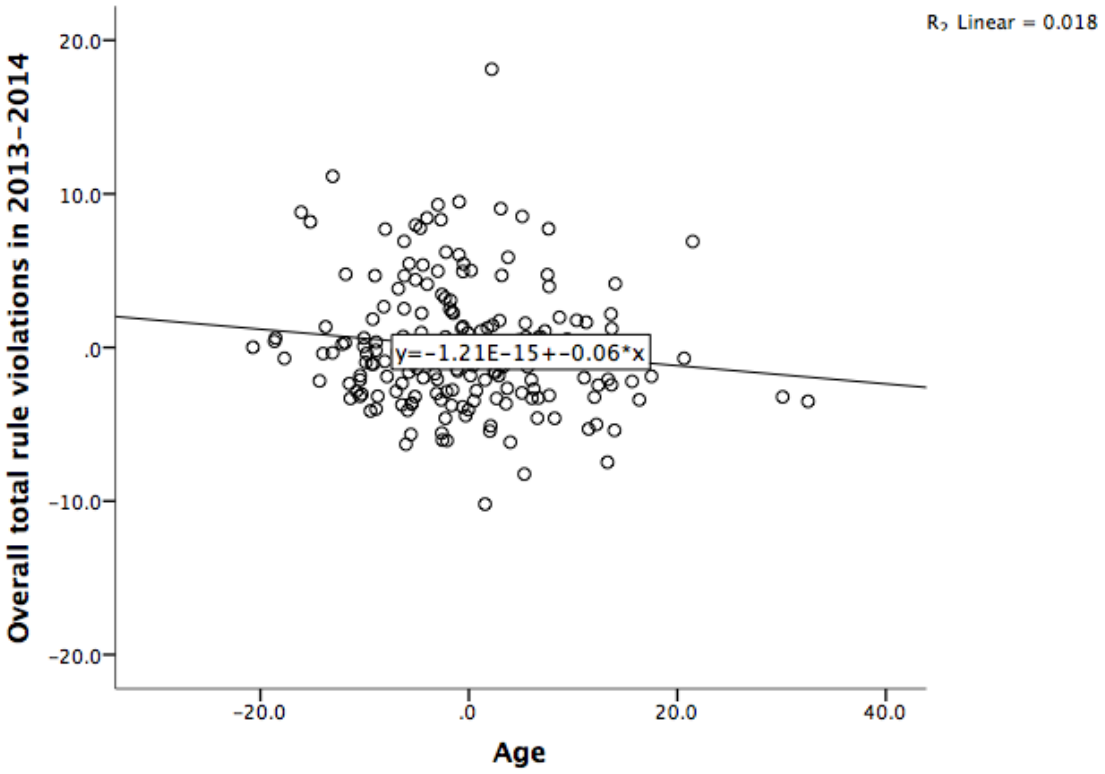

Figure 15. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Age, using the data set with outliers removed.

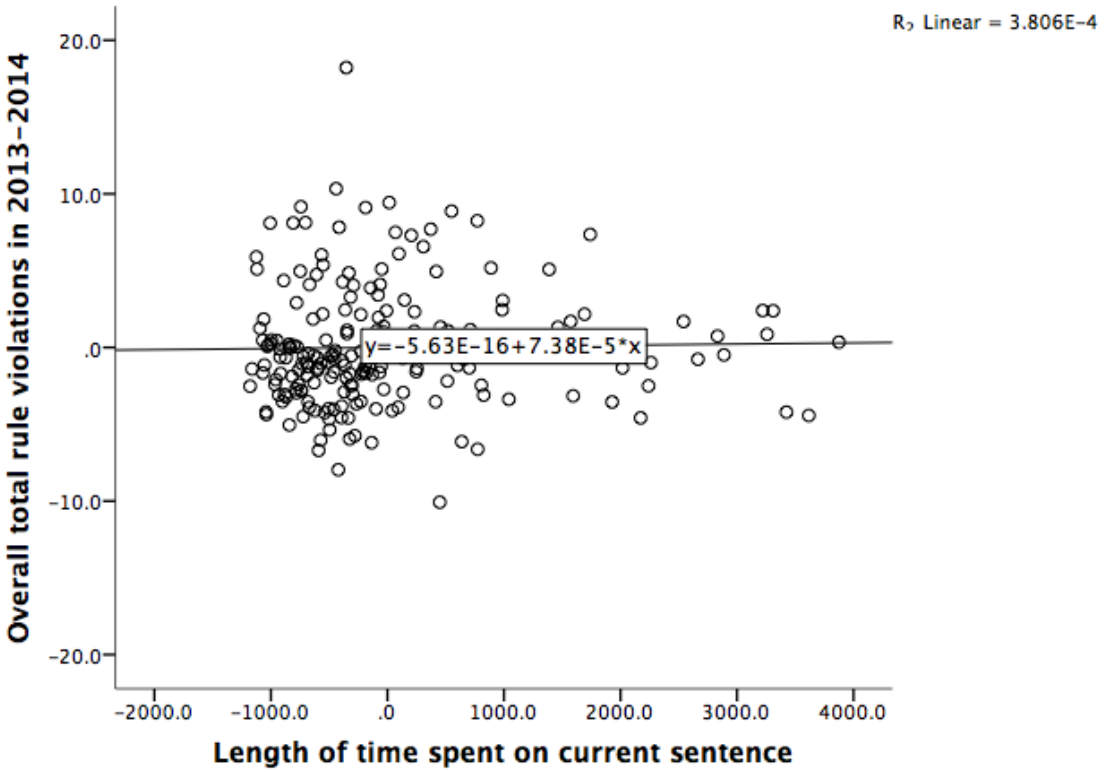

Figure 16. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Length of Time Spent on Current Sentence, using the data set with outliers removed.

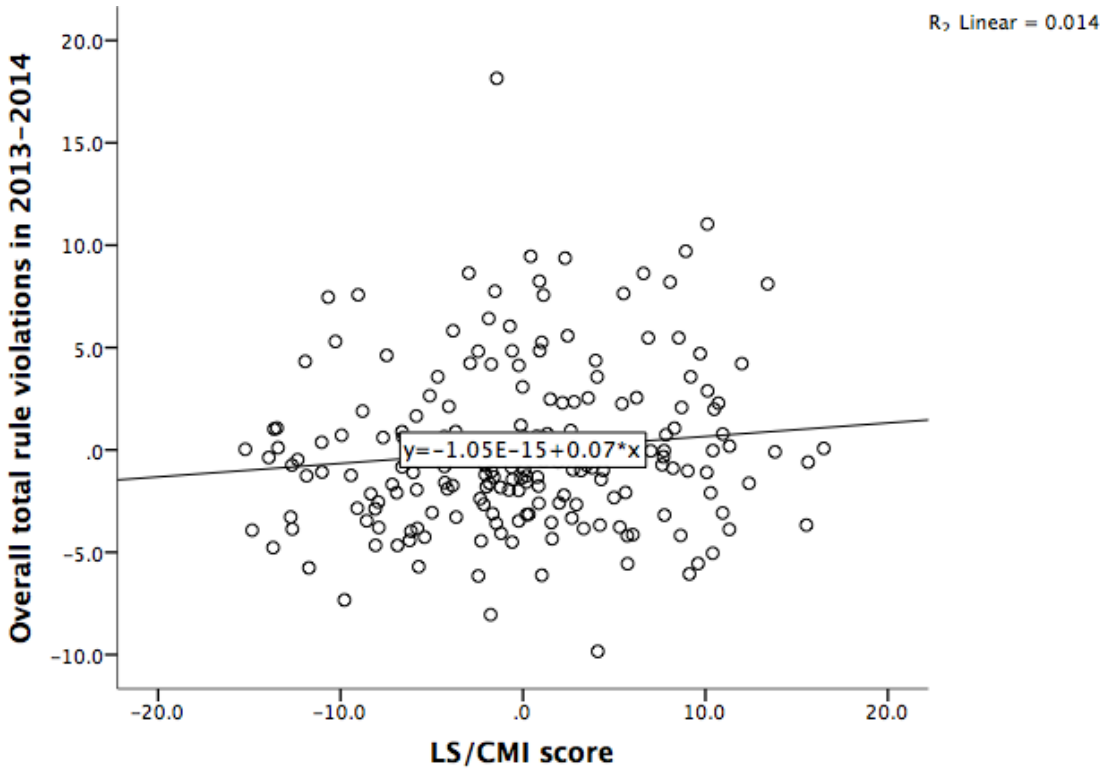

Figure 17. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and LS/CMI Score, using the data set with outliers removed.

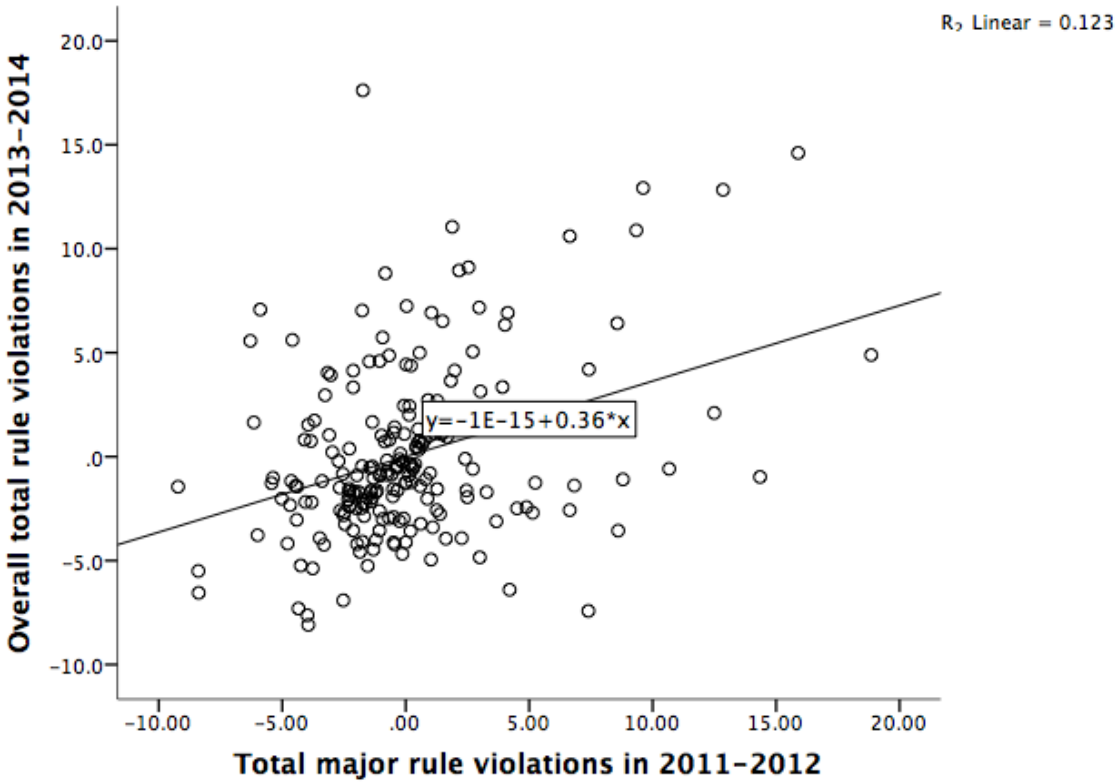

Figure 18. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Prior Major Rule Violations in 2011-2012, using the data set with outliers removed.

Figure 19. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Prior Minor Rule Violations in 2011-2012, using the data set with outliers removed.

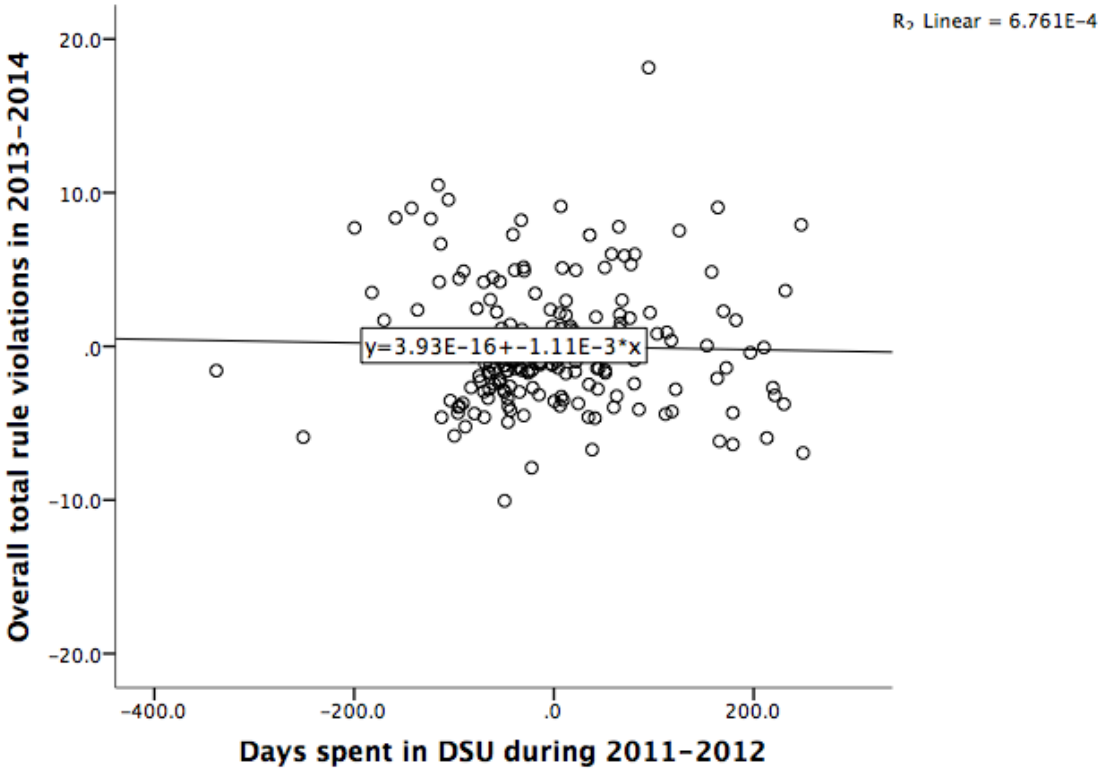

Figure 20. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Length of Time Spent in Disciplinary Segregation in 2011-2012, using the data set with outliers removed.

Appendix G: Scatterplot and Partial Plots of Data With Outcome Variable Overall Total Rule Violations in 2013-2014, Full Data Set

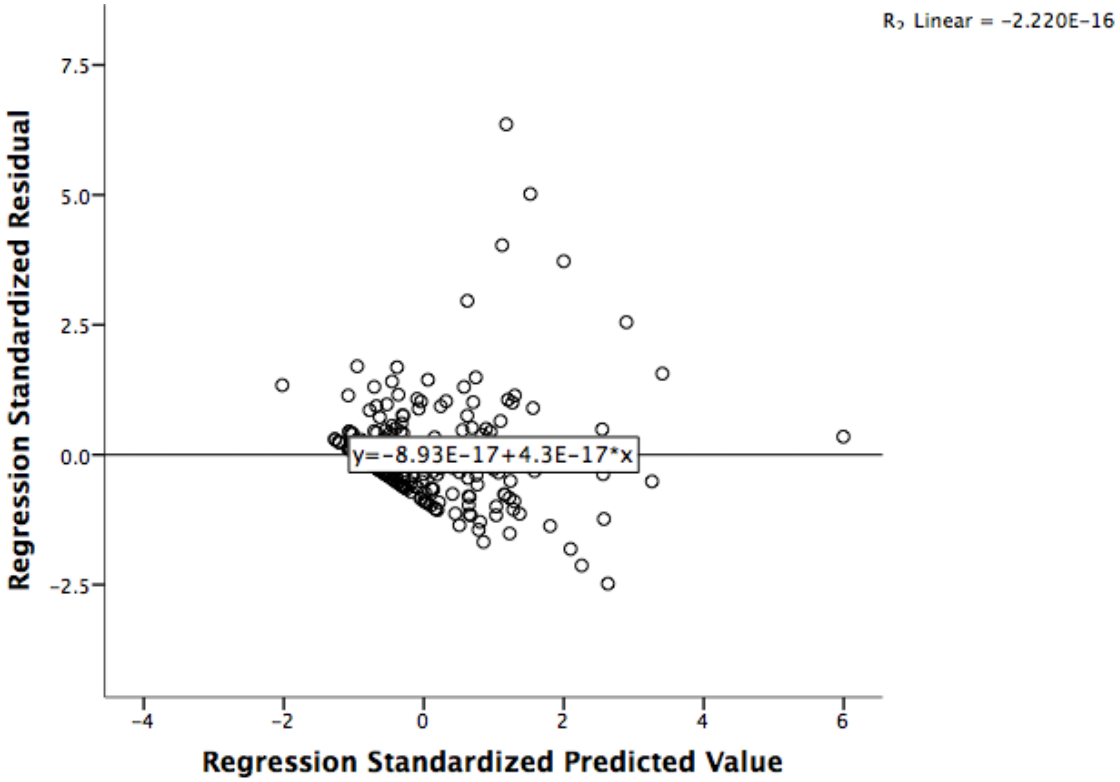

Figure 21. A scatterplot with the outcome variable of Overall Total Rule Violations in 2013-2014, using the full data set.

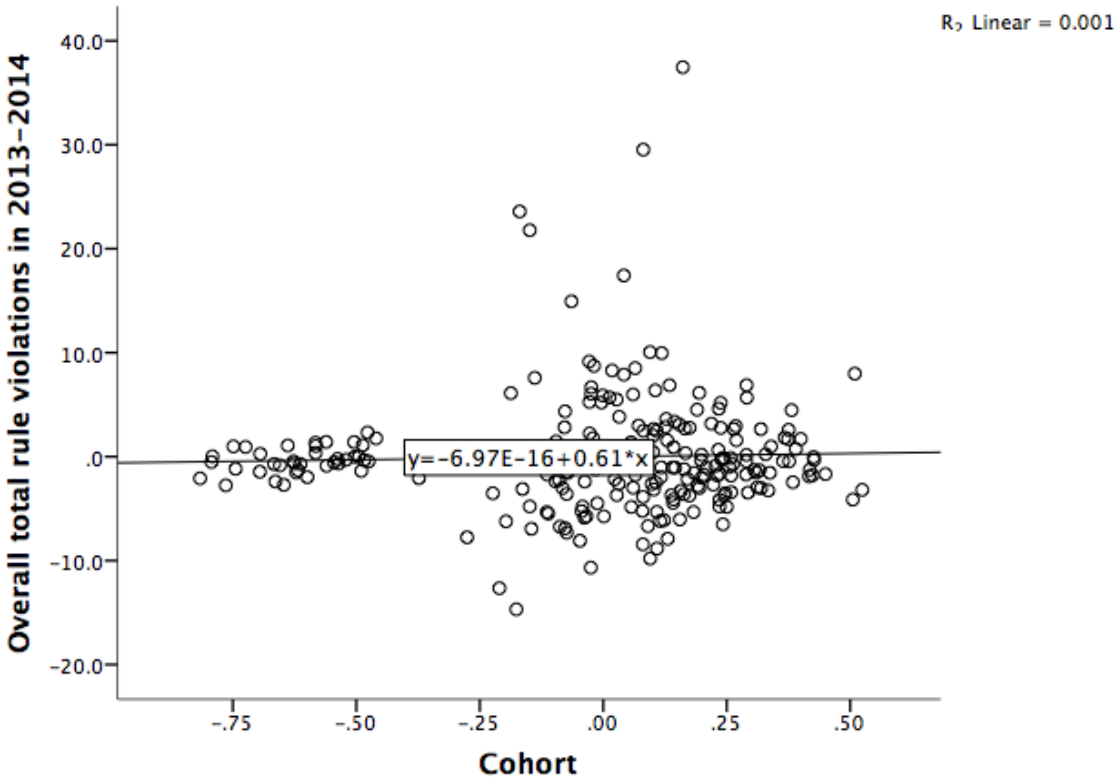

Figure 22. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Cohort, using the full data set.

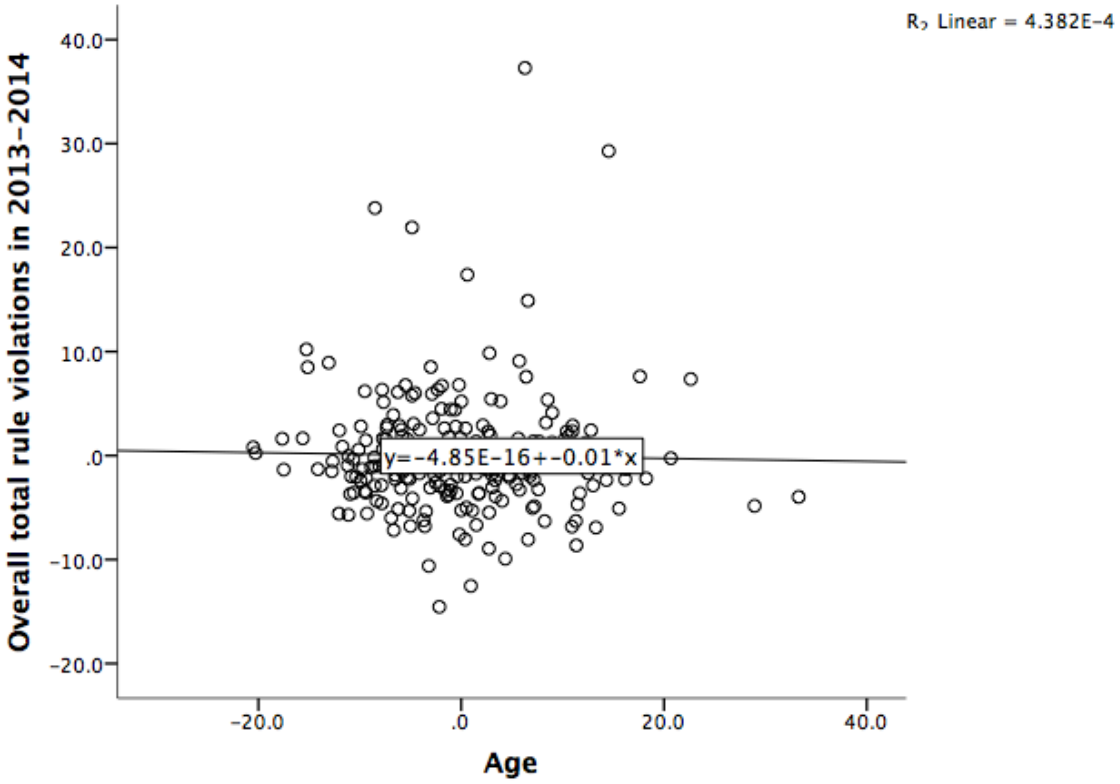

Figure 23. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Age, using the full data set.

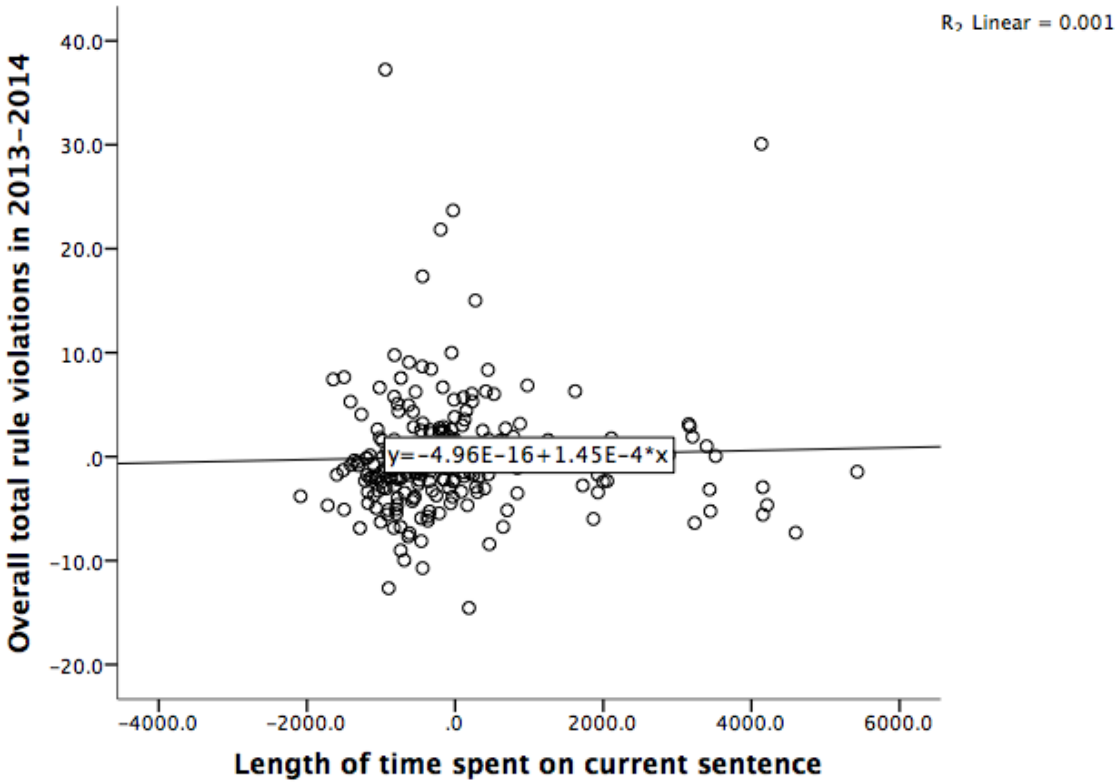

Figure 24. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Length of Time Spent on Current Sentence, using the full data set.

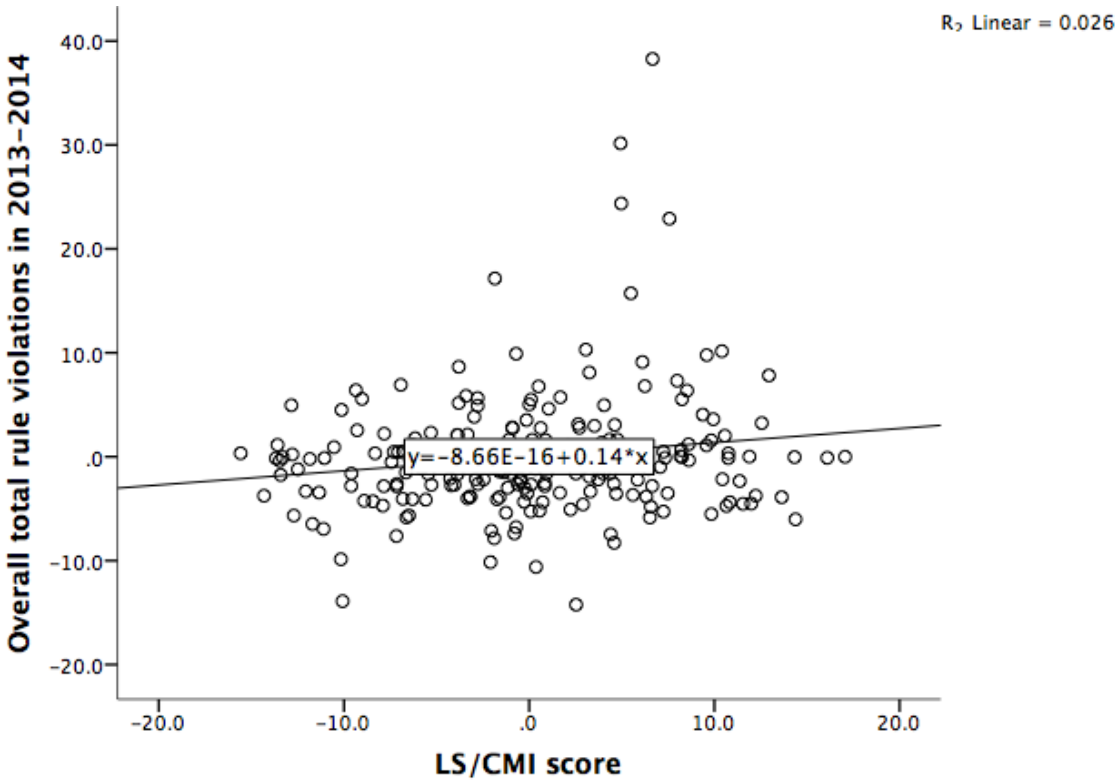

Figure 25. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and LS/CMI Score, using the full data set.

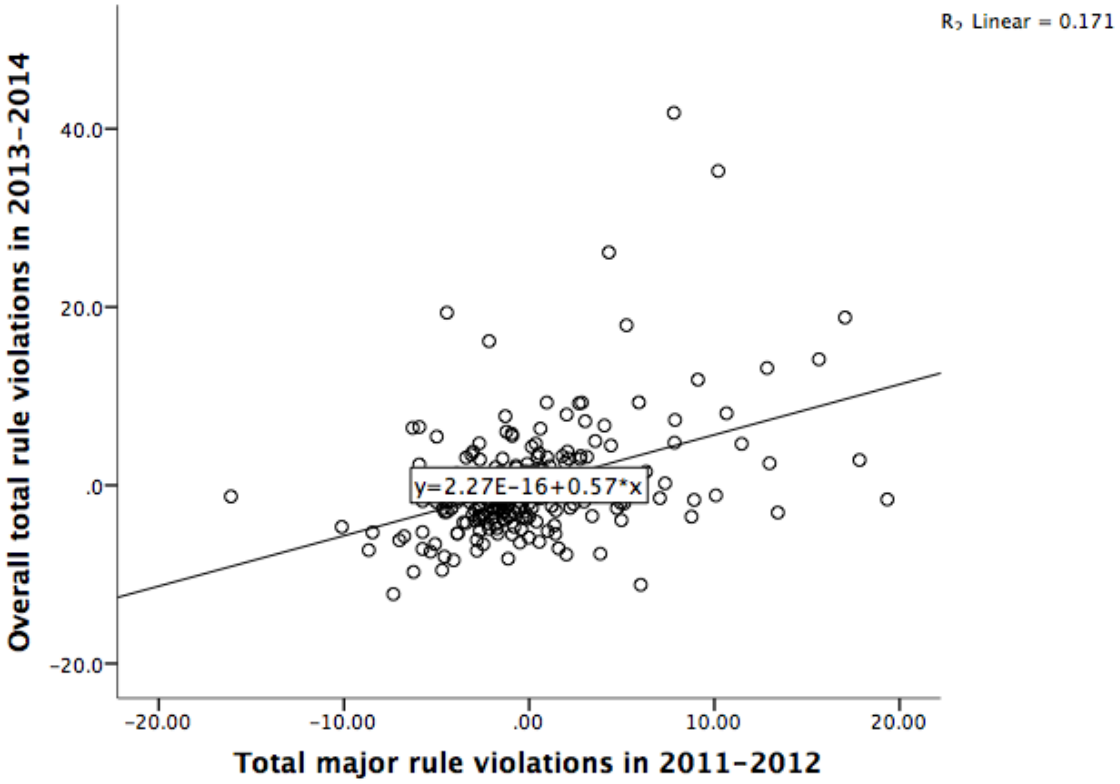

Figure 26. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Prior Major Rule Violations in 2011-2012, using the full data set.

Figure 27. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Prior Minor Rule Violations in 2011-2012, using the full data set.

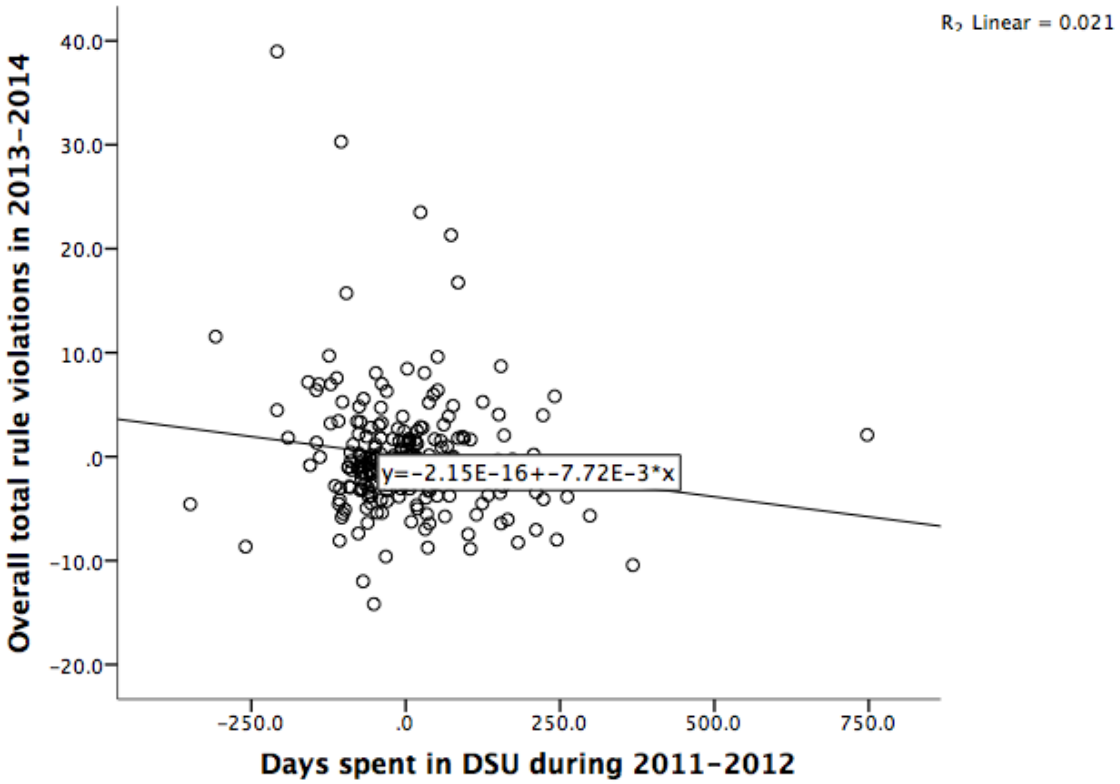

Figure 28. A partial plot with the variables Overall Total Rule Violations in 2013-2014 and Length of Time Spent in Disciplinary Segregation in 2011-2012, using the full data set.

Appendix H: Scatterplot and Partial Plots of Data With Outcome Variable Total Major Rule Violations in 2013-2014, Outliers Removed

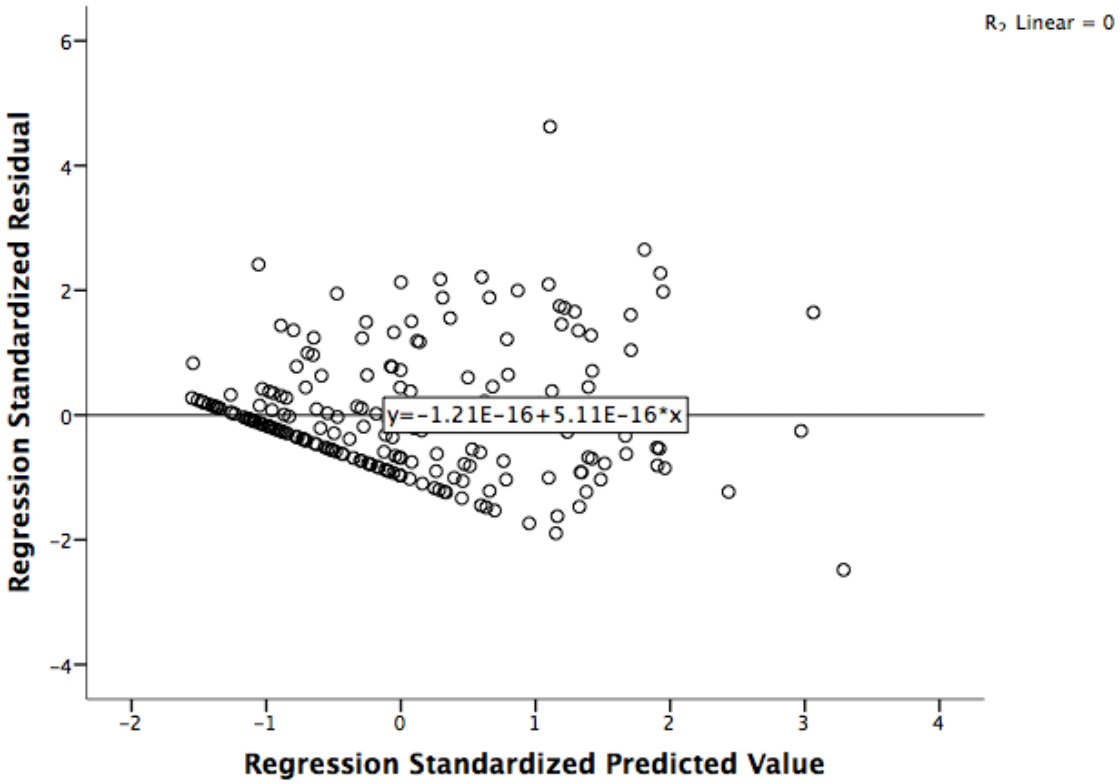

Figure 29. A scatterplot with the outcome variable of Total Major Rule Violations in 2013-2014, using the data set with outliers removed.

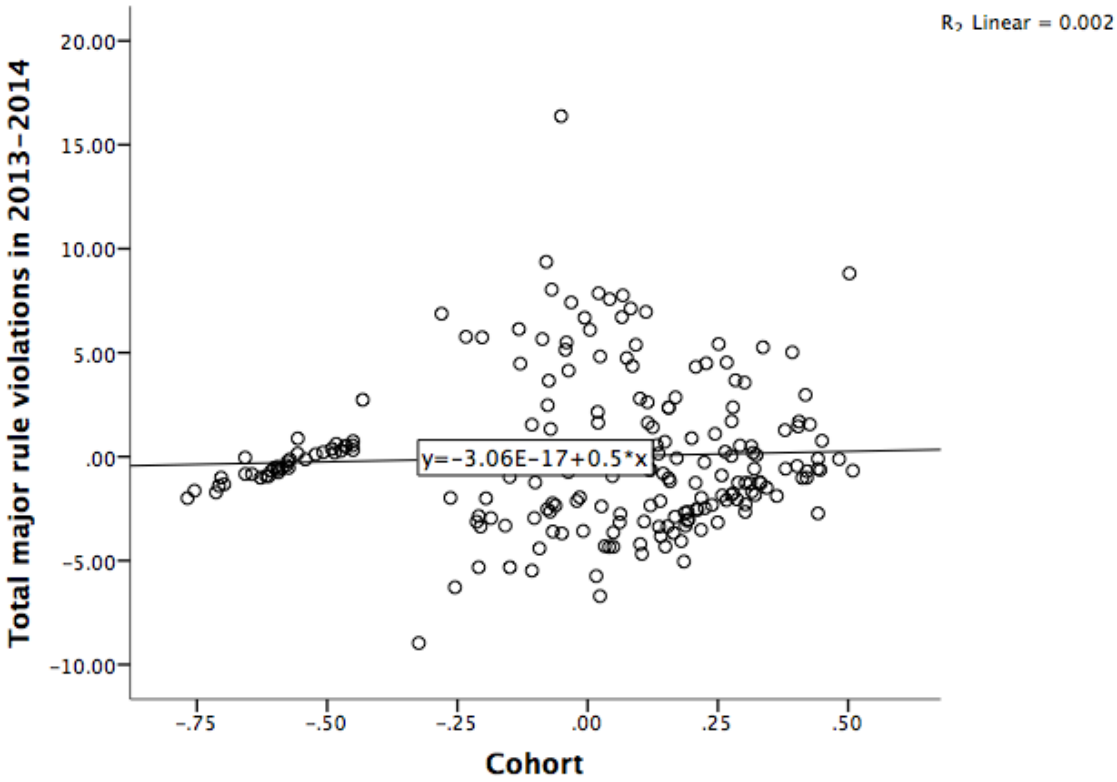

Figure 30. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Cohort, using the data set with outliers removed.

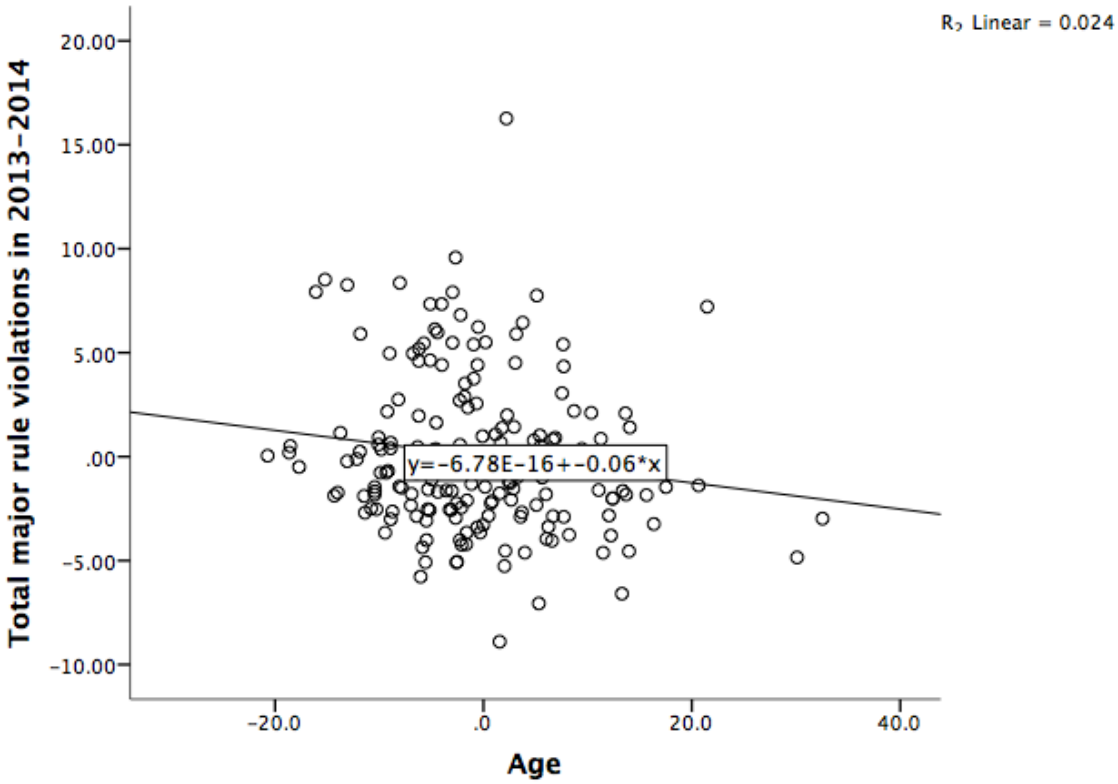

Figure 31. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Age, using the data set with outliers removed.

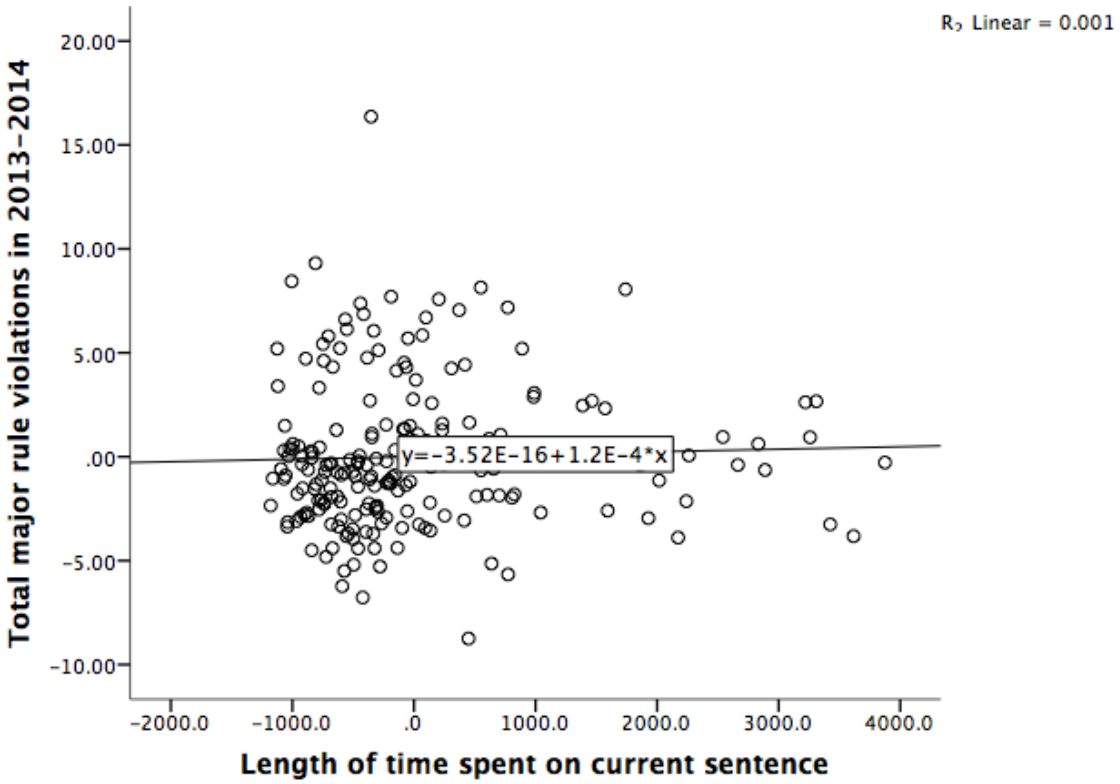

Figure 32. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Length of Time Spent on Current Sentence, using the data set with outliers removed.

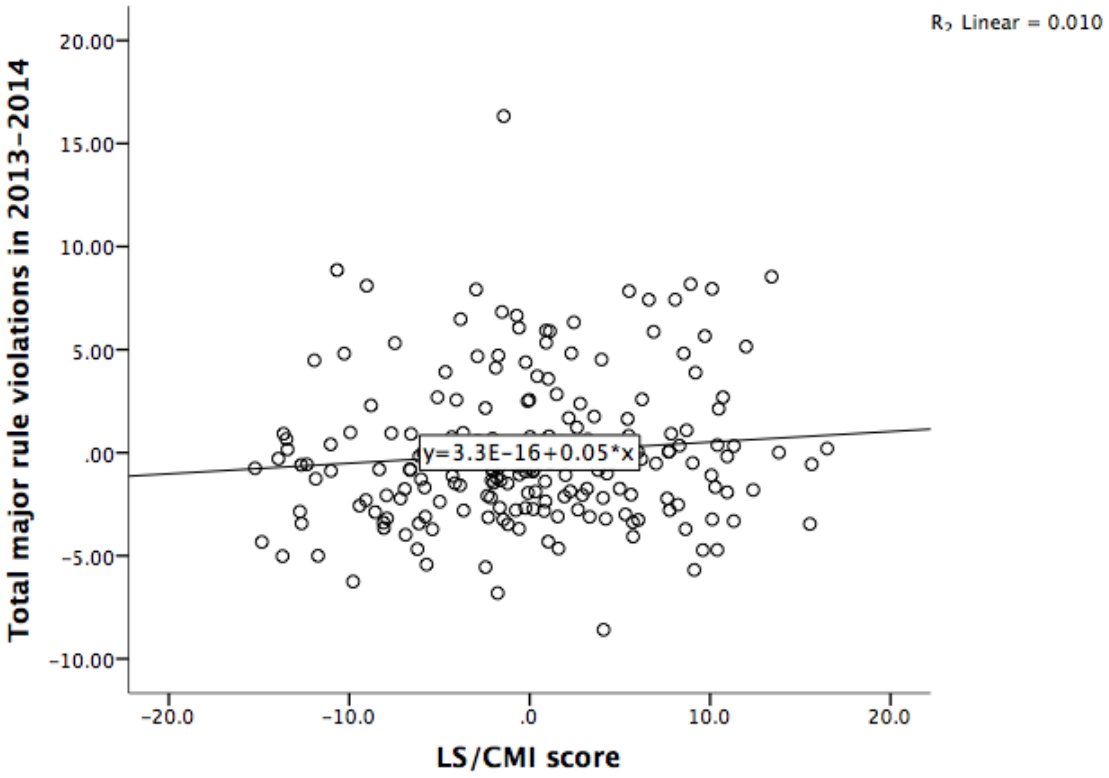

Figure 33. A partial plot with the variables Total Major Rule Violations in 2013-2014 and LS/CMI Score, using the data set with outliers removed.

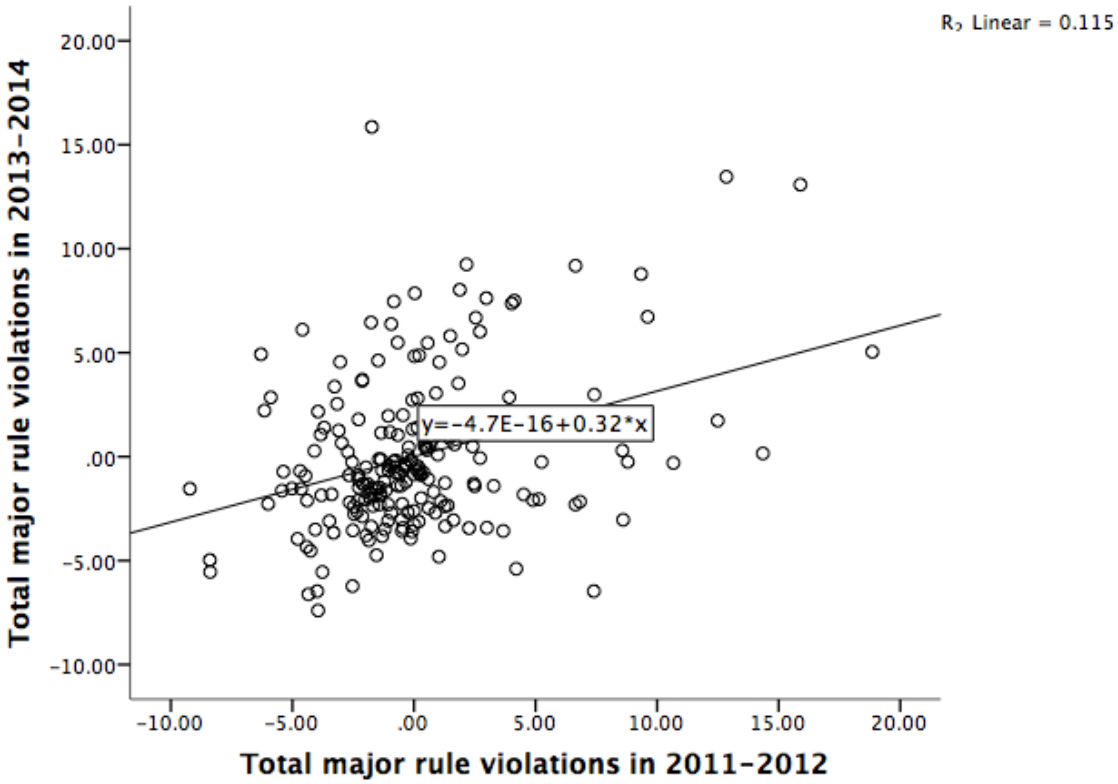

Figure 34. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Prior Major Rule Violations in 2011-2012, using the data set with outliers removed.

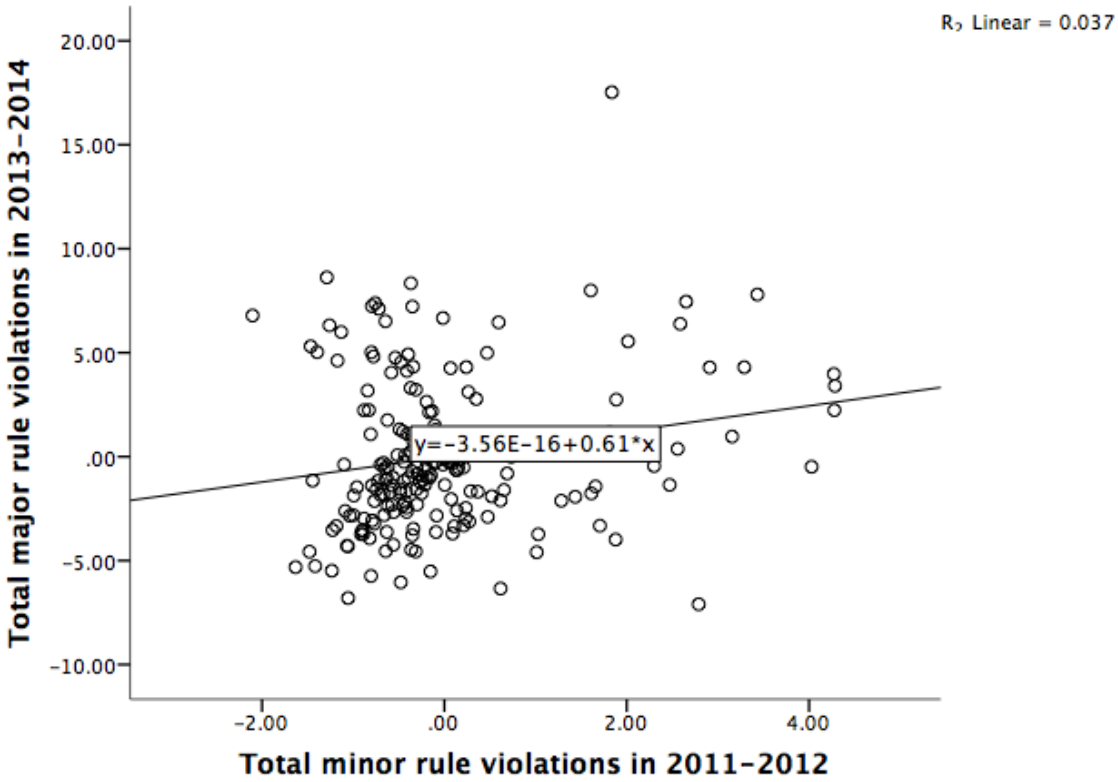

Figure 35. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Prior Minor Rule Violations in 2011-2012, using the data set with outliers removed.

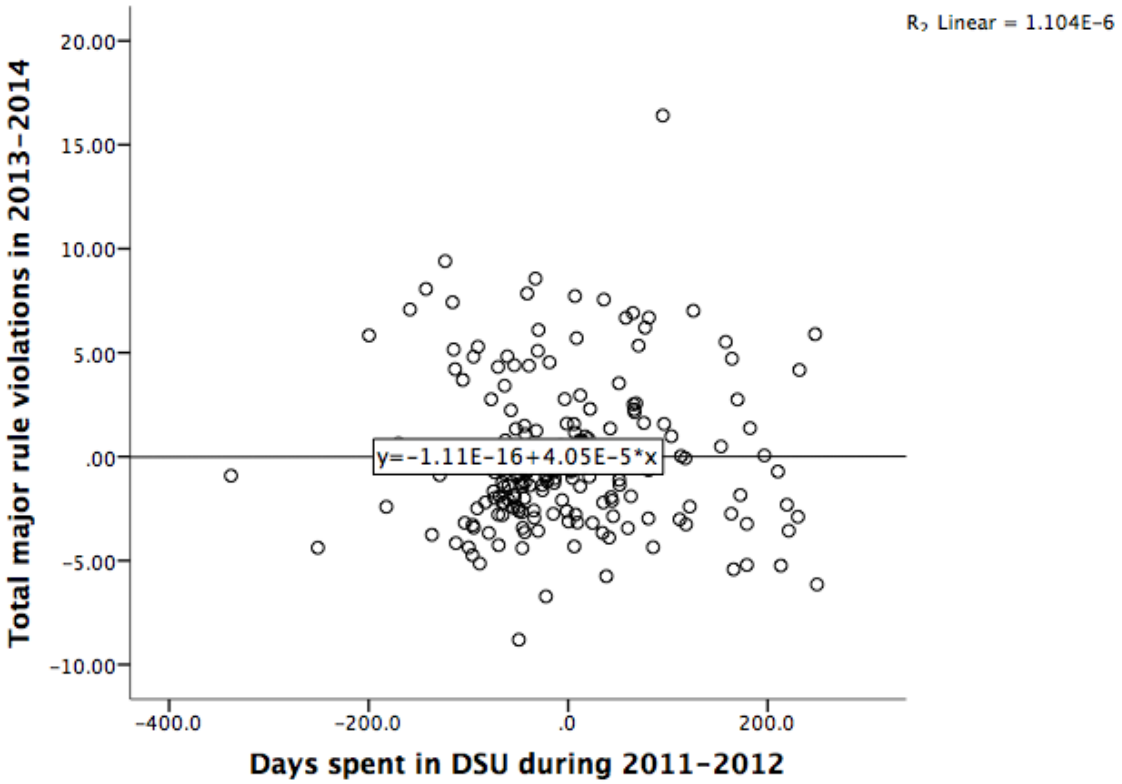

Figure 36. A partial plot with the variables Overall Total Major Rule Violations in 2013-2014 and Length of Time Spent in Disciplinary Segregation in 2011-2012, using the data set with outliers removed.

Appendix I: Scatterplot and Partial Plots of Data With Outcome Variable Total Major Rule Violations in 2013-2014, Full Data Set

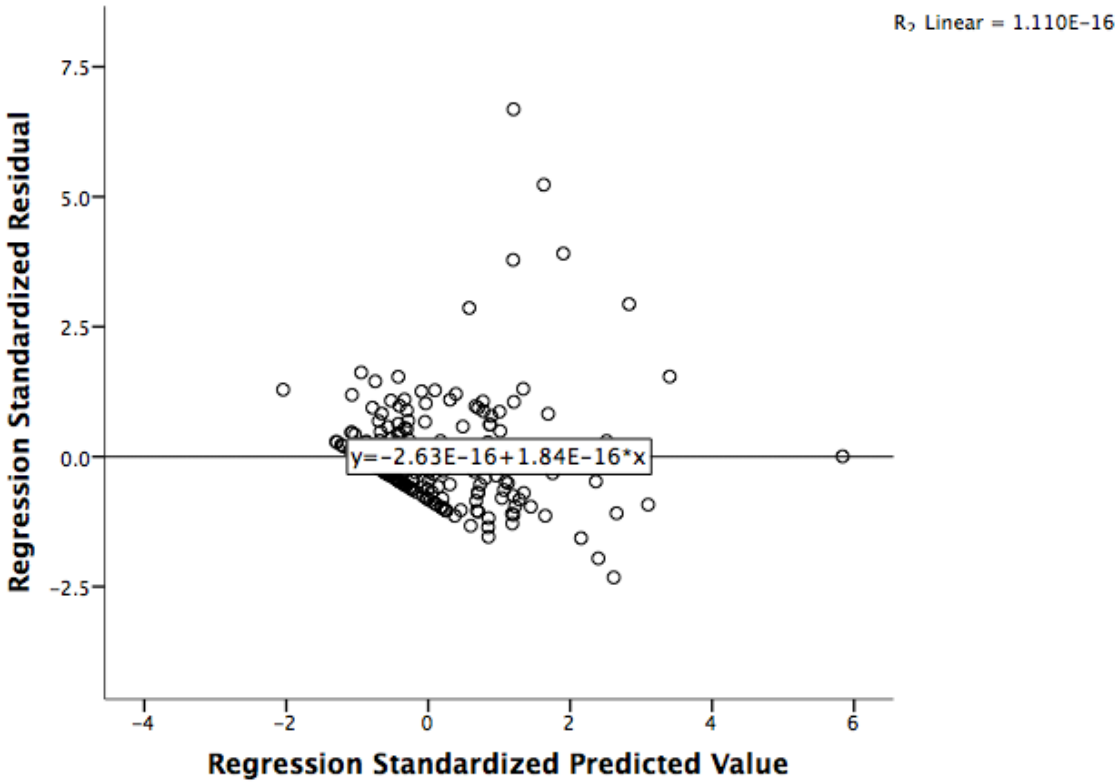

Figure 37. A scatterplot with the outcome variable of Total Major Rule Violations in 2013-2014, using the full data set.

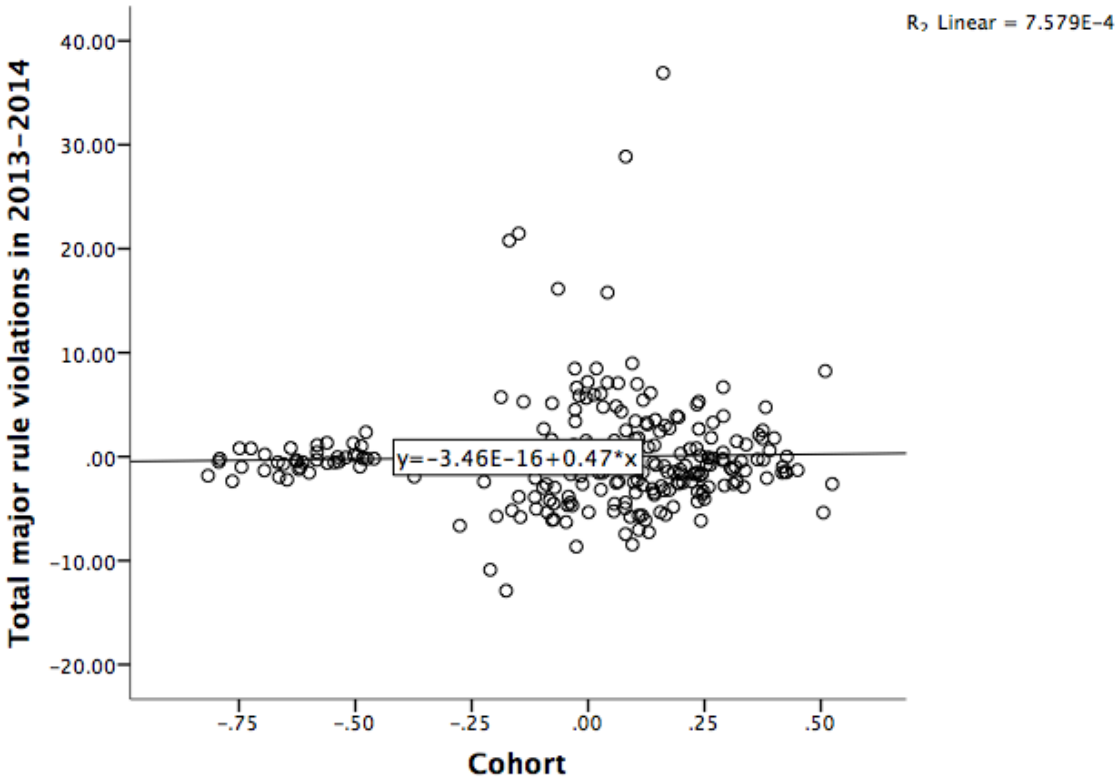

Figure 38. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Cohort, using the full data set.

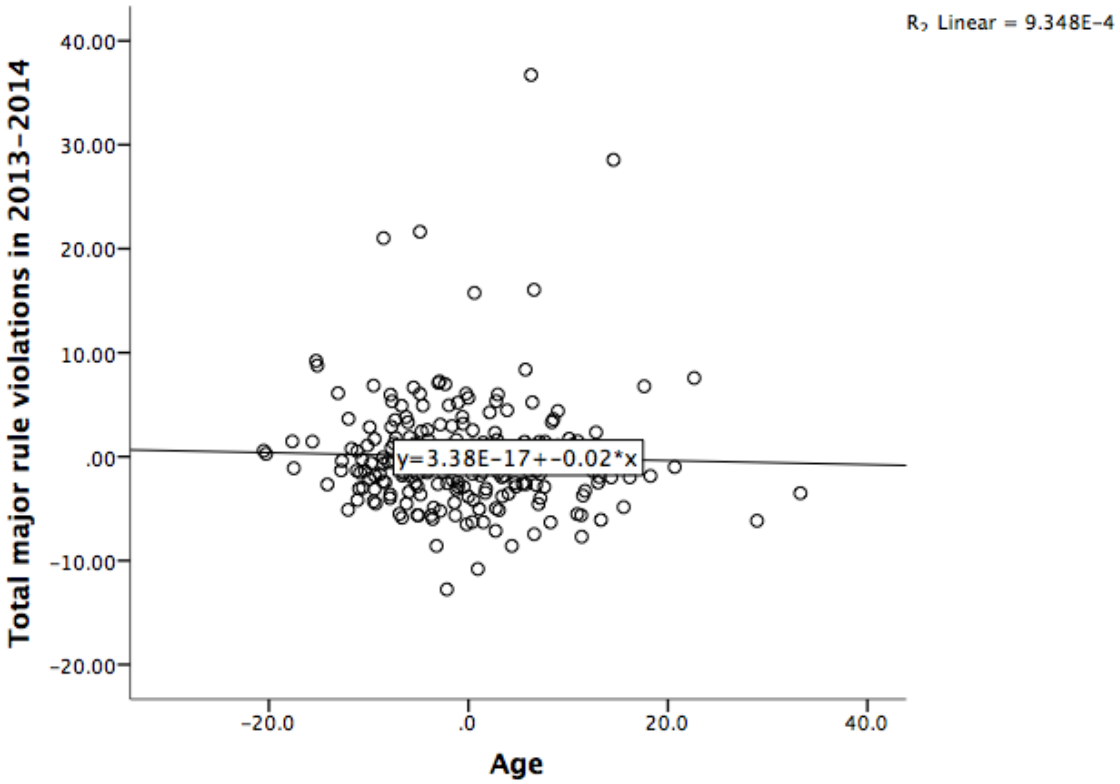

Figure 39. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Age, using the full data set.

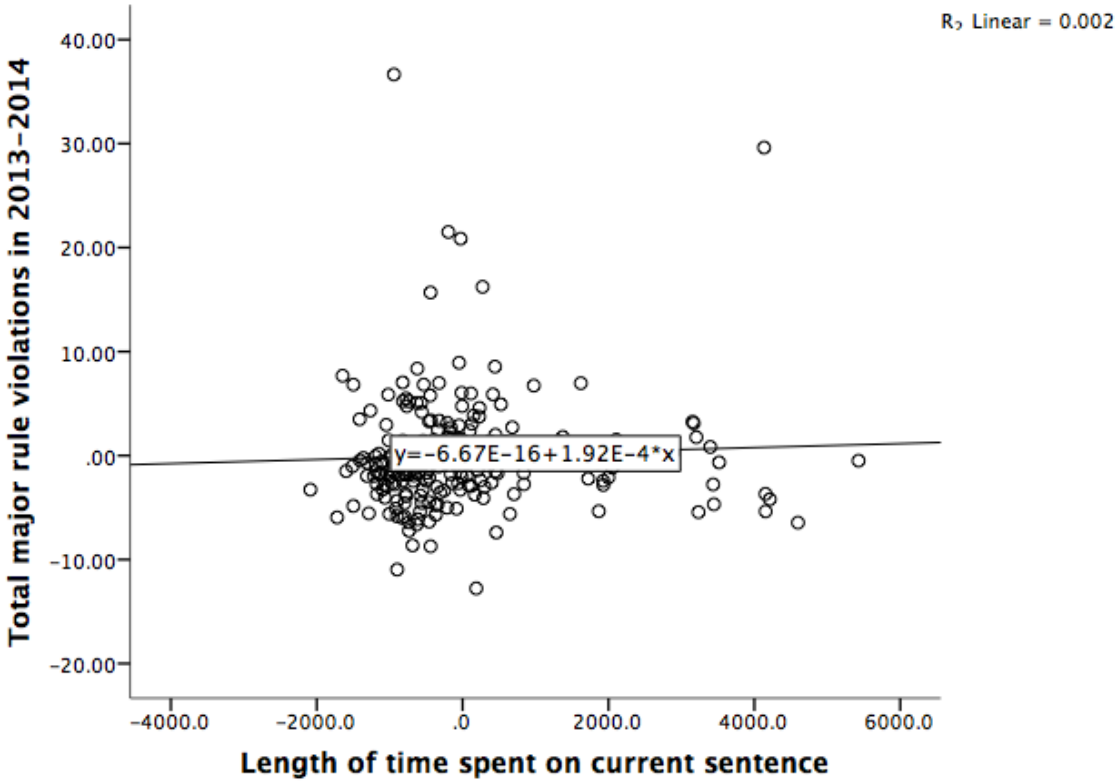

Figure 40. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Length of Time Spent on Current Sentence, using the full data set.

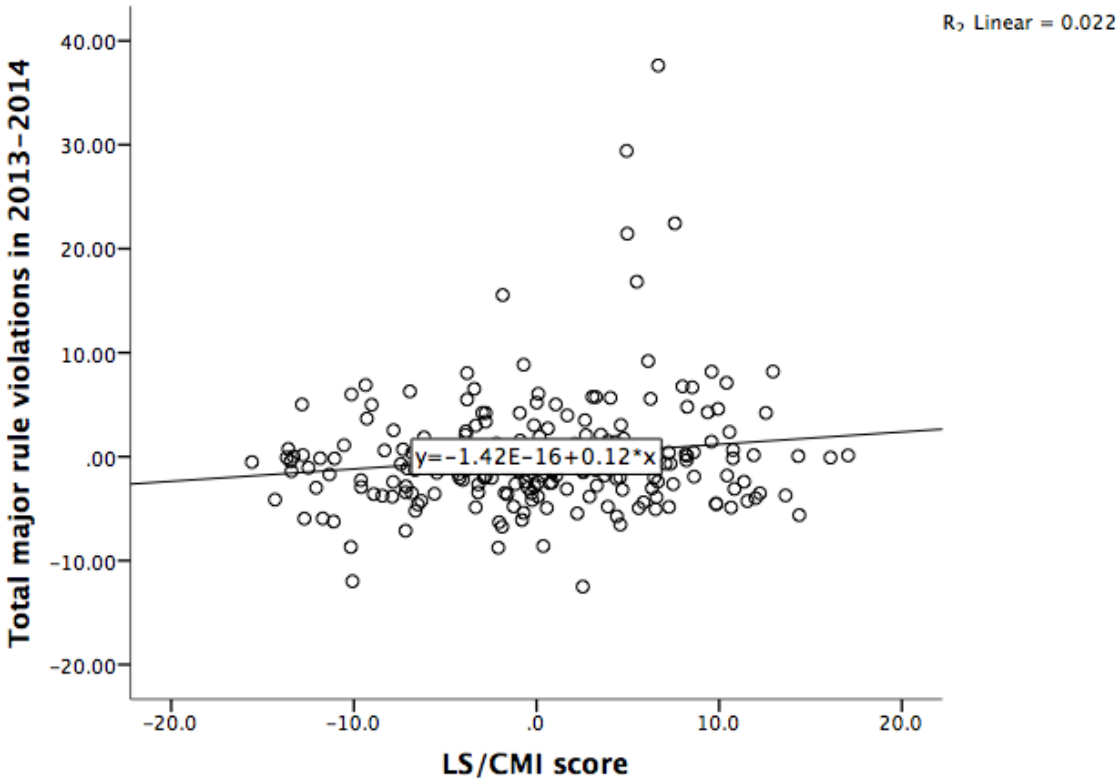

Figure 41. A partial plot with the variables Total Major Rule Violations in 2013-2014 and LS/CMI Score, using the full data set.

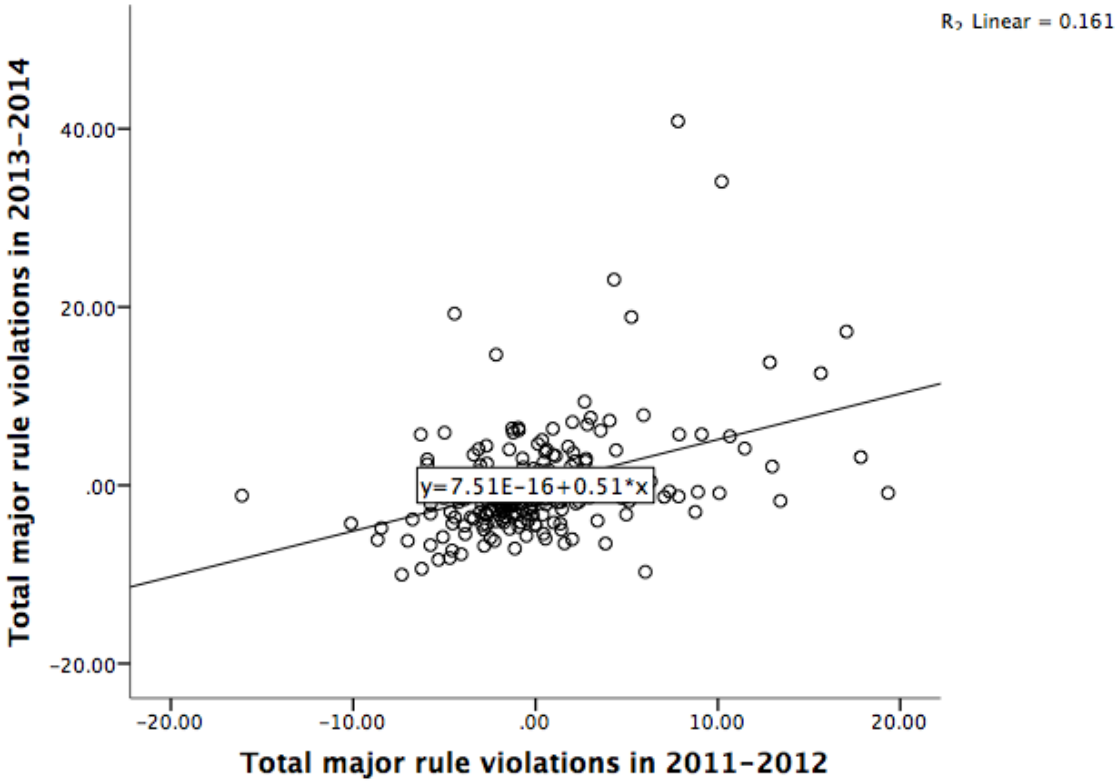

Figure 42. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Prior Major Rule Violations in 2011-2012, using the full data set.

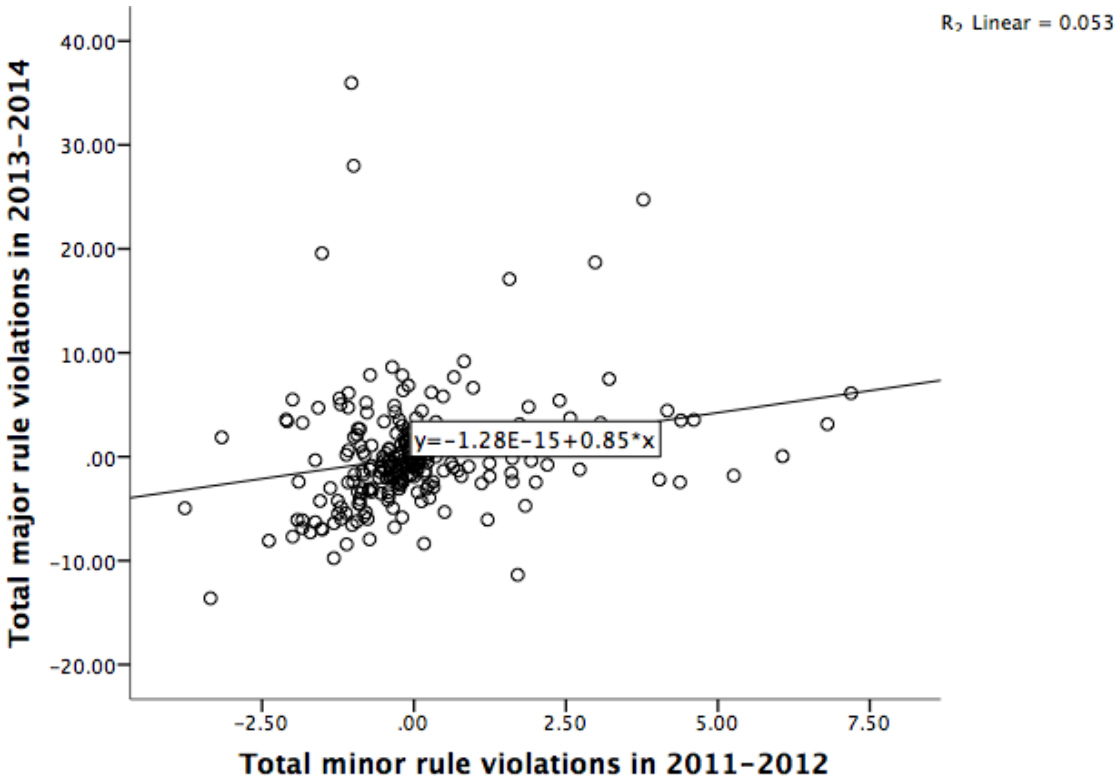

Figure 43. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Prior Minor Rule Violations in 2011-2012, using the full data set.

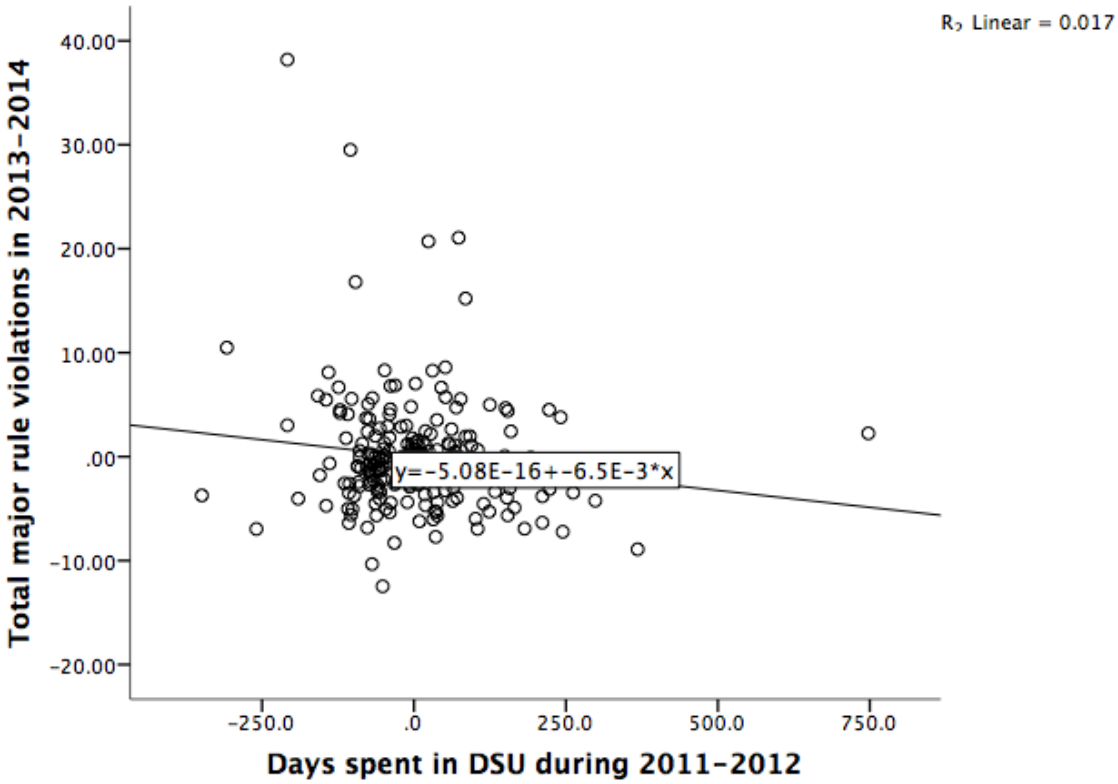

Figure 44. A partial plot with the variables Total Major Rule Violations in 2013-2014 and Length of Time Spent in Disciplinary Segregation in 2011-2012, using the full data set.

Appendix J: Scatterplot and Partial Plots of Data With Outcome Variable Total Minor Rule Violations in 2013-2014, Outliers Removed

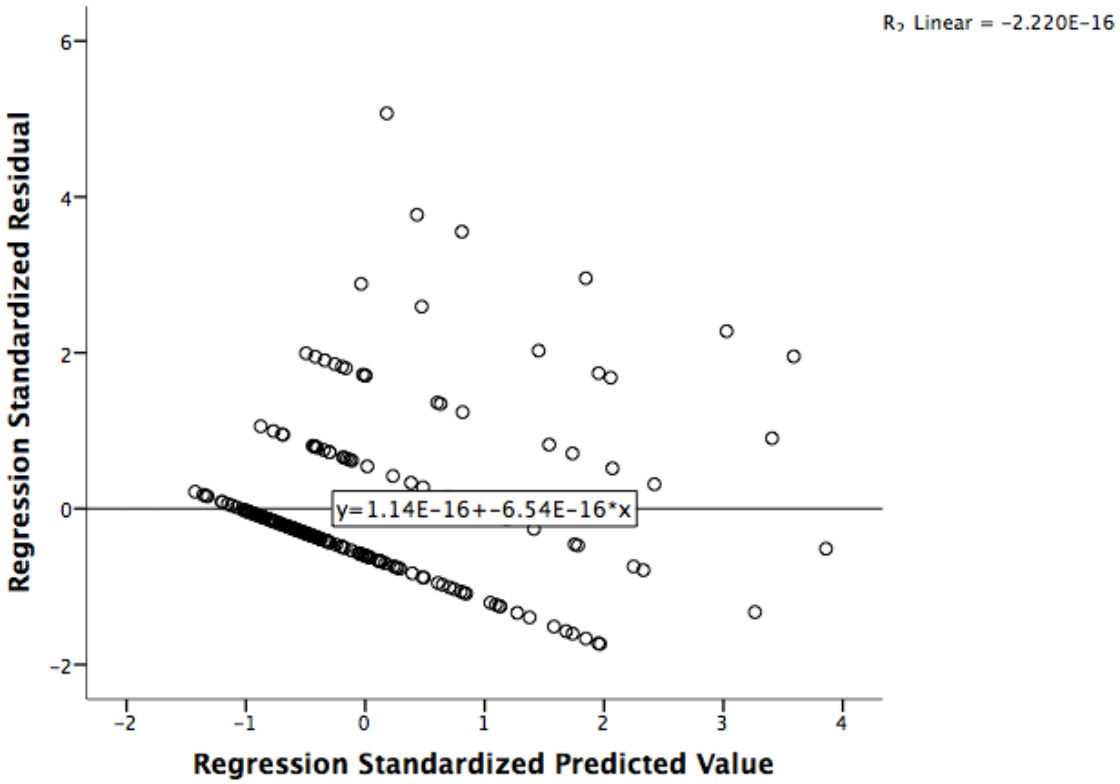

Figure 45. A scatterplot with the outcome variable of Total Minor Rule Violations in 2013-2014, using the data set with outliers removed.

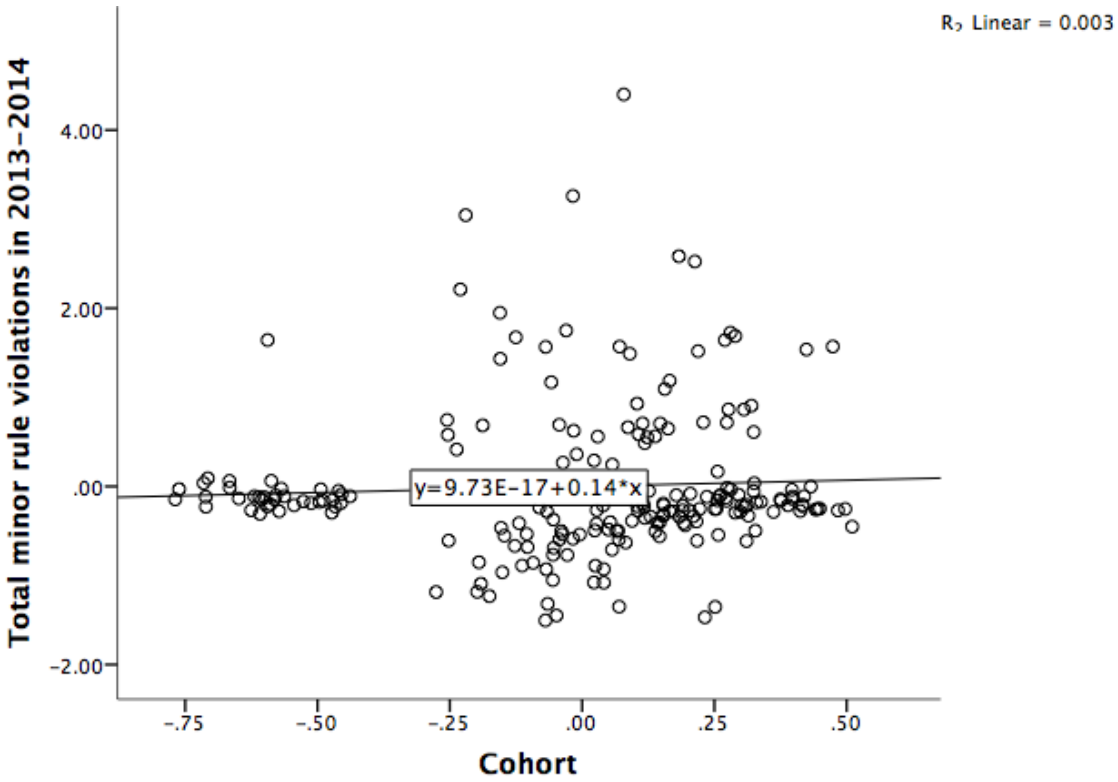

Figure 46. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Cohort, using the data set with outliers removed.

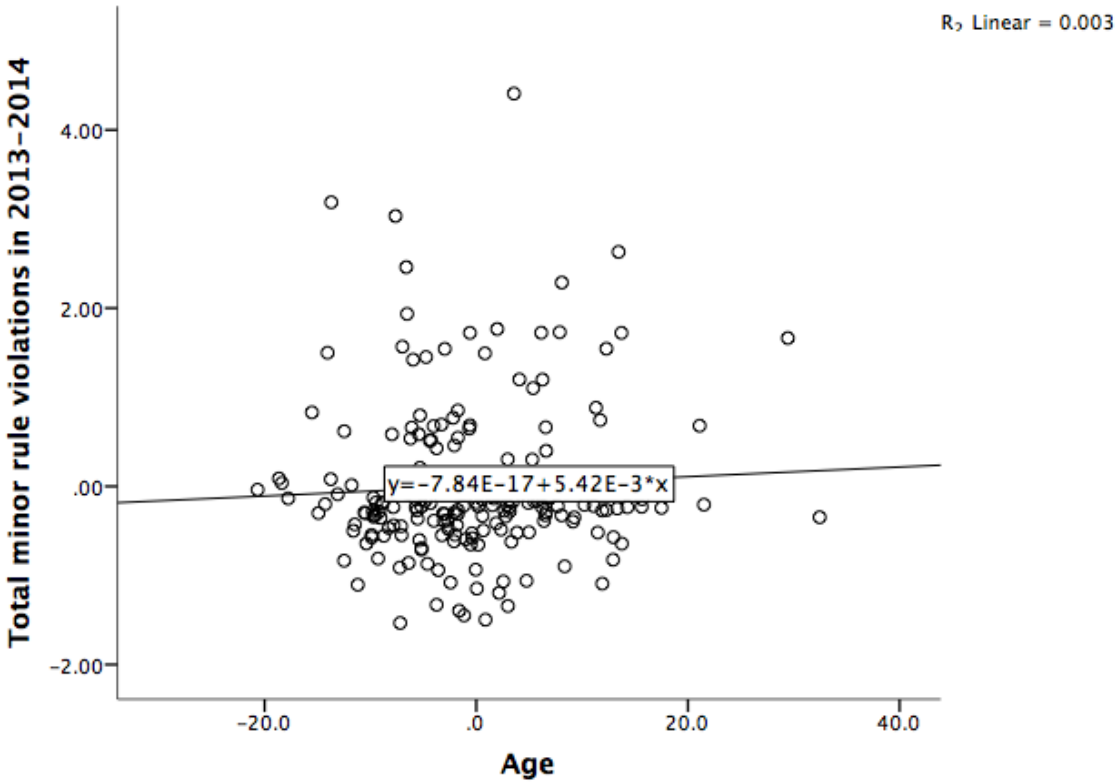

Figure 47. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Age, using the data set with outliers removed.

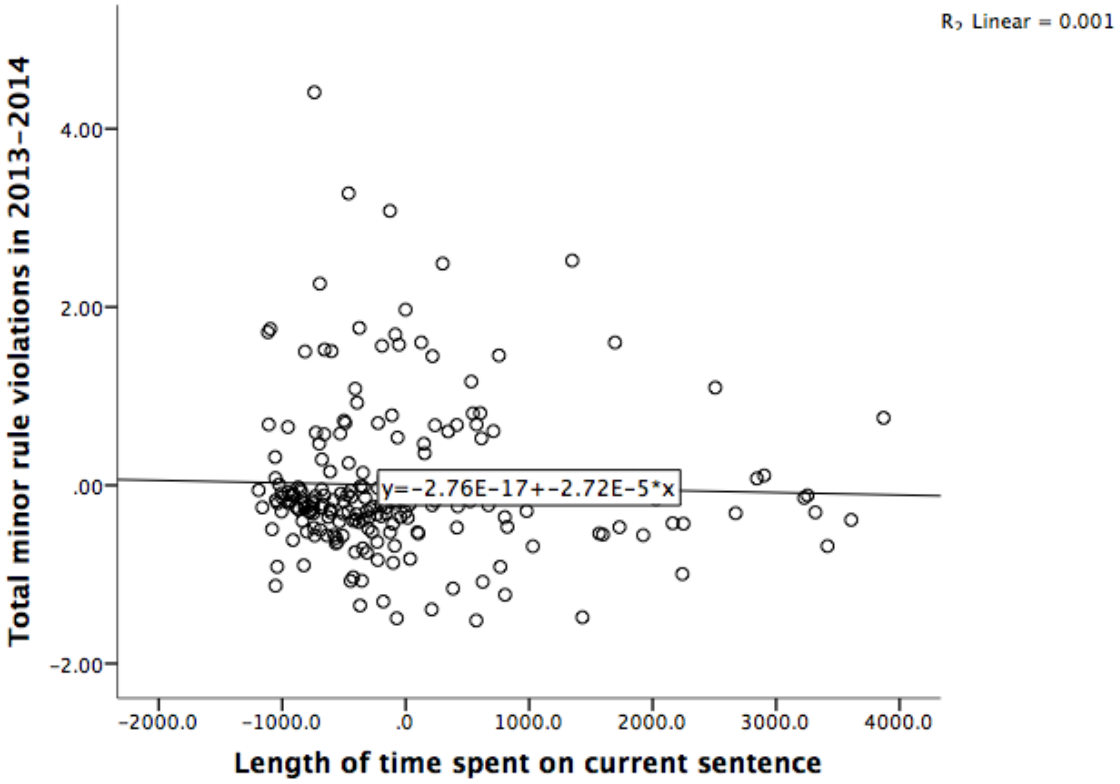

Figure 48. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Length of Time Spent on Current Sentence, using the data set with outliers removed.

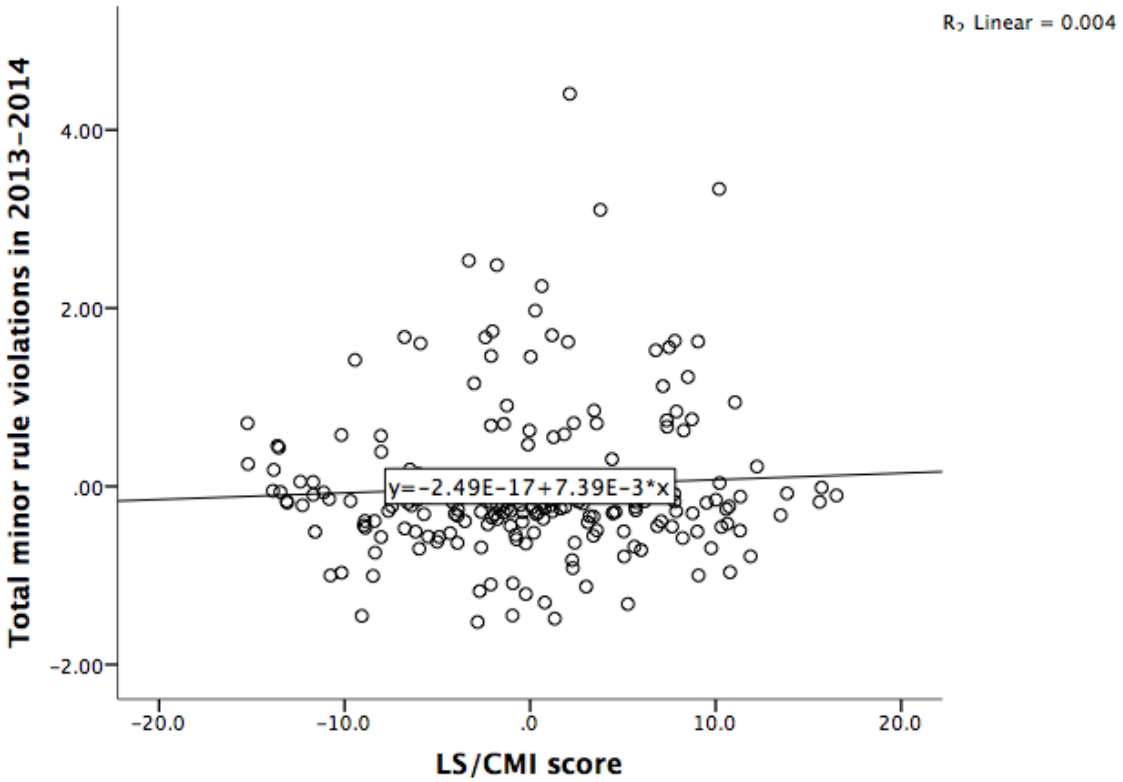

Figure 49. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and LS/CMI Score, using the data set with outliers removed.

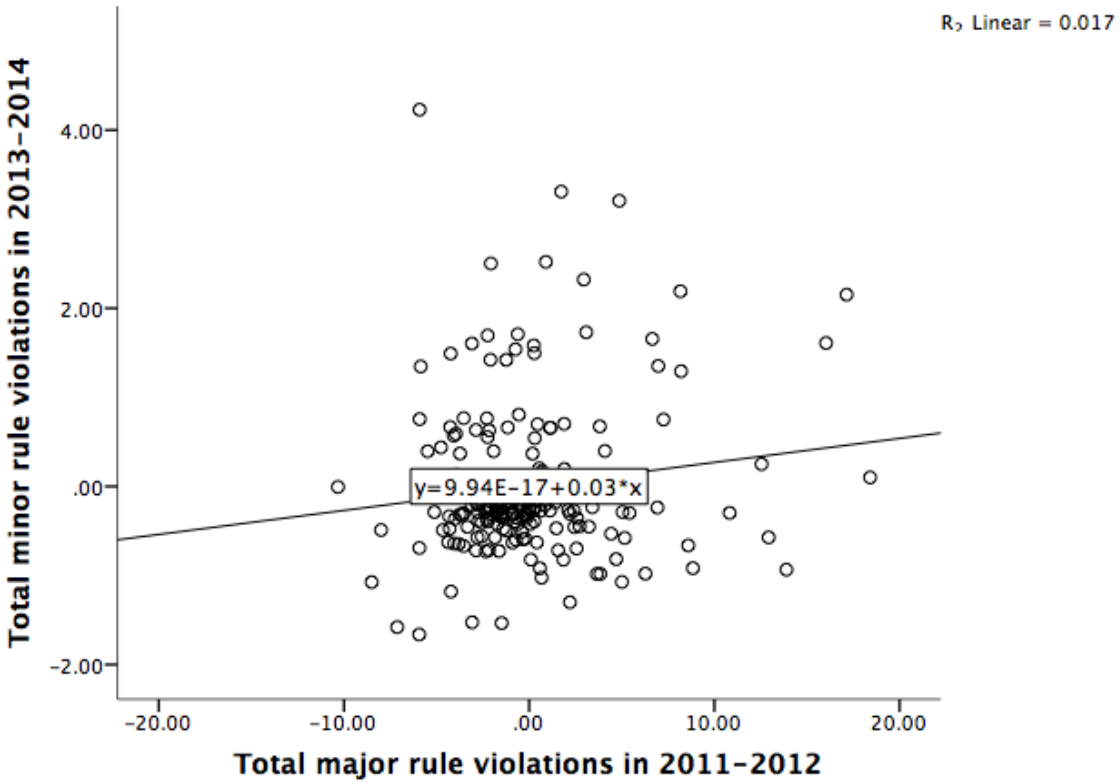

Figure 50. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Prior Major Rule Violations in 2011-2012, using the data set with outliers removed.

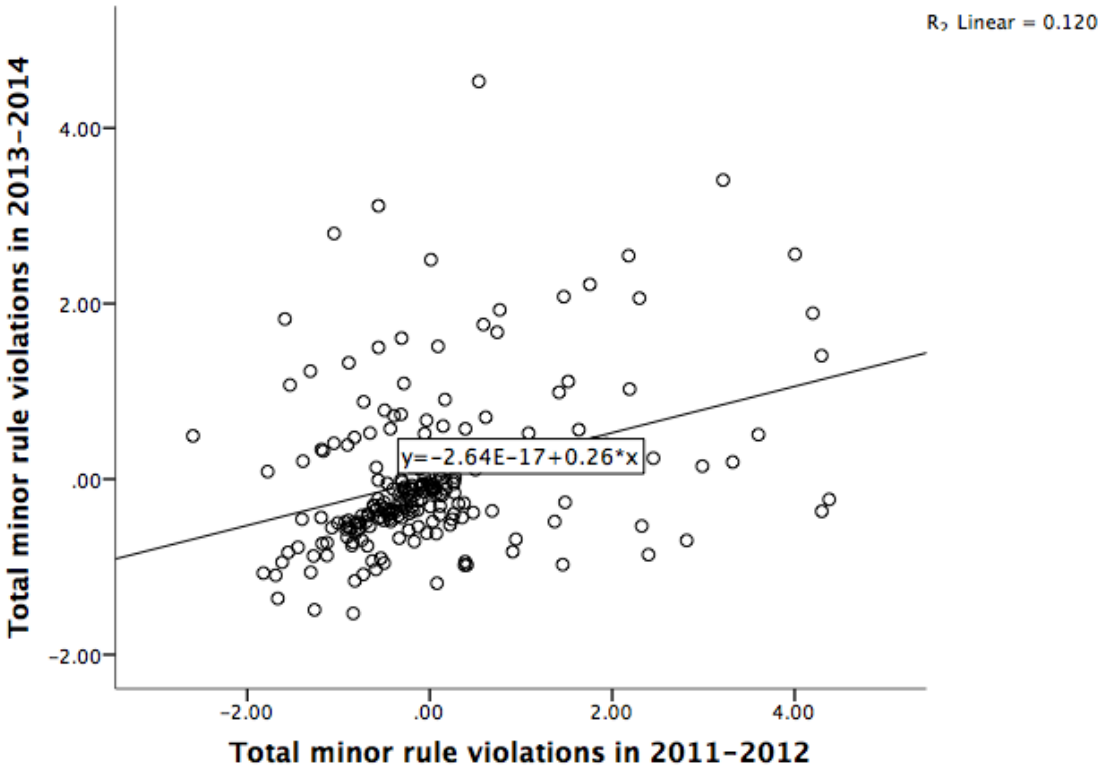

Figure 51. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Prior Minor Rule Violations in 2011-2012, using the data set with outliers removed.

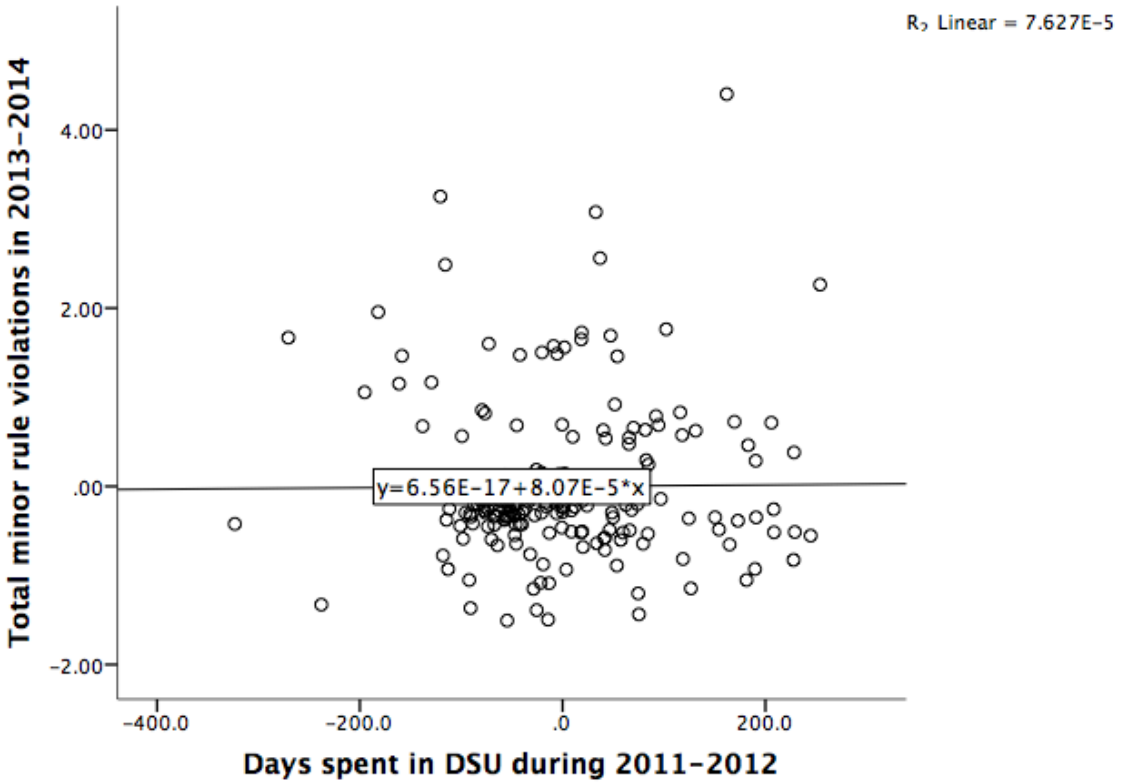

Figure 52. A partial plot with the variables Overall Total Minor Rule Violations in 2013-2014 and Length of Time Spent in Disciplinary Segregation in 2011-2012, using the data set with outliers removed.

Appendix K: Scatterplot and Partial Plots of Data With Outcome Variable Total Minor Rule Violations in 2013-2014, Full Data Set

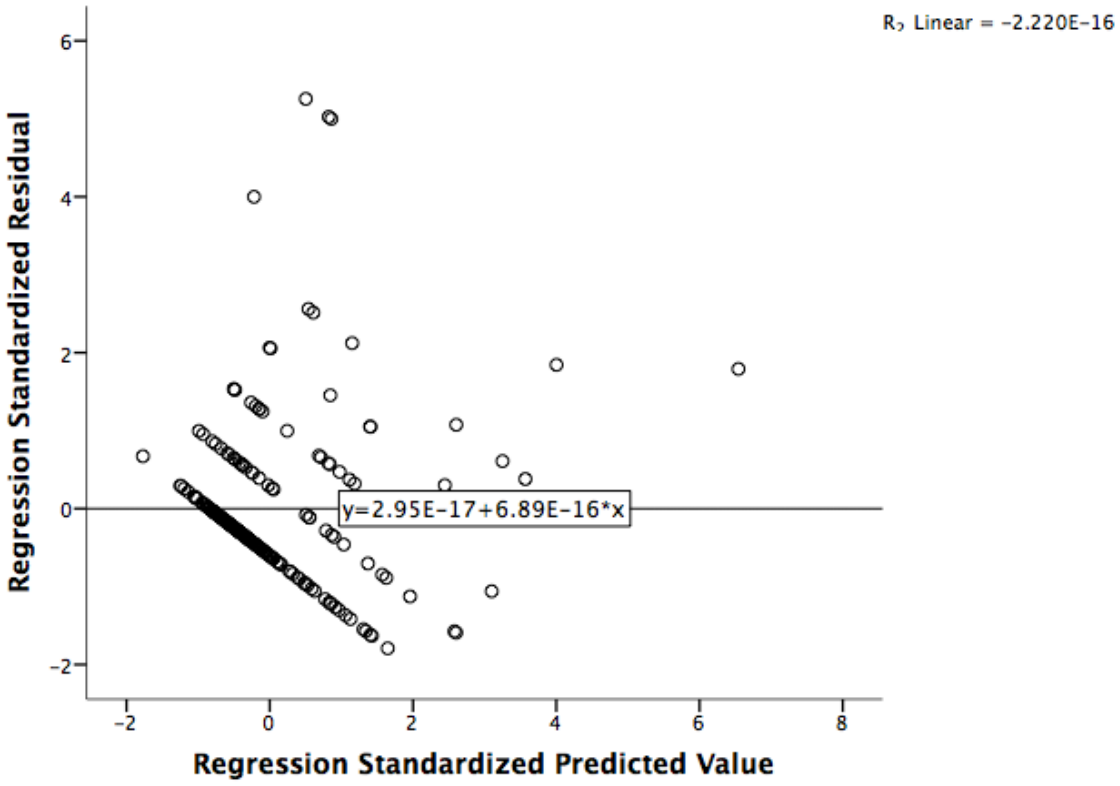

Figure 53. A scatterplot with the outcome variable of Total Minor Rule Violations in 2013-2014, using the full data set.

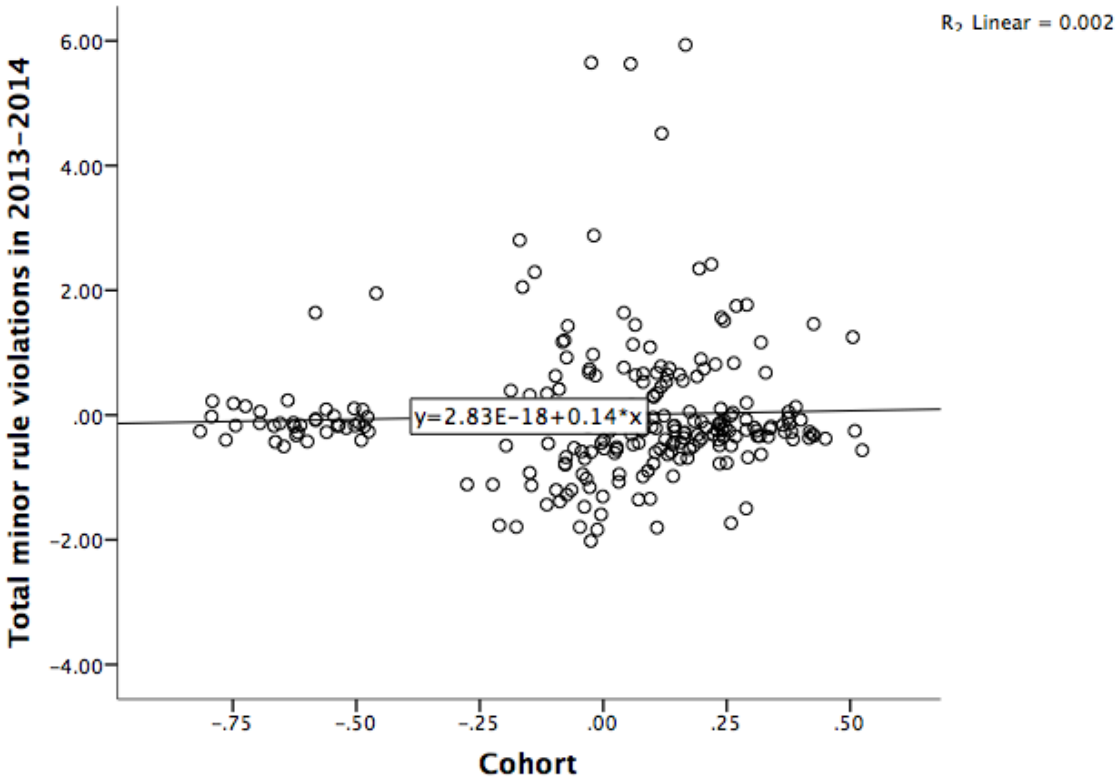

Figure 54. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Cohort, using the full data set.

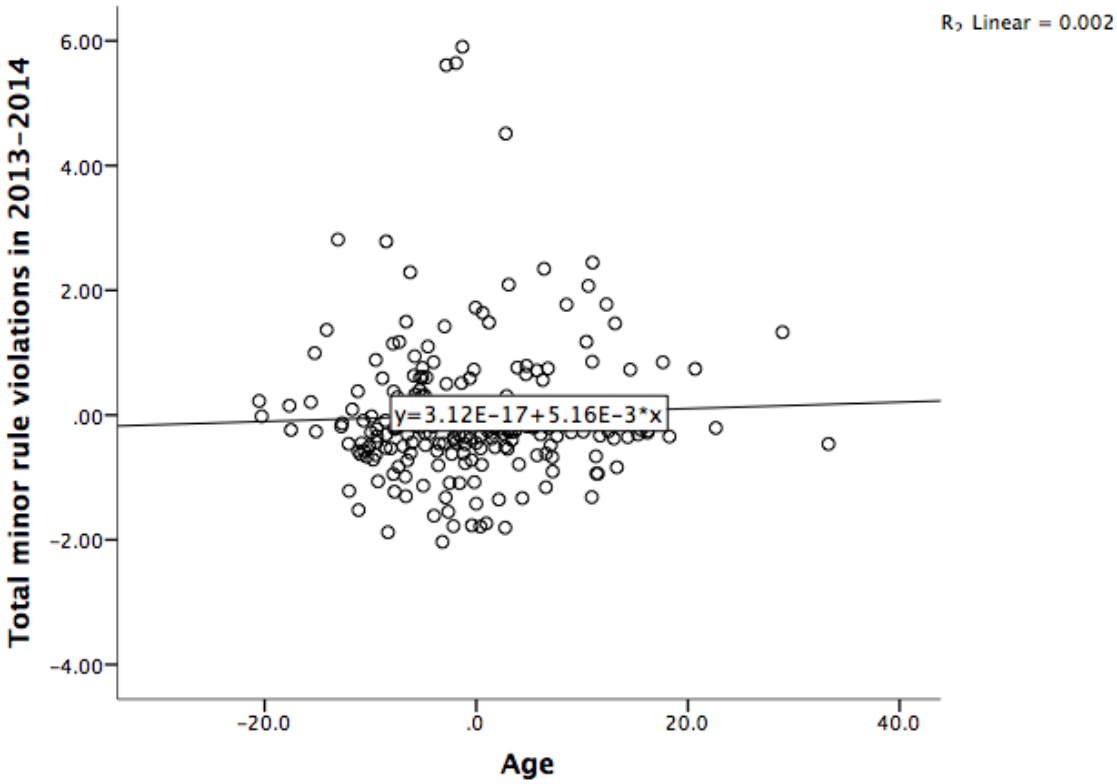

Figure 55. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Age, using the full data set.

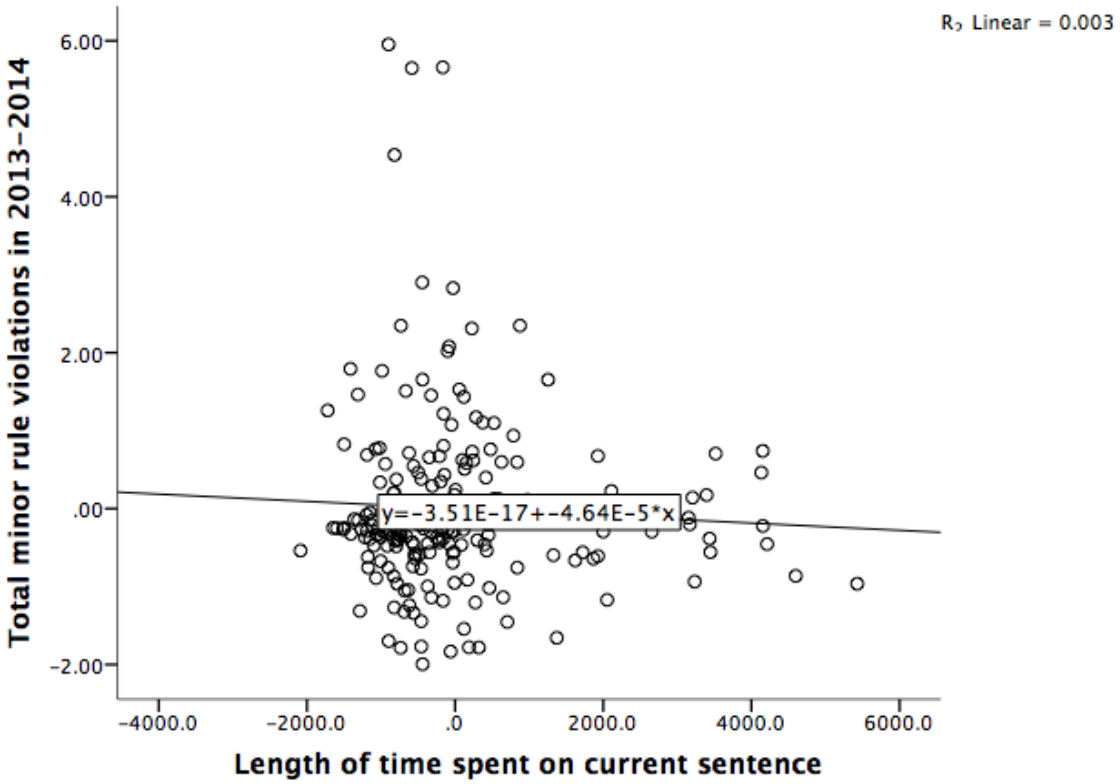

Figure 56. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Length of Time Spent on Current Sentence, using the full data set.

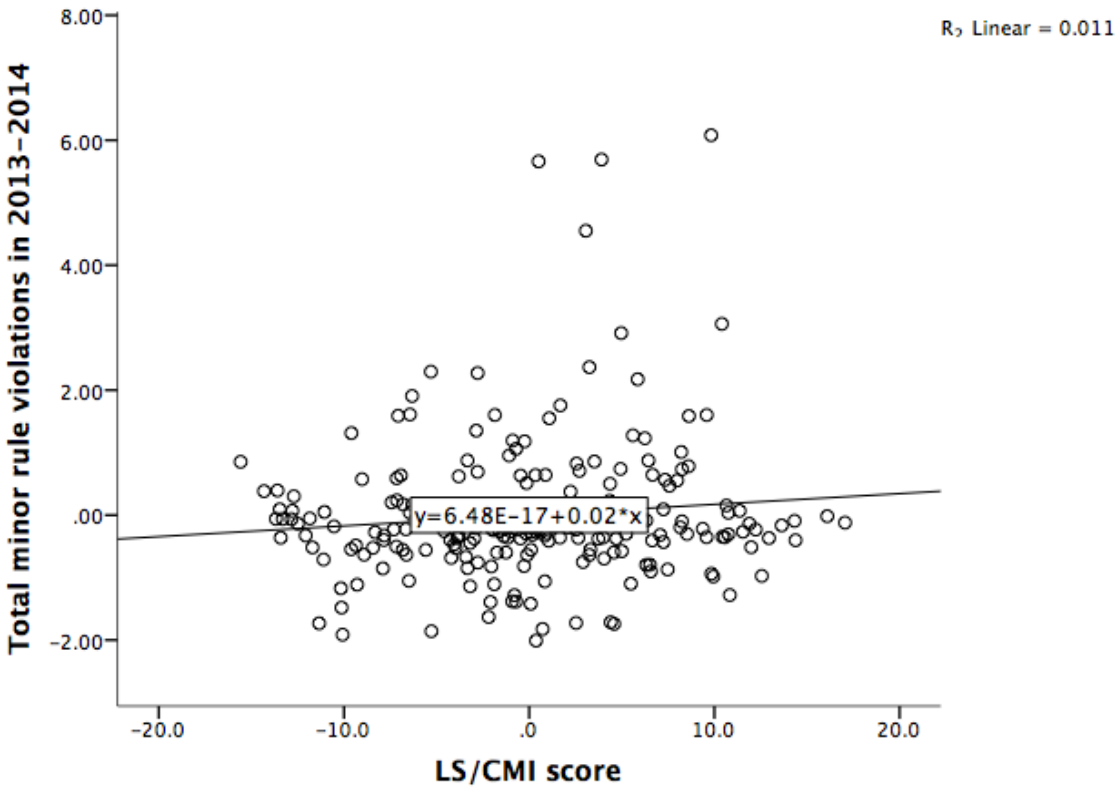

Figure 57. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and LS/CMI Score, using the full data set.

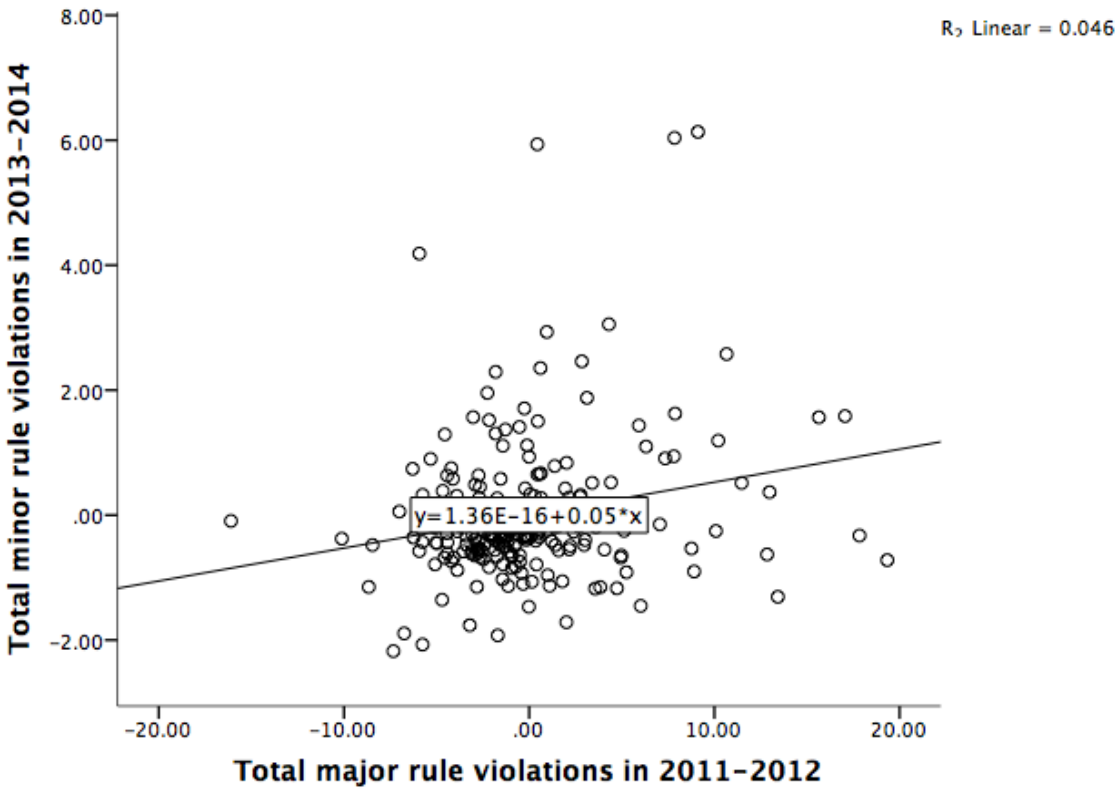

Figure 58. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Prior Major Rule Violations in 2011-2012, using the full data set.

Figure 59. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Prior Minor Rule Violations in 2011-2012, using the full data set.

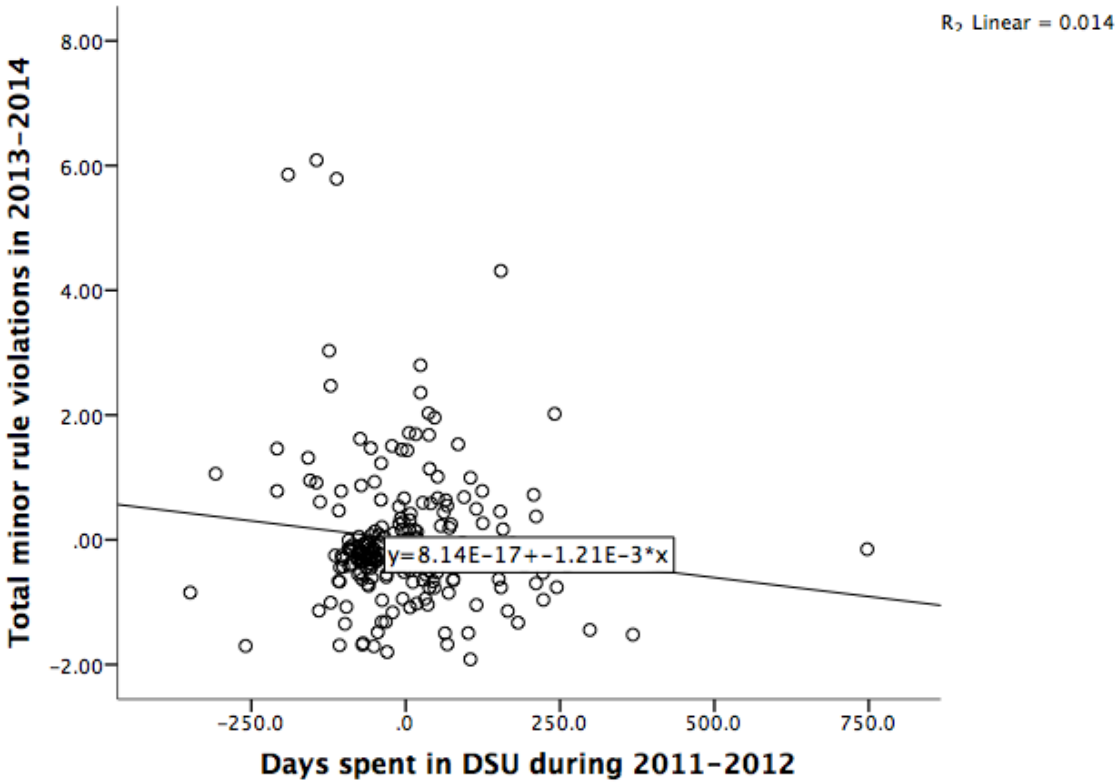

Figure 60. A partial plot with the variables Total Minor Rule Violations in 2013-2014 and Length of Time Spent in Disciplinary Segregation in 2011-2012, using the full data set.