The Effects of Prisons on Inmate Misconduct and Later Outcomes

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Abstract

Inmates in the US are assigned to different government-run prisons to serve their sentences and can face highly heterogeneous environments. I study how being assigned to prisons with different levels of inmate misconduct affects their outcomes. Using data from the Pennsylvania Department of Corrections, I estimate the effects of prisons on inmate misconduct while incarcerated controlling for a rich set of sentencing and assessment variables used to assign inmates to prisons. I test for bias in my estimates in two ways. First, I show balance across inmate demographics. Second, I leverage inmate transfers between prisons in a "movers" design to demonstrate that misconduct effects accurately reflect causal prison treatment effects. Being assigned to a prison in the highest vs. lowest decile of misconduct effects approximately doubles the inmate's misconduct, increases additional months in prison by 9%, and increases prison reentry from serious crime by 11%. Overcrowding and the criminality of peers are predictive of misconduct effects. A policy that assigns 20% of new inmates to the prisons that most reduce misconduct can decrease these inmates' misconduct by up to 40%, time in prison by 4%, and reentry from serious crime by 5%.

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1 Introduction

The United States imprisons more individuals than any other country in the world, with nearly 2 million people incarcerated at any given time (Carson, 2022; Zeng, 2022). Inmates often spend a long time behind bars, serving over two years on average, with a high likelihood of returning to prison after their release (Gaes and Laskorunsky, 2023). The majority of inmates serve their sentence in one of approximately 1,600 state-run facilities across the country (United States Bureau Of Justice Statistics, 2022). These facilities have highly heterogeneous characteristics, and little is known about how differences between state prisons in the US affect inmates.

The theory that different prison environments can shape inmate behavior dates back to Clemmer (1940) and Sykes (1958), who proposed that inmates responded to the "pains" of their imprisonment by changing their behavior. The criminology literature reports that prison characteristics such as security, overcrowding, inmate composition, management practices, and available programs are associated with significant changes in inmate behavior while incarcerated, as measured by rates of inmate misconduct (French and Gendreau, 2006; Steiner et al., 2014; Taxman and Blasko, 2016; Steiner and Wooldredge, 2019; Glazener and Nakamura, 2020; Wooldredge, 2020). However, this evidence is often mixed. The key issue is selection: Differences in behavior across facilities may reflect nonrandom sorting of inmates with different characteristics.

This paper leverages rich administrative data and quasi-experimental designs to estimate how prisons affect inmate misconduct and long-run outcomes. Misconduct is one of the few inmate outcomes that can be observed while incarcerated. It comprises a wide range of behaviors, from assault or arson to having contraband or disobeying orders. It can directly threaten the safety of inmates and staff, result in a range of punishments, and be indicative of future behavior. While the impact of misconduct has received little attention in economics,[1] studies in criminology have linked individual misconduct to recidivism by matching on inmate observables (Trulson et al., 2011; Cochran et al., 2014; Cochran and Mears, 2017). Misconduct *records* can also extend prison time through parole decisions or good-behavior policies, with any misconduct potentially resulting in "excess" prison time (relative to an inmate's minimum release date).[2]

[1]Exceptions include recent studies that have also examined inmate misconduct as an outcome (e.g., Arbour et al., 2023; Alsan et al., 2024).

[2]Misconduct is classified into types based on severity, major and minor; for the remainder of this section, misconduct only refers to minor misconduct. The reason is that my main test for bias uses a design that I can only apply to minor misconduct. See Sections 2.1.2 and 2.1.3 for more details; futrher, as discussed in Sections 3.2 and 4.5, my main results are similar if I use major misconduct.

I estimate the effects of prisons on misconduct by comparing observably similar inmates who enter prison around the same time. I use data from all inmate admissions to 26 State Correctional Institutions (SCIs) administered by the Pennsylvania Department of Corrections (PADOC) from 2010–2020. I estimate the effect of an inmate's initially assigned prison on his misconduct during his incarceration by controlling for key inmate sentencing and assessment data considered by PADOC for inmate assignment. I then propose two ways to test for potential unobserved characteristics that may bias my estimates. First, I show inmate demographics are balanced relative to prison misconduct effects. Second, I demonstrate that prison misconduct effects accurately predict changes in misconduct in a separate sample with inmate transfers between prisons. These tests suggest that my estimates reflect causal prison treatment effects. I then show that being assigned to prisons with high misconduct effects increases excess time in prison and prison reentry from serious crime.

Initial prison assignment is balanced across available demographics relative to prison misconduct effects. Inmates who enter prison to serve time for a new sentence undergo a classification process to determine the prison where they will serve their sentence. Initial assignment is based on a combination of their individual characteristics and idiosyncratic factors, such as bed space availability. I compare inmates who enter prison in the same month and have similar observables relevant to their classification (including their offense, sentence, custody level, committing county, individual assessments, and criminal history). Inmates assigned to facilities with different misconduct effects have a similar age, race, and marital status. A regression of predicted outcomes (misconduct, excess time, and prison reentry predicted solely from demographics) on prison misconduct effects yields precise zeros. Assuming the remaining variation is uncorrelated with prison misconduct effects, I can estimate the causal impact of being assigned to prisons with higher or lower misconduct.

I further test for bias in the estimated misconduct effects by using a movers design. The key identifying assumption is that inmates who transfer to higher-misconduct facilities would have had similar trends in misconduct to those who transfer to lower-misconduct facilities absent the move (i.e., any trends for movers cannot vary systematically with their origin and destination). I find that inmates who transfer to prisons with higher misconduct effects have nearly identical pre-trends to those transferring to prisons with lower misconduct effects. However, inmates who go to higher-misconduct prisons commit more subsequent misconduct than those who go to lower-misconduct prisons. Further, the change in their own misconduct is proportional to the difference between the misconduct effect of their destination and their origin. A regression of the mover misconduct effect on the prison misconduct effect yields a precise coefficient that is statistically indistinguishable from

1. Hence, I argue that I can use my estimated prison misconduct effects to recover the average effect on inmate outcomes of being assigned to prisons with higher or lower misconduct treatment effects.

My misconduct estimates suggest that there is substantial variation across prisons in their effect on inmate outcomes. An inmate assigned to the top decile of prison misconduct effects would on average double his misconduct rate relative to the bottom decile. I also find a significant effect on excess prison time: Being assigned to a top-decile prison increases excess time in prison by 0.7 months (approximately 3 weeks, or 9% of the average). The average monthly cost of each inmate in Pennsylvania implies top-decile prisons are $1.4M more costly for the state, and individual incarceration costs from Abrams and Rohlfs (2011) imply that top-decile prisons are $310,000 more costly for their inmates.

I also find evidence that assigning inmates to higher-misconduct facilities increases future crime. Being assigned to a prison in the top decile of misconduct effects increases the probability of reentering prison with a new sentence by 0.9 percentage points over 5 years (11% of the average). While recidivism is often defined as future arrests or charges, I only observe prison reentry. However, I am able to observe whether an inmate reentered prison due to a new sentence, associated with a serious crime, or through parole, which can be associated with any type or crime or a technical parole violation.[3] I find evidence that, in the long-run, inmates assigned to higher-misconduct prisons substitute parole violations for new criminal activity. While misconduct effects do not significantly impact overall reentry, they significantly decrease reentry from parole violations (driven by a decrease in technical violations) and significantly increase reentry from new crime (driven by an increase in new sentences).

What is driving these estimated differences across prisons? The literature reports that facility characteristics such as overcrowding, security level, race composition, and age composition are all correlated with misconduct (Steiner et al., 2014; Steiner and Wooldredge, 2019; Glazener and Nakamura, 2020). There is also some experimental evidence that being assigned to a higher security level prison increases minor infractions (Tahamont, 2019) but not violent behavior (Gaes and Camp, 2009).[4] While I also find that overcrowding predicts prison misconduct effects, I do not find an effect

[3]Examples of technical parole violations include breaking curfew, moving without permission, failing to report as instructed, or unauthorized contact with a victim (Pennsylvania Parole Board, 2022). From 2013 onward, reentry to an SCI was only required for certain types of technical parole violations (e.g., if the violation involved a physical threat). An inmate can also enter through a non-technical parole violation and not have a criminal conviction. I do not observe the details associated with either type of reentry.

[4]Gaes and Camp leverage an experiment in California that randomly assigned inmates to be assigned into prisons by different algorithms. They find high security prisons increase recidivism (which is consistent with less precise quasi-experimental evidence; see Chen and Shapiro (2007); Drago et al. (2011)) but have no effect on violent behavior.

for either security level or demographic composition. Instead, I find the composition of offender types (rates of drug and sex offenders, average sentence length) are significant predictors of prison misconduct effects. This is consistent with quasi-experimental evidence that being exposed to peers of higher criminality increases an inmate's own criminality (Bayer et al., 2009; Stevenson, 2017; Harris et al., 2018).

Finally, I show that assigning a portion of inmates to prisons based on misconduct effects can reduce their misconduct, time in prison, and future serious crime. This assignment rule leverages the fact that PADOC prisons have been undercrowded since the COVID-19 pandemic. Assuming the total prison population remains constant, redirecting 20% of inmates who enter prison each month for a new sentence to the lowest-misconduct prisons will keep those prisons below capacity. Further assuming that prison misconduct effects remain constant, this assignment rule would reduce the misconduct of those inmates by 40%, excess time by 4.2%, and future serious crime by 5.4%.

This paper contributes to three main literatures by estimating prison misconduct effects and linking them to later inmate outcomes. First, I add to the criminology literature studying how prison characteristics impact misconduct (Steiner et al., 2014; Steiner and Wooldredge, 2019; Glazener and Nakamura, 2020; Wooldredge, 2020). In contrast to this literature, I directly estimate the effects of prisons on misconduct by focusing on the inmate assignment process and then explicitly test the validity of my estimates. I argue that inmate assignment is not confounded by any factors correlated with prison misconduct effects once I control for key inmate observables relevant to this process. To demonstrate I successfully account for selection, I propose a validation test that uses a quasi-experimental movers design. This test shows that there is little bias in my estimates of prison misconduct effects and that initial assignment is unlikely to be driven by any unobserved factors that predict misconduct. Finally, I show a significant effect on long-term inmate outcomes of being assigned to prisons with high vs. low misconduct effects.

Second, I add to the literature studying the effects of specific prison characteristics on inmate outcomes, which has paid little attention to the differences in prison conditions between state-run facilities in the US.[5] While Tobón (2022) and Mastrobuoni and Terlizzese (2022) find very large

[5] One exception is Gaes and Camp (2009), who study inmate assignment in California; their main limitation is that they study a small sample (561 inmates assigned over 6 months) and one prison classification (high vs. low security). Two recent papers, Arbour et al. (2023) and Alsan et al. (2024), also study both misconduct and recidivism; however, both focus on a different context. Arbour et al. (2023) studies the impact of Cognitive Behavioral Therapy in prisons in Quebec, Canada; Alsan et al. (2024) studies the IGNITE program in one county jail in the US (Flint, Michigan). In contrast, I conduct a comprehensive analysis comparing all prisons in one US State (Pennsylvania).

negative effects on recidivism of assigning inmates to newer facilities with better conditions,[6] it is unclear to what extent these results apply to the typical inmate assignment decision of state-run systems in the US. Drago et al. (2011) and Lotti (2020) also study prisons outside the US; Chen and Shapiro (2007) study federal prisons; Bayer et al. (2009) and Mukherjee (2021) compare all public prisons to other prison types.

Finally, I add to a literature using "value-added" methods to estimate the quality of institutions such as schools, doctors, teachers, nurses, hospitals, and health insurance plans (Kane and Staiger, 2008; Fletcher et al., 2014; Chetty et al., 2014a,b; Yakusheva et al., 2014; Angrist et al., 2016, 2017; Doyle et al., 2019; Abaluck et al., 2021; Angrist et al., 2024). Methodologically, I draw from studies that exploit variation in outcomes around individual moves. This design was originally used to study wage differences across companies and types of workers (Abowd et al., 1999; Card et al., 2013) and has since been applied to other settings, such as the variation in health care utilization and outcomes (Finkelstein et al., 2016, 2021; Badinski et al., 2023), intergenerational mobility (Chetty and Hendren, 2018a,b), and voting behavior (Cantoni and Pons, 2022). I show that a movers design can be used to test for bias in observational value-added estimates.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the data; Section 3 presents the empirical framework, balance on observables, and the movers design to test for bias and validate prison misconduct effect estimates; Section 4 presents the main results; Section 5 discusses potential mechanisms; Section 6 concludes the paper.

2 Setting and Data

2.1 Pennsylvania Department of Corrections

I use administrative data on all adult male inmates present in one of Pennsylvania's 26 SCIs from 2010 through 2020. PADOC currently operates 23 SCIs and one motivational bootcamp. An additional five SCIs that have since closed were in operation for at least some portion of this time period, and two of PADOC's SCIs only house female inmates.

For each inmate's prison stay in the sample, I observe sentencing data and demographics, any transfers between prisons, and misconduct during their incarceration; I also observe all past and future admissions, including the reason for their admission. The main outcomes of interest are

[6] Tobón (2022) studies prisons in Colombia that are less crowded and have more security and higher participation in rehabilitative programs; Mastrobuoni and Terlizzese (2022) study a prison in Italy that is less crowded and has open instead of closed cells.

inmate misconduct, excess time in prison, and prison reentry. "Excess" time in prison refers to time served in excess of an inmate's parole eligibility date. Prison reentry is my main proxy for recidivism (reoffending after prison exit), with a particular focus on comparing reentry because of a new crime to reentry because of a parole violation.

2.1.1 Prison Entry and Initial Assignment

Inmates primarily enter prison to serve time for a new sentence or for a parole violation.[7] For a new sentence, inmates are assigned a minimum and a maximum release date, with the minimum serving as their parole eligibility date.[8] Most sentences range from 1 to 7 years (bottom and top decile of inmate sentence lengths). Individuals admitted through a parole violation can spend anywhere from 3 to 18 months back in prison,[9] depending on the nature of their violation. Technical parole violators serve the least amount of time, while individuals convicted of a new crime while on parole can be made to serve the remaining balance on their previous sentence (i.e. until their maximum date) or be given a new sentence, depending on the severity of their crime.

Inmates that enter prison for a new sentence go through a diagnostic and classification process to determine their initial prison assignment. During this time, inmates receive a treatment plan that takes into account any programs they need to complete, their physical and mental health, the nature of their offense, the time they will be in prison, and their current and past behavior.[10] For the majority of my sample period, inmate assignment was then done sequentially. Every week, a team of two individuals would receive the files for a group of inmates that needed initial placement (on average each group had over 100 inmates). Each would go through a set of files and one by one determine where each inmate could be placed.[11] An inmate's initial assignment accounts for his treatment plan and needs, custody level (security),[12] home region, available bed space at each

[7] Approximately 3% of inmates enter an SCI for other reasons, e.g., being temporarily held, an out of state transfer, or a returned escapee.

[8] While inmates do not have a right to parole in Pennsylvania, PADOC has a longstanding practice of allowing individuals to interview for parole to be granted around their minimum release date.

[9] For technical parole violations, the only exception is a failure to pay fines, which has a minimum of 0 months. New crimes committed while on parole can carry longer sentences; see 37 PA Administrative Code §75 for details.

[10] This process additionally gathers data to determine an inmate's placement within their designated facility, which typically has different units and cell blocks. Movement within a prison happens at the discretion of individual unit managers within each facility; for more details, see Harris (2014).

[11] In September, 2016 PADOC began using a computerized inmate assignment decision support system (IADSS) with the goal of improving the efficiency of the classification process. I discuss this in more detail in Section 4.5 and I demonstrate that my results are robust to restricting my sample to inmates who entered prison before this system was in place.

[12] Custody level ranges from 1–5 in increasing levels of security. Level 1 inmates qualify to be placed in a separate

facility, and some additional factors (Pennsylvania Department of Corrections, 2019).[13] PADOC's target time frame for the diagnostic and classification process is 30 working days. SCI Camp Hill is PADOC's designated facility for classification, but during the sample period, a sizable portion of inmates are temporarily held elsewhere before going through the classification process (overall, however, 98% of inmates spend at least some time at SCI Camp Hill for classification).

I observe detailed proxies for key factors used in determining initial inmate assignment. The sentencing data include offense committed (over 500 offense descriptions), sentence length (minimum and maximum), committing county, past prison stays, and type of admission. The diagnostic data include custody level, mental health assessments (each assessment gives a 1–4 score indicating an inmate's level of mental health needs), TCU score (Texas Christian University score, ranging from 0–9; this is the result of a drug screening), and RCT score (Risk Classification Tool score, ranging from 0–9; this is a summary measure of PADOC's assessment of an inmate's criminality).

There are, however, some variables observed by PADOC at the time of initial assignment that I do not observe. For example, I do not know the specific capacity constraints considered during each inmate's initial placement, his individual treatment plan, or his physical health status. To account for this, first, I will control for an inmate's year-month of admission to compare inmates who enter prison around the same time; these inmates should face similar overall capacity across PADOC's prisons but different specific bed availability as they are classified.[14] Further, the sentencing and classification data I do observe should control for the components that determine an inmate's treatments. I will also show that excluding facilities that specialize in treating inmates with specific health conditions does not change my main results (Section 4.5). Finally, to determine the extent to which any other unobservable factors bias my estimates I will rely on a validation test that uses inmate transfers in a quasi-experimental "movers" design.

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set of facilities than the set of SCIs I study; level 5 inmates will be placed in restricted housing. During the sample period, only 0.6% of inmates are ever classified as levels 1 or 5, and no inmates in the sample described in Section 2.2 exit classification with a custody level of 1 or 5.

[13] For example, PADOC considers "separations," which are inmates who should not be housed together, when making assignment decisions. While I do not observe separations, they should not make inmates disproportionately more likely to be assigned to any given facility, only less likely to be assigned to a specific facility.

[14] During my sample period the majority of facilities were overcrowded, so PADOC's accounting of bed availability did not necessarily match the stated capacity of each prison. Instead, PADOC tracked bed availability using an internal bed management system. To my knowledge, the state of this system at the time of each inmate's assignment was not saved, so I was unable to explicitly model the capacity constraints faced by each inmate.

2.1.2 Inmate Transfers

After classification, the large majority of inmates will move to a newly designated facility (some inmates serve their sentence at SCI Camp Hill). Subsequent inmate transfers between prisons can be requested by the inmate or the institution for several reasons at any point during their incarceration (Pennsylvania Department of Corrections, 2019). An inmate or a facility can request a transfer to testify in court near a different prison, to participate in a specific program, due to medical concerns, psychological concerns, special needs, a history of violence (as either victim or perpetrator), a lack of bed space, to separate them from another inmate (for example, if the inmate was the victim or the attacker in a violent incident), to be prosecuted for certain types of misconduct, to be closer to the inmate's home county, and several other reasons. Transfers are subject to space availability, and individuals participating in a prison program or undergoing treatment will generally not be transferred (furthermore, individuals who have pending programs or treatment will only be transferred to facilities that can accommodate those needs).

Leveraging inmate transfers after initial classification can yield a powerful test of the bias in prison misconduct effects. First, initial assignment and inmate transfers are distinct processes. I will test whether the misconduct effect of an inmate's initial prison assignment predicts the change in misconduct when other inmates transfer to that prison. Second, I will test for parallel tends in misconduct around the time of an inmate's transfer. Transfers are seldom an instantaneous process: After being requested, they must be reviewed and approved by PADOC before taking place. Furthermore, PADOC prioritizes moving inmates out of classification and into their permanent facilities; non-classification transfers can take several weeks to several months to be approved and then actualized. In other words, I can test whether inmates have similar trends in misconduct before and after they transfer; I can also test whether, after their transfer, the change in their misconduct is precisely predicted by prison-level misconduct estimates based on initial assignment.

Transfers can take place in response to violent behavior or require that an inmate is not guilty of serious misconduct. I do not observe the specific reason for a given transfer, so I do not know what portion of transfers may be endogenous to misconduct. However, as discussed in the subsection below, misconduct is classified into types by severity. Thus, to address this potential issue, I will separately analyze each type of misconduct, and focus on "minor" misconduct in my main analysis.

2.1.3 Inmate Misconduct

Inmate misconduct comprises a large range of behaviors, from assault or arson to having contraband or disobeying orders. In Pennsylvania, misconduct is classified into types based on severity, major and minor,[15] and inmate misconduct of each type is handled differently (Pennsylvania Department of Corrections, 2022). Appendix Table A1 reports the distribution of the most common major and minor misconducts. The immediate impact of misconduct is the effect on the target of the inmate's behavior and the punishment the inmate receives. If found guilty of a lesser offense, discipline ranges from no action (i.e., no consequence) to revocation of pay for work duties, cell restriction, loss of certain privileges, and other punishments. For more severe offenses, a hearing is conducted to determine guilt. The inmate can present his version of events, and witnesses can give testimony on what happened. The consequences are analogous to those for lesser misconduct, but more severe.

Misconduct can directly threaten the safety of inmates and staff, but it can also lengthen time in prison and undermine rehabilitative efforts. Researchers in criminology primarily focus on the former; studies focus on understanding inmate behavior and the mechanism through which prison conditions affect it (for reviews, see Steiner et al. 2014 and Steiner and Wooldredge 2019). One key issue with attributing differences in misconduct to inmate behavior is that it is not possible to observe misconduct directly: Only records of reported misconduct are available. Therefore, differences in misconduct across prison facilities might be due to different detection, reporting, or conviction propensities, which can affect any behavioral interpretation and policy implications.

Misconduct *records* of both types remain consequential for inmates regardless of any reporting bias. First, punishment is based on reported misconduct; second, reported misconduct is partly used to determine parole decisions. Inmates prepare for their parole interview months in advance in conjunction with a parole agent. One of the requirements to be granted parole is having no misconduct of any type on one's record for a year before being released; parole can also be retroactively revoked if an inmate is found guilty of misconduct.[16] In practice, the impact of misconduct on parole depends on the nature of the offense. The board can consider older instances of misconduct or grant parole despite lesser recent offenses. During my sample period any flexibility in this requirement was at the discretion of each inmate's parole agent. They were also able to consider misconduct that occurred more than a year before the inmate's minimum sentence date. If parole is

[15] Internally, PADOC classifies misconduct into Class I or type A, which I dub "major," and Class II or types B through E, which I dub "minor," based on the severity of the offenses and the types of punishments they can elicit.

[16] See 204 PA Code §309.3 for additional details.

denied, rescheduling an interview can take several months or even a year. Among eligible inmates, 34% are released within 30 days of their minimum sentence date.

There is evidence linking misconduct to future recidivism based on matching by inmate characteristics (Trulson et al., 2011; Cochran et al., 2014; Cochran and Mears, 2017). While it is possible that misconduct is only an indicator of future behavior, another possible mechanism is that inmates are negatively impacted by prison conditions. Rehabilitation, for example, might prove more difficult in a place where bad behavior is normalized. Inmates in different prisons are exposed to different management, security, mix of peers, programs, crowding levels, and several other differences that can effect changes in their outcomes.

2.2 Data and Summary Statistics

I obtained individual-level administrative data on all adult male inmates who served time at a PADOC SCI between 2010 and 2020. Over this period, there were 177,517 prison stays from 115,096 unique adult male inmates. I observe all past and future stays for all inmates present during this period; for each prison stay I observe the reason the inmate entered prison, whether he exited, all his transfers and transfer dates (including his entry and, if applicable, exit), and all instances of misconduct (only if he was found guilty). Most inmates enter prison due to a new sentence or parole violation, but there are other reasons that an inmate can enter prison: an out of state transfer, an escapee that was recaptured, a detentioner being temporarily held, and so forth. Most inmates leave through parole or after their sentence ends; however, they can leave for other reasons, such as a vacated sentence, an erroneous admission, an escape, or death. While data on transfers also include temporary and medical transfers, I do not otherwise observe the specific reason for the transfer. Misconduct data include the date, facility, severity, and description of the incident.

I observe sentencing, diagnostic, and demographic data (age, race, marital status) for all inmates with a prison stay that starts with a new sentence. From 2010 to 2020 there were 91,908 such prison stays (from 86,700 unique inmates). Diagnostic data can be updated during a stay (there can be multiple assessments during and after classification); for each diagnostic variable, I only keep the last record that takes place during classification.[17] I restrict the analysis to inmate stays from new sentences where the inmate remains in prison, leaves through parole, or leaves because his sentence is complete. In addition, I only consider sentences where an inmate starts at classification and is

[17] Mental health assessments are typically conducted after classification in my sample; less than 0.1% of inmates have a mental health assessment during classification. I therefore omit this variable.

then assigned to an SCI. My selected sample includes 80,388 inmates with stays from 2010 to 2020. Prison exit and reentry is right-censored on December 21, 2023; 9,132 inmates from this sample were still in prison as of that date. For my main analysis sample I drop instances where an inmate was transferred for any reason after his initial classification transfer.

Table 1 summarizes the analysis sample compared to all stays in the data. The table demonstrates that the distribution of demographics, custody, offense, and reentry are largely unchanged between the new sentences and the analysis sample. Adding the analysis sample restriction reduces sentence length (as inmates with shorter sentences are less likely to transfer) and misconduct (as inmates that spend less time in prison have less time to commit misconduct and as inmates are more likely to be transferred due to an incident of serious misconduct). The goal of using transfers is to test for bias in my estimates of prison misconduct; hence, I construct a separate sample of inmate transfers that happen after initial classification. I only consider that a transfer has occurred if an individual spends more than a month (30 days) in the transfer facility. This means that even if the transfer is flagged as temporary or medical, if the inmate spends a considerable amount of time at his destination, I mark it as a transfer. Conversely, if a transfer is flagged as permanent but the inmate leaves within the month I do not consider it a real transfer. Finally, I do not consider an inmate's move from classification to his initial location a transfer. (Section 3.3.4 will check the robustness of the movers validation to including or excluding different types of transfers or stays.)

To analyze misconduct among inmates who transfer I construct a monthly panel with each individual's end-of-month location and total misconduct for that month that occurred in that facility. This panel includes all inmates present between 2010 and 2020 with at least one transfer. Multiple transfers are stacked, so an observation in the panel is an inmate-stay-origin-destination-month; there are 2,714,916 observations in the panel. I will estimate monthly misconduct effects using non-transfer stays in my analysis sample and then I will show that I can validate these estimates using this separate sample of inmate transfers. This "movers" design will allow me to test for bias in my misconduct estimates; I will also be able to test whether the estimates are correlated with any unobserved factors by checking for trends in misconduct around the transfer.

3 Empirical Strategy

3.1 Observational Prison Effects

I estimate the effects of prisons on misconduct by controlling for a large number of variables used at classification. For inmate-stay i, initial prison assignment $j(i)$, x_i design controls (offense, minimum and maximum sentence length, custody level, committing county, prior offenses, prior misconduct, drug screening score, risk score, past admissions, past misconduct, misconduct committed during classification, and year-month of entry), I estimate prison misconduct effects μ_j as the prison effect on monthly *minor* misconduct m_i from the regression:

$$m_i = \mu_{j(i)} + x_i'\beta + u_i. \tag{1}$$

As mentioned in Section 2.1.2, I focus on minor misconduct because I will use inmate transfers to test for bias in my estimates, and transfers are potentially endogenous to major misconduct.[18] However, as I will note throughout my analysis, my results are similar when I use major misconduct.

Variation in the observational coefficients $\mu_{j(i)}$ reflects differences in prison-level outcomes among inmates who enter prison around the same time and have observably similar classification data. I only estimate prison effects for an inmate's first facility (after classification) among inmates who were not transferred during their stay. I estimate monthly misconduct effects by weighting Equation (1) by the number of months inmates spent in their first facility after classification. I account for statistical noise in the observational estimates by applying a conventional empirical Bayes shrinkage correction (Morris, 1983). The shrinkage is minimal for most facilities, averaging 0.95.[19]

Figure 1 shows the distribution of monthly minor misconduct in the analysis sample across facilities, and compares them to the distribution of shrunk observational outcomes. Even among observably similar inmates, there remains substantial variation across facilities in major and minor misconduct. Taking an inmate from the bottom to the top decile of facilities by minor misconduct effects would increase monthly minor misconduct by 123%. (Appendix Figure A1 shows the distribution for major misconduct; the analogous move would increase major misconduct by 74%.)

The residual variation in Equation (1) should reflect a combination of the idiosyncratic capacity

[18] I test this formally in Section 3.3.2 and show evidence in Appendix Figure A7 that justifies this concern.

[19] Only 3 facilities receive fewer than 1,000 inmates during the sample, and their shrinkage coefficients average 0.83. Furthermore, the standard deviation of my shrunk estimates (scaled per 100 inmates) is 1.02, while the estimated standard deviation of the underlying effects is 1.04. I continue to use the shrunk version of the estimated misconduct effects, though in practice it makes little difference for my main results, as shown in Panel D of Appendix Table A5.

constraints across facilities that an inmate faces when he enters prison and any unobserved factors that may influence inmate assignment independent of x_i. If such an unobserved factor is correlated with prison misconduct effects or is predictive of individual inmate misconduct, then my estimates will be biased. Thus, I develop two ways to test for nonrandom selection in prison misconduct effects. First, I can test for bias indirectly by leveraging the demographic data available for each inmate. Conditional on the design controls, which include detailed sentencing and diagnostic data on each inmate, demographics should not be the main determinant of inmate assignment, with specific exceptions.[20] However, age, race, and marital status are highly predictive of inmate outcomes (Appendix Table A4). I can therefore test for non-random selection in the observational misconduct effects by testing whether demographics are balanced across these estimates. Section 3.2 below expands on this idea and demonstrates that demographics are balanced relative to the misconduct effects of an inmate's initial prison assignment.

My main validation test uses a quasi-experimental design that leverages inmate transfers. If inmates have parallel trends in monthly misconduct before and after being transferred between prisons, then any changes in their misconduct thereafter can be attributed to differences in treatment effects between the prisons. This approach can only be applied to misconduct; the key insight is to observe that, unlike excess months or reentry, misconduct can happen at any point during an inmate's stay and can therefore be computed as a rate. (This justifies the use of monthly misconduct to estimate μ_j from Equation (1).) Section 3.3 below first builds intuition for this "movers" design and then explains the formal model and validation tests.

3.2 Balance on Observables Validation

Let $y_i \in \{\text{total misconduct}, \text{excess months}, \text{prison reentry}\}$ denote an outcome for inmate i. If prison effects $\mu_{j(i)}$ are uncorrelated with unobservable characteristics net of the design controls x_i, then θ in the following is causal:

$$y_i = x_i'\phi + \mu_{j(i)}\theta + v_i. \tag{2}$$

That is, θ would capture the causal impact on a given outcome y_i of being assigned to a prison with higher or lower misconduct effect. To assess the validity of this assumption, I test for balance against available demographics: age, race, and marital status. Table 2 shows the outcome of a

[20] Inmates aged 18–20 may qualify for a special program at SCI Pine Grove, and Hispanic inmates may qualify for a special program at SCI Chester. In Section 4.5 I show my main results remain unchanged if I exclude these inmates or if I add age and race as design controls.

regression analogous to Equation (2) with the indicated demographic variable as the dependent variable. Age and marital status are balanced relative to minor misconduct effects; however, there is a slight imbalance with respect to race, and facilities with higher misconduct effects are more likely to have a lower percentage of non-White, non-Black individuals. (Appendix Table A2 shows the balance results for major misconduct are similar, with a slight imbalance by race but balanced for age and marital status.)

I estimate predicted outcomes $\hat{y}_i$ from a regression of each outcome y_i on demographics and then test whether prison effects can be linked to predicted outcomes. This provides a summary measure of the balance test that accounts for the relative importance of each demographic for each outcome. Table 3 shows that predicted outcomes are balanced relative to minor misconduct effects (Appendix Table A3 shows they are also balanced relative to major misconduct effects).[21] Specifically, the estimated coefficient on misconduct effects for all predicted outcomes—excess time in prison, reentry, and misconduct—is precisely estimated and close to zero,[22] suggesting a causal interpretation of Equation (2). In other words, there is little to no variation in observable characteristics across facilities that both affects the outcomes of interest and is related to misconduct effects.

It is still possible that there are unobservable characteristics correlated with both misconduct effects and inmate outcomes. A large portion of the variation in the outcomes of interest remains unexplained; if inmates are assigned to facilities based on an unobservable factor that is predictive of misconduct then my estimates will be biased.[23] I therefore rely on a validation test that uses a quasi-experimental movers design, explained in Section 3.3 below. I argue that if observational misconduct effects accurately predict changes in inmate misconduct on a separate sample of inmate transfers then they reflect underlying misconduct treatment effects. In that case, for any unobservable characteristic to bias the results it would need residual variation, net of inmate demographics and the design controls, that predicts changes in inmate misconduct both around initial assignment and inmate transfers. To address this concern, however, I will additionally test whether there are

[21] Appendix Table A4 shows demographics significantly predict the main outcomes of interest and have sizable coefficients. For example, married inmates exit on average about a month earlier than other inmates; white inmates have misconduct rates a third lower than black inmates; an inmate in the bottom decile by age is 5.8 percentage points more likely than the top decile to reenter prison through a new sentence (serious crime), or 69% of the sample's average. As a summary measure, Appendix Figure A2 shows the distribution of predicted excess months and predicted 5-year reentry across individuals have a wide range of values.

[22] Taking an individual from the bottom to the top decile of prisons by misconduct effects would increase predicted misconduct by less than 1%; the increase for excess time in prison is under 0.1% and for serious crime is under 0.5%.

[23] In Section 4, my main specification has an R^2 of 0.31 for excess months and under 0.15 for other outcomes; in Appendix Table A4, R^2 is under 0.04 for the regressions of outcomes on demographics.

any differential trends in misconduct around inmate transfers.

3.3 Movers Validation

3.3.1 Motivating Evidence for Movers Design

Figure 2 shows the average change in minor misconduct of individuals who move to a higher-misconduct-effect facility compared to individuals who move to a lower-misconduct-effect facility (the change is relative to -1, the month before their transfer). Inmates have similar trends in misconduct before being transferred, regardless of the facility they go to. This snapshot of misconduct around transfer time shows roughly parallel trends that diverge after the month of transfer. (Note that the large dip in period 0 is primarily an artifice of the fact that inmates transfer at various points during the month, so misconduct in period 0 reflects roughly half the time as other periods.) Moreover, the change in misconduct for individuals transferred to a higher-misconduct facility is larger than the change for individuals transferred to a lower-misconduct facility, which is the intuitive direction for this comparison.[24]

Figure 3 plots the change in individual minor misconduct before and after their transfer against the difference in destination−origin misconduct effects. In Figure 3a, I compute the individual change in misconduct from 6 months before their transfer to 1 month before; in Figure 3b, I compute the individual change in misconduct from 1 month before their transfer to 6 months after. Importantly, both figures show the full range of moves across all facilities in terms of the difference in destination−origin misconduct effects. A nonzero slope in Figure 3a would indicate that individuals whose misconduct is increasing (decreasing) are sent to higher (lower) misconduct effect facilities, so any correlation with their subsequent change in misconduct would be partly spurious. However, this slope is nearly flat and statistically insignificant (-0.10, SE 0.21) and the slope in Figure 3a is positive and not significantly different from one (1.16, SE 0.18). Together these results suggest that misconduct effect estimates are on average unbiased predictors of causal differences in misconduct across prisons. In other words, Figure 3 provides evidence that trends in individual misconduct are roughly parallel regardless of the differences in destination-minus-origin misconduct before the transfer, and thereafter the trends diverge proportionally to this difference, one-to-one.

[24] Another way to interpret Figure 2 is to note that it illustrates a simple version of the identification strategy, which corresponds to a two-facility case with only two types of transfers that are symmetric (i.e., from the higher- to the lower-misconduct facility and the converse). The next step is to account for J facilities and the $J(J-1)/2$ possible transfers. In this case the scale of the transfer also matters; inmates transferred between facilities that are similar in terms of misconduct should not expect as large a change as inmates transferred between facilities that are very different.

3.3.2 Forecast Coefficient and Event Study

I estimate casual misconduct effects using movers (inmate transfers), and then regress these estimates on the misconduct effects estimated for initial prison assignment from non-movers. Consider a causal model for monthly misconduct:

$$m_{it} = \gamma_{j(i,t)} + e_{it}, \tag{3}$$

where m_{it} is total minor misconduct in month t in inmate-stay i committed in his end-of-month facility $j(i,t)$ and $\gamma_{j(i,t)}$ the treatment effect of facility j on monthly minor misconduct. Now take the projection of γ_j onto observational misconduct effects μ_j

$$\gamma_j = \lambda\mu_j + \eta_j. \tag{4}$$

If $\lambda = 1$, then μ_j unbiasedly predict γ_j.

To test this, I adapt the movers design in Finkelstein et al. (2016) and leverage the variation in misconduct around transfer time illustrated by Figures 2 and 3. Let w_{it} be a vector of individual, month, and relative time of transfer indicators, denoted by α_i, τ_t, and $\rho_{r(i,t)}$, respectively (if t_i^* is the month inmate i is transferred then $r(i,t) \equiv t - t_i^*$ as the relative time of the transfer). γ_j can be identified from movers (inmate transfers) under a parallel trends assumption:[25]

$$E[e_{it}|w_{it}] = \alpha_i + \tau_t + \rho_{r(i,t)}. \tag{5}$$

Let $d(i), o(i)$ be the destination and origin facilities for individual-stay i. Define $\Delta_i \equiv \mu_{d(i)} - \mu_{o(i)}$ as the difference in misconduct effects between inmate i's destination $d(i)$ and origin $o(i)$; define $\widetilde{\alpha}_i \equiv \alpha_i + \gamma_{o(i)}$ as the individual's effect plus the effect of his origin facility. Equation (3) can be written as a difference-in-differences specification[26]

$$m_{it} = \widetilde{\alpha}_i + \tau_t + \rho_{r(i,t)} + 1(r(i,t) \geq 0)_{\text{DiD}} \times \Delta_i + \varepsilon_{it} \tag{6}$$

[25] $\gamma_{j(i,t)}$ are then identified from the observed change in misconduct when an individual's end-of-month facility changes. Note this is only possible when the data include inmate transfers; without these "movers" the individual fixed effects would absorb the prison fixed effects.

[26] See the derivation in Appendix B for more details.

with corresponding event study

$$m_{it} = \widetilde{\alpha}_i + \tau_t + \rho_{r(i,t)} + \lambda_{r(i,t)}\Delta_i + \varepsilon_{it}. \tag{7}$$

I estimate both λ and λ_{DiD} to be approximately 1. Figure 4a sows a weighted (by inmate-months at each facility) regression of the estimated γ_j from Equations (3) and (5) on μ_j from Equation (1). The coefficient is close to unity (1.01; SE 0.30). Furthermore, the difference-in-differences estimate based on Equation (6) gives a precise estimate of 0.99 (SE 0.06, clustered by inmate-stay-origin-destination). This means prison misconduct effects μ_j, estimated for initial inmate assignment among non-movers, predict, one-to-one, mover misconduct effects (recall the movers sample includes only non-classification transfers). The are two possibilities: Either both μ_j and γ_j are estimates of causal prison misconduct effects or both μ_j and γ_j are biased.

I show the parallel trends assumption in Equation (5) is likely to hold. If μ_j and γ_j are both biased, the cause must be an unobservable inmate characteristic that predicts both initial assignment and inmate transfers. If this were the case, we should observe that inmates with higher or lower misconduct systematically transfer to facilities with higher or lower misconduct effects. This would be inconsistent with parallel trends.[27] Figure 4b shows the event study based on Equation (7) (in practice I estimate the event study for a 6-month window around an inmate transfer). It exhibits no pre-trends and the jump is approximately 1. Visually, the confidence intervals around each event-study coefficient for $t < -1$ include 0, and all confidence intervals around each event-study coefficient for $t > 0$ include 1 (the transfer month 0 is highlighted because individuals can transfer at any point during a given month, so most individuals spend the month of their transfer partly at their origin and partly at their destination). Formally, a Wald test for the null that all event-study coefficients for $t = -6, \ldots, -2$ are equal to 0 has a p-value of 0.720.[28]

[27] In this framework, $\lambda_{r(i,t)}$ in Equation (7) are meant to capture the inmate's response to differences in misconduct effects between his destination and his origin. If transfers induce a change in inmate misconduct due to a change in their environment (i.e., the prison they are in), then the $\lambda_{r(i,t)}$ will be flat pre-move (to indicate no change in response to a future environment) and positive post-move (to indicate the extent to which changes in inmate misconduct are caused by their new current environment). If misconduct effects unbiasedly predict misconduct treatment effects, $\lambda_{r(i,t)}$ will be flat and equal to one post-move.

[28] Appendix Figure A7 shows the results for major misconduct. The event study shows a pre-trend; furthermore, several of the confidence intervals for the pre-transfer coefficients include 1. This suggests the movers design only yields a sound test for minor misconduct, not major misconduct. This was expected based on the concern that a significant portion of transfers could be endogenous to major misconduct. Another factor that may be contributing to this issue is that major misconduct is a rarer outcome and thus estimates based on major misconduct are less precise; roughly two-thirds of all misconduct is minor, and the results for major misconduct are much noisier.

3.3.3 Timing of Transfer Approval vs Actualization

I exploit delays between an inmate's transfer approval date and an inmate's actual transfer date to show the is no change in misconduct around the former, only the latter. My estimates would be biased if inmate transfers were partly based on changes in inmate behavior that are observed around the time of their transfer. To test against this possibility, I leverage the fact almost all non-classification transfers (i.e., not for initial placement) are delayed by at least a day, and 3/4 of such transfers are delayed by more than a week. The reason is typically logistics: Once transfers are approved, PADOC has to find bed space for each inmate in their destination facility and arrange for their transport (on a secure bus).

Consider an estimating equation based on Equations (3) and (5) at the weekly level:

$$m_{iw_k} = \alpha_i + \rho_{w_k} + (3/13) \times \gamma_{j(i,w_k)} + \varepsilon_{iw_k}. \tag{8}$$

$k \in \{\text{approval}, \text{actualization}\}$, $w_k \in \{-4, \ldots, 3\}$ indicates the number of weeks before or after inmate-transfer i's approval or actualization, m_{iw_k} is the inmate's misconduct w_k weeks from their approval or transfer, ρ_{w_k} are relative week to approval or week to transfer fixed effects, and $j(i, w_k)$ is either the facility i was approved to be in at week w_k (for k = approval) or the facility he is actually in at week w_k (for k = actualization). Finally, $\gamma_{j(i,w_k)}$ are the monthly misconduct effects described in Section 3.3.2, scaled by the average number of weeks in a month $(12/52 = 3/13)$.

Figure 5 shows the results for $k \in \{\text{approval}, \text{actualization}\}$, as well as the corresponding event studies.[29] There are no pre-trends in either event study, $\lambda^{\text{approval}}$ is not significantly different from 0, and $\lambda^{\text{actualization}}$ is not significantly different from 1 (0.92, SE 0.13 clustered by inmate-transfer). There is no change in inmate misconduct around the time their transfer is approved: The change only happens when their transfer actually takes place.

3.3.4 Robustness of the Forecast Event Study

I verify the robustness of the movers validation test to a number of alternative sample specifications, summarized in Table 4 and Appendix Figure A3. Table 4 shows a Wald test for the existence of pre-

[29] Let $d(i), o(i)$ be the destination and origin facilities for individual-transfer i. Following Section 3.3.2, I estimate the difference-in-differences specifications:

$$m_{iw_k} = \widetilde{\alpha}_i + \rho_{w_k} + (3/13) \times 1(w_k \geq 0) \times \lambda^k \times \Delta_i + \varepsilon_{iw_k}, \tag{9}$$

where $\widetilde{\alpha}_i \equiv \alpha_i + \gamma_{o(i)}$ and $\Delta_i \equiv \mu_{d(i)} - \mu_{o(i)}$.

trends, based on the event study, and the pooled difference-in-differences estimate of the forecast coefficient (λ_{DiD}; corresponding standard errors are clustered by inmate-stay-origin-destination). Appendix Figure A3 shows the event study estimates; when viewed in conjunction, all specifications appear qualitatively similar, albeit with varying levels of precision.

The forecast coefficient from the difference-in-differences specification in Equation (6), λ_{DiD}, is only equivalent to the forecast coefficient λ defined in Equation (4) up to weighting. The movers sample is not balanced and the transfer rates between facilities are not uniform. Furthermore, individuals can appear and transfer multiple times, and for multiple reasons. Table 4 shows that the forecast coefficient is robust to several alternative sample restrictions, each of which also implies different weights. First, including non-transfers in the estimation ("full sample") makes little difference (the coefficient changes by less than 1 standard error; this is not too surprising given the flexibility of the model, where non-transfers should only help identify the time fixed effects).

I restrict the sample to moves where the inmate spent at least 6 months in the origin and the destination facility. For individuals with very short stays in their origin or destination it is arguably unclear whether misconduct, or a lack thereof, represents a trend or a deviation. However, the pooled estimate for this "balanced" sample remains very close to 1 (1.09, SE 0.05) and exhibits no pre-trends (p-value 0.720). Next then I consider restricting the sample to individuals admitted for a new sentence. Individuals admitted from parole necessarily have a criminal history but are not necessarily entering prison for a new crime; they also have significantly shorter stays, so they may behave or be treated differently than individuals who enter to serve a new sentence. While the point estimate is slightly large at 1.15 (SE 0.08), it remains statistically indistinguishable from 1. There is also the possibility that the results are driven by a particular type of transfer. Thus, I exclude, in turn, multiple transfers, temporary (including medical) transfers, and inmates who may quality for an incentive-based transfer (IBT). The non-stacked sample (only using the first transfer) yields a slightly low estimate (0.88, SE 0.08), but all three coefficients remain statistically indistinguishable from 1 and, in all cases, the Wald test yields a non-significant p-value.

3.3.5 Identification of Misconduct Treatment Effects

The key requirement of the model presented in this section is for changes in inmate misconduct to not be systematically correlated with differences in misconduct between their origin and destination facilities. α_i allows for arbitrary differences in misconduct levels across inmates, and any time-invariant factors that affect misconduct and are absorbed by these individual fixed effects. For example, the specification allows for high-misconduct inmates to transfer to high-misconduct facilities. Further, $\rho_{r(i,t)}$ allows for differential trends in misconduct around the time of the move. For example, the transfer decision can be correlated with shocks to misconduct as long as these shocks are not also correlated with misconduct differences between the origin and destination. These two sets of fixed effects also allow individuals who are transferred to differ arbitrarily from individuals who are not transferred both in their misconduct level and in their misconduct trends.

Despite this flexibility, there are some important limitations. First, changes in misconduct cannot be correlated with both the time of the transfer and with misconduct differences between the origin and destination; however, Figure 5 shows this is unlikely to be an issue. Second, the model assumes the individual components of misconduct are additively separable. This form disallows different types of inmates from having heterogeneous prison effects for their misconduct; it also implies that the change in misconduct from a transfer between any pair of facilities j and j' has the same absolute value as the change from a transfer between j' and j. Figure 3 shows some evidence this form is plausible, as both plots have a linear and approximately symmetric form. Finally, the model does not allow for dynamic effects. An inmate initially assigned to a high-misconduct prison could adopt high-misconduct behavior; however, in the model α_i does not depend on past values of m_{it}. Dynamic effects of this type are unlikely to be a major concern, as they would bias λ_{DiD} towards 0 (they would lead the model to overstate the importance of α_i relative to facility effects).

4 Results

Table 5 shows the impact of misconduct effects on inmate outcomes: higher misconduct, longer prison stays, and a higher probability of reentering prison with a new sentence.

4.1 Impact on Misconduct

Inmates assigned to a prison in the top vs. the bottom decile of misconduct effects will have an additional 0.24 major misconduct incidents and 0.80 minor misconduct incidents over the course of their sentence (60% and 98% of the respective averages in the analysis sample). Figures 6a and 6b show binned scatters with the results from Table 5 and contrasts them with the balance test based on Table 3. Both major and minor misconduct increase with minor misconduct effects; by contrast, neither predicted major nor minor misconduct have any relation to minor misconduct effects.

Minor misconduct effects are computed as a monthly rate per 100 inmates; the difference in monthly minor misconduct between the top vs. the bottom decile is ∼2.7, which I multiply by the regression coefficients for major and minor misconduct reported in Table 5. As a benchmark, an increase in monthly minor misconduct of 0.027 for a given inmate would have the equivalent effect over 29 months. On average, inmates spend ∼25 months in prison after classification, so this increase would translate to 0.69 total minor misconduct incidents. The two magnitudes are comparable, and the discrepancy is only due to the fact different inmates spend different lengths of time in prison (i.e., they would be the same if all inmates had identical sentence lengths). Alternatively, a difference of 2.7 minor misconduct incidents per month pre 100 inmates is approximately 85% of average monthly minor misconduct, which is again a comparable magnitude (in percentage terms) to the change in total misconduct between the bottom and top deciles.

4.2 Impact on Time in Prison and Recidivism

Inmates assigned to a prison in the top vs. the bottom decile of misconduct effects will spend an additional 0.7 months in prison (9% of average excess months) and have a 0.9 percentage-point higher probability of reentry from a new sentence within 5 years (11% of the average 5-year reentry rate). These magnitudes can also be interpreted relative to the variation in each outcome across prisons. I show the distribution of observational excess months and observational time in prison in Appendix Figure A4, both based on Equation (1) (replacing misconduct with the corresponding outcome). Taking an inmate from the bottom to the top decile of observational excess-months would increase time in prison by 4.6 months; the change from targeting misconduct effects captures only 15% of this variation. By contrast, taking an individual from the bottom to the top decile of observational 5-year reentry (from a new sentence) increases the probability by 2.0 percentage points; this means the change from targeting misconduct effects captures 46% of this variation.

Figures 6c and 6d show binned scatters of observed and predicted outcomes vs minor misconduct effects for excess months and 5-year reentry from a new sentence. Both observed outcomes increase with prison misconduct effects but there is no change in either predicted outcome across the distribution of misconduct effects. Note that in Table 5 there is no impact on overall reentry. However, inmates are less likely to commit a parole violation (0.46 percentage-point decrease corresponding to one additional minor misconduct per month per 100 inmates), and this effect is almost entirely driven by a decreased likelihood of a technical parole violation (0.43 percentage-point decrease). On the other hand, reentry from a crime increases significantly (0.39 percentage-point increase), largely driven by reentry from a new sentence (0.33 percentage-point increase). This means that while inmates reenter prison at similar rates after being assigned to prisons with higher vs. lower misconduct effects, they substitute lesser technical offenses for new serious criminal activity.

4.3 Improving Low-Performing Prisons

What would happen if we improved the top decile of misconduct-effect prisons to have the same effect as the bottom decile? (Note that the previous section only discusses what would happen to the average inmate.) Each year, 463 inmates are released from the two facilities in the top decile. An additional 0.7 months per inmate translates to ∼309 excess months served. The average monthly cost per inmate in these facilities is approximately $4,400, so this translates to $1.4M in costs over the course of a year.[30] Abrams and Rohlfs (2011) show that willingness to pay for freedom is $1,000 for 90 days in 2003 dollars; adjusted for inflation, this is approximately $1,000 per month, or $310,000 over the course of a year.

A 0.9 percentage-point increase in the probability of a new sentence translates to ∼4.2 additional serious crimes over 5 years from these facilities (∼4.9 for any criminal conviction). However, there are two potential trade-offs. First, we should account for the reduction in less serious offenses and technical violations, which are similar in magnitude. Second, additional time in prison can affect recidivism, both through a direct incapacitation effect as well as indirectly (Bushway and Owens, 2013; Mastrobuoni and Rivers, 2016). Note that for the incapacitation effect to offset the increase in new crime, the latent criminality of the incapacitated population would need to exceed 100%.[31] Furthermore, both Bushway and Owens (2013) and Mastrobuoni and Rivers (2016) examine sentences that became unexpectedly shorter, via a policy change and pardons, respectively. By contrast, I examine time in excess of an inmate's minimum release date: In my sample inmates are seldom released before their minimum date (and I explicitly exclude pardons and commuted sentences from my analysis).

4.4 Policy Simulation

For most of my sample period, PADOC SCIs operate at over 100% capacity, meaning that the number of inmates exceeds the number of beds that the prison is rated for. Appendix Table A9

[30] While the marginal cost should be lower than the average cost (of keeping an inmate in prison for a month), even if it were 10 times lower, total costs here would remain high at $140,000.

[31] If an inmate spends additional time in prison they cannot commit a crime outside of prison. Thus, it is possible the increase in crime over the long-term is offset by a short-term reduction in crime from this incapacitation effect. In this case, the incapacitation effect does not offset the future increase in crime even if I assume every inmate would have comitted a crime upon early release.

shows that the prisons with higher misconduct effects also have a higher excess inmate population: 80% of facility-months from 2010–2019 end with over 100% capacity; only 3 SCIs during that period have an average occupancy rate under 100% (and one of them is SCI Phoenix, which opened mid-2018). While the inmate population for PADOC SCIs remained relatively steady from 2010–2019, averaging 41,000–43,000 each year, it declined sharply after March 2020, to roughly 28,000. This decline coincides with the COVID-19 pandemic; the population has since increased, but it has yet to reach pre-pandemic levels. As of December 2023 there were 35,414 inmates in male-only SCIs and almost 3,500 open beds (excluding Camp Hill, which is used as PADOC's intake facility for male inmates), with only 3 SCIs at over 100% capacity. On average, approximately 400 inmates enter male-only PADOC SCIs due to a new sentence each month, and it is possible to front-load the assignment of new inmates to facilities based on misconduct effects while accounting for capacity constraints in order to reduce overall misconduct.

I consider a counterfactual where the prisons with the lowest misconduct effects take on more inmates until they reach (near) capacity, while other facilities continue to receive inmates as normal.[32] This would result in changing the assignment of approximately 20% of the entering inmate population to be purely based on misconduct effects, resulting in a decrease in major and minor misconduct, time in prison, and reentry due to new crime. Table 6 reports the magnitudes of the reductions that result from this policy simulation, assuming that prison misconduct effects remain constant after this change to the assignment process. The results suggest that it is possible to achieve substantial reductions in misconduct among inmates that follow this new assignment rule (by 28.5% for major and 45.4% for minor misconduct), decrease their time in prison (by 4.2%), and decrease future crime (by 5.3%, at the expense of an increase in parole violations).[33]

Table 6 also shows there is limited scope for improving outcomes across all inmates by reassigning only a portion based on misconduct effects. One intuitive alternative would be to apply this assignment rule to all inmates. However, this exercise relies on the assumption that misconduct effects remain constant under the new assignment rule. Applying it to only 20% of inmates translates to less than 3% of the inmate population in a given year; applying it to all inmates would affect 14% of the inmate population over the course of a year. In the latter scenario, the assumption that misconduct effects remain constant becomes implausible, as changing the composition of inmates

[32]In Section 5 I find that overcrowding is one of the factors associated with higher misconduct effects. However, Appendix Figure A6 suggests overcrowding only has a negative impact when facilities are at *excess* capacity, i.e. over 100% occupancy.

[33]The pooled reduction in major and minor misconduct is $(0.12 + 0.37)/(0.41 + 0.82) \approx 0.4$

across facilities may also change their misconduct effects.[34]

4.5 Robustness Checks

Panel A in Appendix Table A5 shows the impact of major misconduct effects on inmate outcomes. Except for excess time in prison, where the major misconduct effect predicts a change that is over 50% larger (1.05 vs 0.67), the impact of going from a prison in the top vs. bottom decile of major or minor misconduct is very similar. Panel B shows the impact of unadjusted minor misconduct on inmate outcomes. With design controls, the estimates remain very similar; furthermore, while the forecast coefficient λ becomes biased (0.81, SE 0.05), unadjusted misconduct remains highly predictive of misconduct treatment effects and the event study has no pre-trends (a Wald test for an event study based on Equation (12) using unadjusted minor misconduct instead of μ_j yields a p-value of 0.25). This further suggests that inmates are not being assigned to facilities based on minor misconduct given the design controls. Without the design controls, however, the impact of being assigned to prisons with high vs. low misconduct would be estimated to be more than twice as large (Panel C).

Appendix Table A6 shows the main results for several subsamples. First, I exclude inmates aged 18–20 and inmates of race "other," which are two demographics disproportionately more likely to go to specific facilities.[35] Next I restrict the sample to individuals who have not been to prison before: I only observe the type and count of past admissions, not any details therein; however, for first-time offenders, there are no historical data to consider. I also restrict the sample to individuals who do not commit any misconduct prior to being assigned a prison, as past misconduct is highly predictive of future misconduct. In all three cases, the results are qualitatively unchanged; furthermore, the only quantitatively significant change is when excluding inmates with any past misconduct, which slightly mutes the impact of prison misconduct on total individual misconduct (from a coefficient of 0.29 to 0.25). Finally, Panel D shows the results of dropping inmate admissions after March, 2020 *and* right-censoring all outcomes at that date. This assesses whether my results are biased by the COVID-19 pandemic. The main change is that the impact on criminality becomes more pronounced, but remains within a standard error of the estimate in my main specification.

[34] Peer effects can significantly affect inmate outcomes (Bayer et al., 2009; Stevenson, 2017; Harris et al., 2018) and in Section 5 I find that the criminality of peers is correlated with my estimated misconduct effects. My argument in this section is that misconduct effects are plausibly invariant to reassigning only a *small* portion of inmates.

[35] I do not observe Hispanic status for most inmates; I use race "other" as a proxy. In addition, Appendix Table A8 Panel A shows the main results with age and race fixed effects added as design controls. The estimates are not significantly different, which is expected given previous tests.

As noted in Section 2.2, I do not observe physical health data, which can be considered when making assignment decisions. PADOC, for example, has to make reasonable accommodations for inmates with disabilities (Pennsylvania Department of Corrections, 2009), and an inmate's disability status may be predictive of his misconduct. Table 4 demonstrated that the movers validation is robust to excluding all medical and temporary transfers. Furthermore, Appendix Table A7 shows that excluding facilities that specialize in accommodating inmates with disabilities does not qualitatively change the main results. If anything, the effect on outcomes of assigning an inmate to a high- vs. low-misconduct prison becomes more pronounced for time in prison and serious crime.

In September, 2016 PADOC introduced an inmate assignment decision support system (IADSS) with the goal of improving the classification process and reducing costs.[36] While the final assignment decision continues to be made by an individual, the IADSS is able to make facility recommendations for sets of inmates simultaneously, taking into account bed space, assignment rules, and placing a higher priority on an inmate's distance from home. Appendix Table A8 Panel C shows the main results using only inmates who enter prison before IADSS was in place. Being assigned to a prison in the top vs. bottom decile of misconduct effects has a similar impact on inmate outcomes when compared to my main specification: It increases major misconduct by 0.55 (vs. 0.67), minor misconduct by 0.22 (vs. 0.24), excess months in prison by 0.71 (vs. 0.80), and reentry from a new sentence by 0.99 percentage points (vs. 0.90). One limitation of this analysis is that I am not powered to examine long-term inmate outcomes post-IADSS; I would only observe 5-year reentry for 8% of inmates if I restricted the sample to post-IADSS entries (note that reentry is computed relative to an inmate's exit; inmates who enter the after IADSS would need to first serve their sentence, which averages over 3 and a half years).[37]

5 Correlates of Prison Misconduct Effects

Steiner et al. (2014) conduct a systematic review of studies that analyze the causes of inmate misconduct. They find that the following prison-level characteristics are significant predictors of misconduct in a majority of studies: fraction of inmates under 25, fraction black, crowding, population,

[36] For details on current version of the system, see https://optamo-llc.com/products.

[37] The correlation coefficient between misconduct effects estimated separately pre- and post-IADSS is 0.55, and a regression of post-IADSS misconduct effects on pre-IADSS misconduct effects yields a coefficient of 0.91 (weighted by the number of inmates in each facility). Note that this does not say anything about the impact of the IADSS on misconduct; rather, it shows that *relative* differences in misconduct effects across prisons before IADSS was implemented remains highly predictive of differences in misconduct effects after its implementation.

and security level. I examine these as well as the fraction of individuals convicted of a sex-related crime, convicted of a drug-related offense, who are single, and with a prior prison stay, the average minimum sentence length, the average risk (RST) and drug screening (TCU) scores, the average distance from the prison to the inmate's committing county, the average monthly population, the average rated capacity, the average percent occupied, security and staff per 100 inmates, and the cost per inmate during the 2020 fiscal year. The data for capacity and staff are from PADOC's publicly available monthly population reports[38]; the data on costs come from PADOC's budget reports. I group each variable by whether it is based on the average characteristics of inmates or if it is a facility characteristics. One limitation of this analysis is that I am not able to explicitly test differences in prison programs or management policies, which are two potentially important categories that may help explain differences in prison misconduct effects (French and Gendreau, 2006; Taxman and Blasko, 2016; Wooldredge, 2020).

Figure 7 shows observable correlates of observational misconduct effects; all covariates in the figure are standardized. I do not find an effect for either security level or demographic composition. However, there is also some experimental evidence suggesting that being assigned to a higher security level prison increases minor misconduct (Tahamont, 2019) but not major misconduct (Gaes and Camp, 2009). Appendix Figure A5 presents an analogous figure with correlates of raw average facility-level monthly misconduct. Security level and demographics (age, fraction single) are significantly correlated facility-level characteristics. The observational estimates suggest that several of these correlations are driven by selection. The contrasting evidence on prison security might be due to heterogeneity in how prison systems use this classification. In Pennsylvania, for example, each prison has bed space for different inmate security levels.

I find that facilities with higher occupancy, fewer drug offenders, more sex offenders, longer sentences, and higher TCU scores are all predictive of higher misconduct effects. The negative relation to overcrowding is consistent with the criminology literature; furthermore, excess capacity is also one of the key features of the newer prisons studied by Tobón (2022) and Mastrobuoni and Terlizzese (2022) that have a large and negative impact on recidivism (Appendix Figure A6 suggests the deleterious effect from overcrowding only appears when facilities are at excess capacity, i.e. over 100% occupancy). Moreover, peer effects are consistent with quasi-experimental evidence that being exposed to peers of higher criminality increases an inmate's own criminality (Bayer et al., 2009; Stevenson, 2017; Harris et al., 2018). These results, while suggestive, have two important

[38]Monthly population reports are available dating back to 2000 at pa.gov.

limitations. First, I only observe 26 facilities in my sample, meaning all the results in this section are based on a small number of observations. Second, I was not able to test the impact of every prison characteristic; in future work it will be important to test other dimensions of the prison environment (for example, management practices and program quality).

6 Conclusion

This paper presents evidence on the impact of prison misconduct on inmate outcomes using a decade of data from correctional institutions in Pennsylvania. I estimate a prison misconduct effect for each prison from a selection-on-observables regression that controls for a rich set of sentencing and assessment variables used to assign inmates to prisons (design controls). I validate these estimates in two ways. First, I show that inmate demographics are balanced relative to prison misconduct effects. Second, I use a "movers" design to demonstrate that prison misconduct effects accurately and precisely forecast changes in misconduct on a separate sample of inmate transfers. Any unobserved characteristic that biases the estimates would need to have residual variation net of the design controls and inmate demographics that biases misconduct around inmate transfers in the same way (otherwise I would observe a pre-trend in the movers event study or a biased forecast coefficient).

Taken as causal, my estimates imply that taking an inmate from the bottom to the top decile of facilities by misconduct effects would increase excess time in prison by 0.7 months and the probability of being sentenced for a new crime within 5 years by 0.9 percentage points (9% and 11% of their respective averages). From a cost perspective, the additional time in prison from the top decile of misconduct effect prisons, relative to the bottom decile, translates to $1.4M a year in costs to PADOC (based on an average cost per inmate of $4,400) and $310k in cost to the inmate (based on a willingness to pay for freedom of $1,000 per month).

The main policy implication is that prison systems in the US can examine how their prisons affect inmates by estimating and analyzing prison misconduct effects. I show that one potential way to leverage this result is to take into account prison misconduct effects during inmate assignment, which can significantly reduce inmate misconduct, time in prison, and future criminality. I find that the composition of offender types and overcrowding are significant predictors of prison misconduct effects, which suggests that prison systems should target overcrowding and specific types of offenders to reduce misconduct (the latter due to the externalities they impose on other inmates).

This paper has three main limitations. First, I only study correctional institutions in Pennsylvania; each US state can have multiple types of facilities and use a different process for inmate assignment. However, as suggested above, future work could use the methods developed in this paper to test how different prisons affect inmate outcomes in other US states. Second, I do not observe recidivism. I cannot analyze with precision what happens to inmates after they leave prison: I only observe whether they reenter through parole or a new sentence (however, one of my main results is that while overall reentry is not impacted, inmates substitute technical violations for new criminal activity). Future work should focus on analyzing a wider range of long-term outcomes for former inmates, including arrests, charges, and employment, among others. Last, I am not able to precisely test the mechanism by which prison misconduct affects inmate outcomes; future work should also directly test the impact of prison management and programs, which have been shown to affect inmate outcomes (French and Gendreau, 2006; Taxman and Blasko, 2016; Wooldredge, 2020; Arbour et al., 2023; Alsan et al., 2024). It remains to be seen exactly what policies can reduce misconduct and improve inmate outcomes across different prisons.

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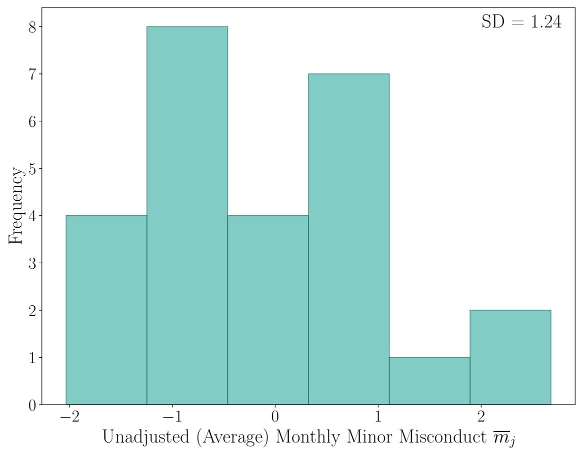
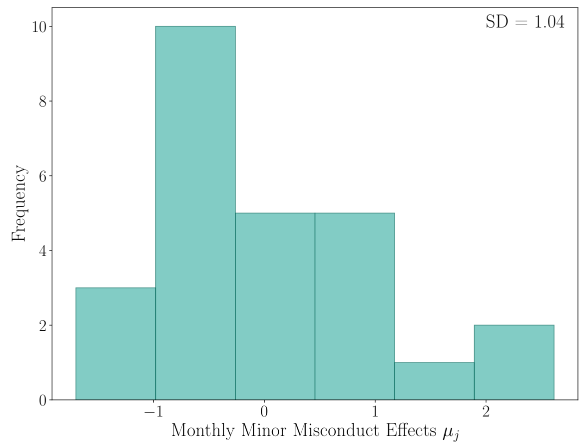
Figures and Tables

Figure 1: Distribution of Minor Misconduct Across PADOC SCIs

(a) Average Misconduct

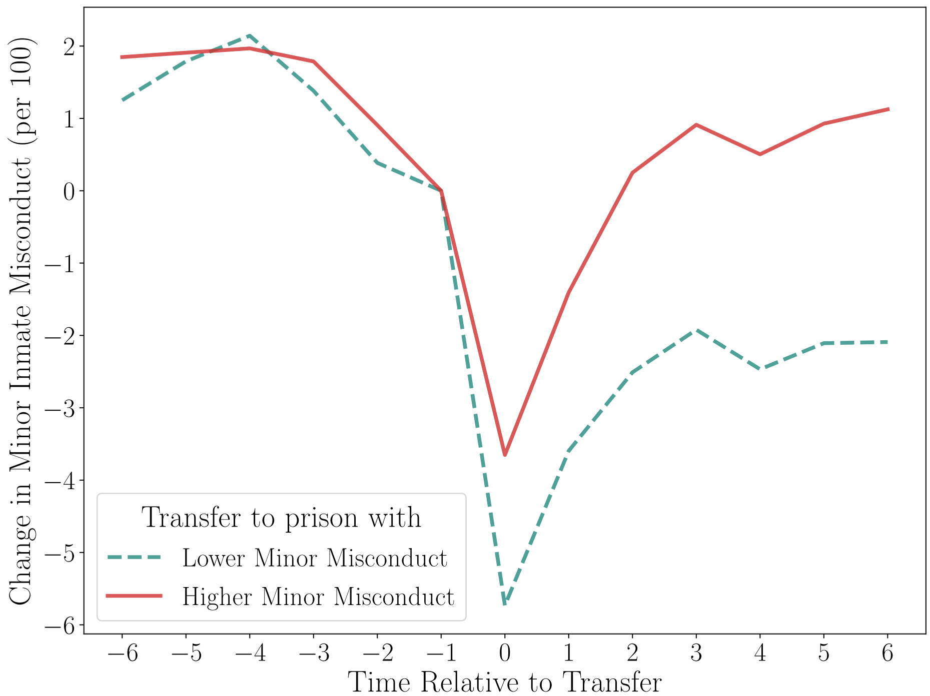


(b) Monthly Misconduct Effects



The figures show the distribution of monthly minor misconduct (per 100 inmates) estimated for inmates' initial facility assignment, centered around the sample average. The left figure shows the average for each prison $\overline{m}_j$ across inmates in the analysis sample (see Table 1). The right figure shows the estimated misconduct effects μ_j based on Equation (1) using that same sample, shrunk applying an empirical Bayes correction.

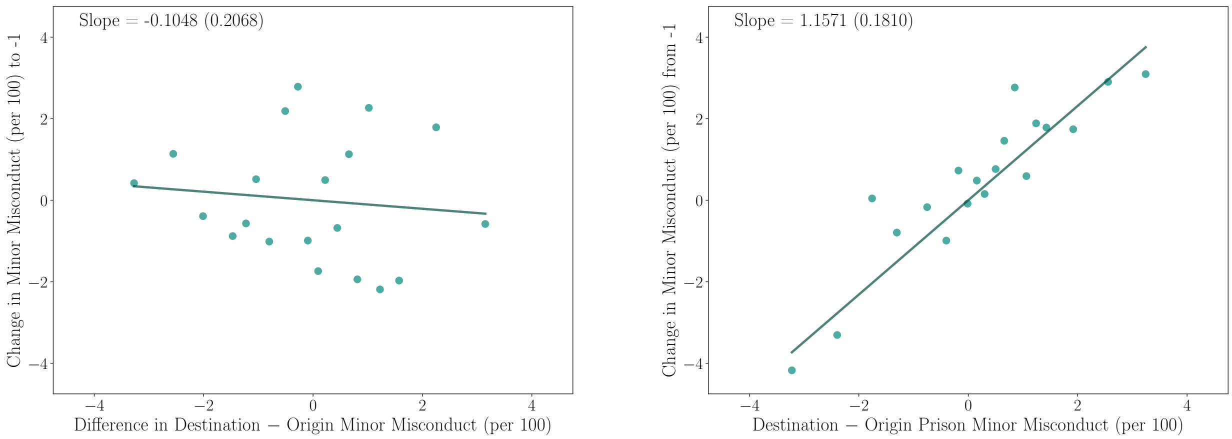
Figure 2: Average Minor Misconduct Around Transfer Time by Transfer Direction



The figure shows the average change in minor misconduct in a 6-month window around a transfer, relative to the month before the transfer (coded as -1). Changes is misconduct are computed for two groups: individuals whose destination had a higher misconduct effect than their origin and individuals whose destination had a lower misconduct effect. Only minor misconduct incidents at the individual's end-of-month facility count towards the total in a given month. The dip during the month of transfer $t = 0$ is artificial since inmates can move at any time of the month, and on average they spend 15 days at their destination during that period.

Figure 3: Change in Minor Misconduct Relative to Pre-Transfer Period

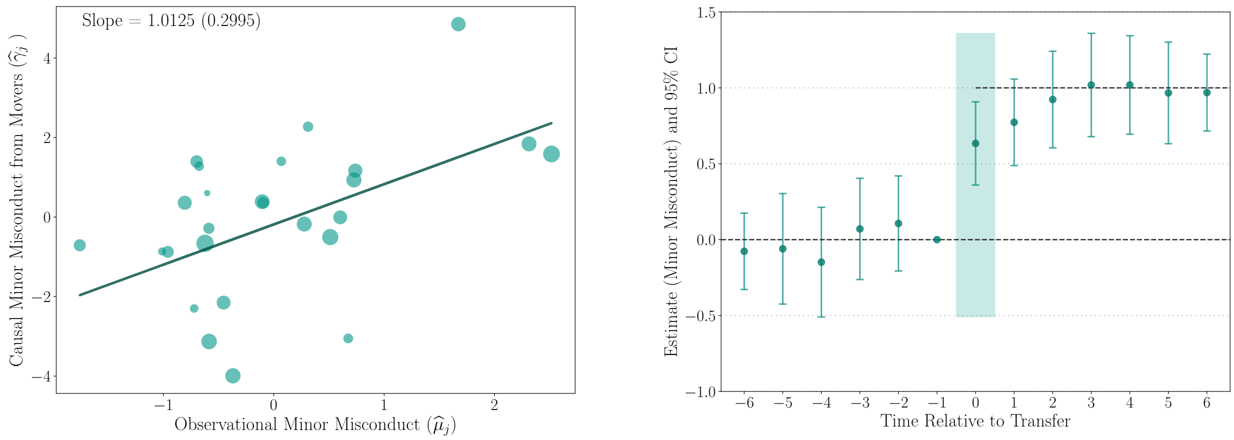
(a) 6 months before transfer (b) 6 months after transfer



The figure shows the change in misconduct before and after transfer by ventiles of the difference in destination−origin misconduct effects. For each inmate I calculate the change in misconduct from 6 months to 1 month before the transfer (left panel) and from 1 month before to 6 months after the transfer (right panel). The ventile average of each variable is shown. The line represents the best linear fit from a simple OLS regression using the data points shown in the graph.

Figure 4: Forecast and Event Study Estimates

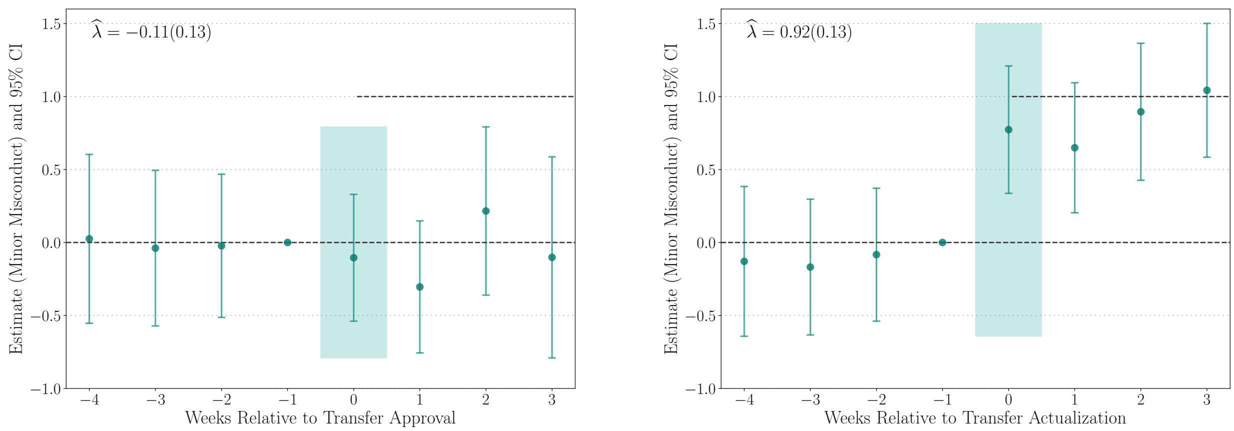
(a) "Movers" $\widehat{\gamma}$ and Observational $\widehat{\mu}$ (b) Forecast Event Study



The left figure shows a scatter of the estimated $\widehat{\gamma}_j$ from Equation (3) vs the observational estimates $\widehat{\mu}_j$. The size of each dot is weighted by the size of the corresponding facility; a linear fit based on a weighted bivariate regression is overlaid. The right figure plots the estimated coefficients and corresponding 95% confidence intervals from an event study based on Equation (6). The dependent variable m_{it} is total minor misconduct in the inmate's end-of-month location $j(i,t)$; this means that period 0 the month of the transfer is only a partial treatment month. For each period I estimate $\widehat{\lambda}_{r(i,t)}$ where $r(i,t)$ is the number of months at t relative to i's transfer during his stay. $\widehat{\lambda}_{-1}$ is normalized to 0; $\widehat{\lambda}_{-6}$ and $\widehat{\lambda}_6$ also group periods before or after 6 months relative to the transfer, respectively. For each inmate-stay, Δ_i is the difference in destination and origin observational misconduct, $\mu_{d(i)} - \mu_{o(i)}$. Standard errors are clustered by inmate-stay i. In both the left and right figures, the observational estimates $\widehat{\mu}_j$ are shrunk by applying an empirical Bayes correction.

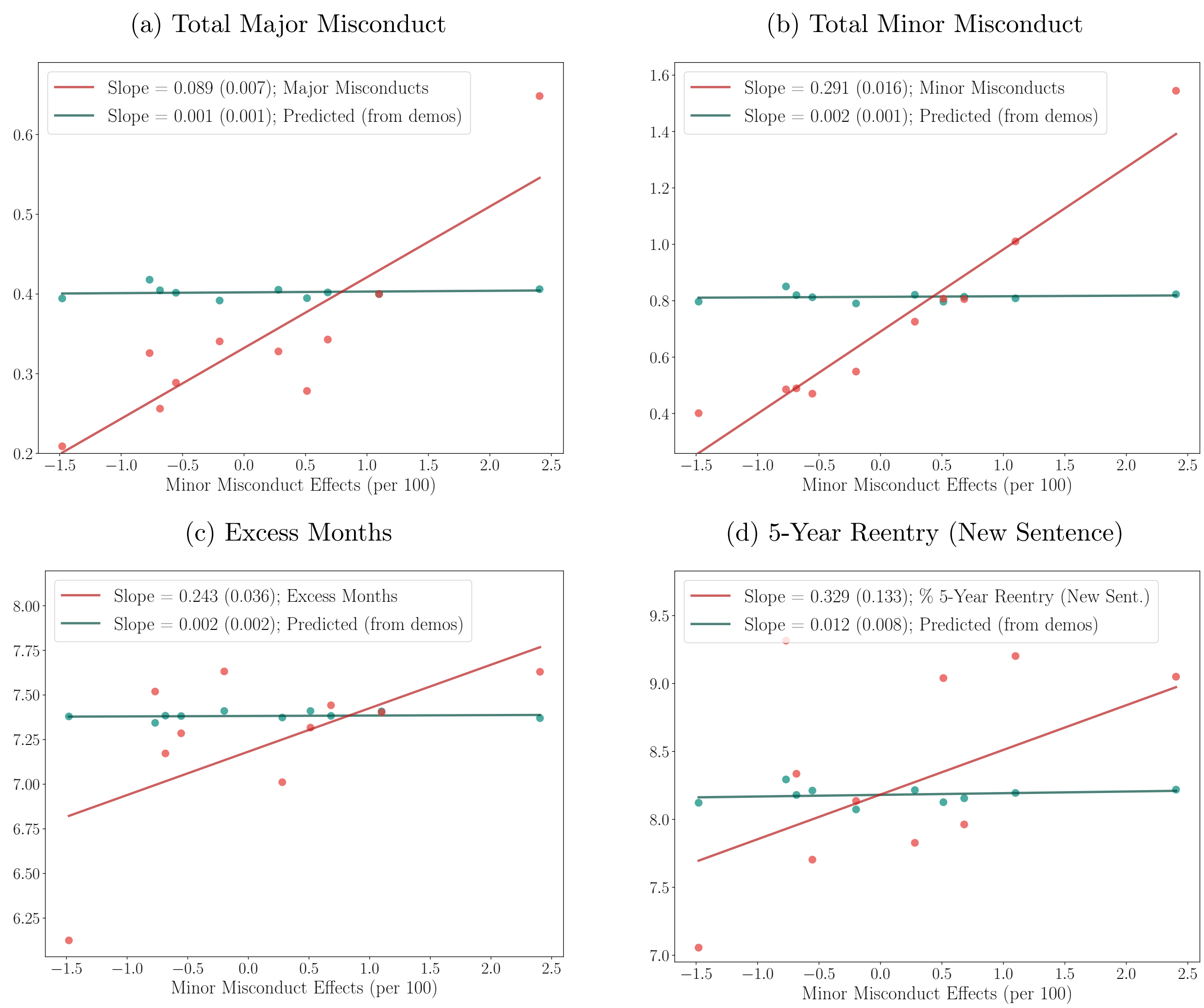
Figure 5: Forecast Event Study Estimates around Transfer Approval vs Actualization

(a) Transfer Approval (b) Transfer Actualization



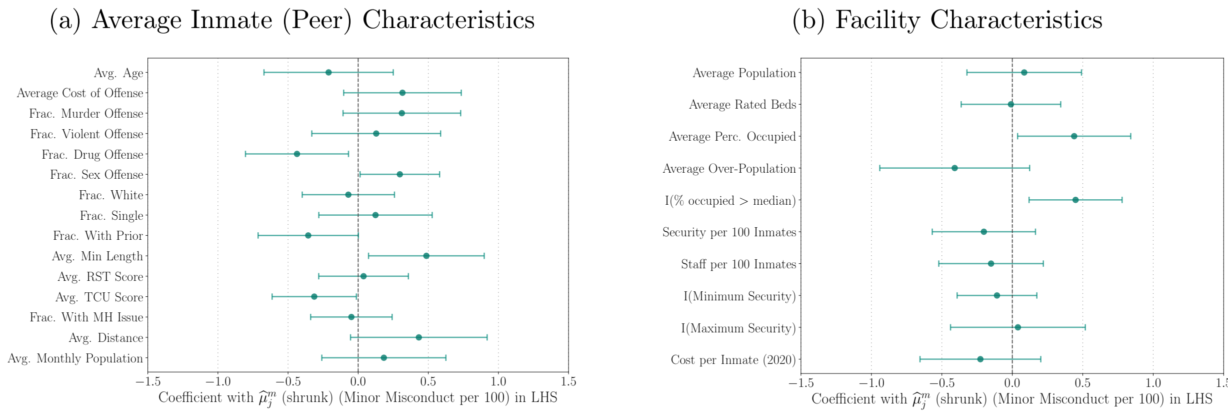
Each figure shows an event study based on Equation (9), plots each estimated coefficient and corresponding 95% confidence intervals, and shows the pooled difference-in-differences coefficient $\widehat{\lambda}$. The dependent variable is misconduct for the week relative to an inmate's transfer approval date (left) and actual transfer date (right). Inmates are grouped relative to the date of their transfer approval or actualization, meaning that week 0 includes days from the reference date until 6 days after, week 1 includes days 7 to 13, and so forth. For each period I estimate $\widehat{\lambda}_w$ where w is the number of weeks since inmate-transfer i's approval or actualization date. $\widehat{\lambda}_{-1}$ is normalized to 0. For each inmate-stay, Δ_i is the difference in destination and origin observational misconduct, $\mu_{d(i)} - \mu_{o(i)}$, scaled by 12/52, the inverse of the average number of weeks in a month. Inmates exit the transfer approval sample when their transfer is actualized. Standard errors are clustered by inmate-transfer i.

Figure 6: Outcomes and Predicted Outcomes vs Observational Minor Misconduct



Each figure shows a binned scatter corresponding to Equation (2), with the indicated outcome in place of y_i. Each binned scatter was constructed by regressing the predicted or observed outcome on deciles of minor misconduct effects with the design controls specified in Table 2; the resulting coefficients are plotted on the vertical axis against average misconduct effect in each bin on the horizontal axis. A linear fit based on a regression of the indicated predicted or observed outcome on misconduct effects is overlaid. See the notes to Tables 3 and 5 for information on each predicted or observed outcome and the design controls.

Figure 7: Bivariate Regressions of Misconduct Effects on Selected Characteristics



The figure shows weighted bivariate OLS regression results of estimated misconduct effects $\widehat{\mu}_j$ on a set of prison-level characteristics. Weights are given by the number of inmate-stay-months in each prison. The left figure shows the results for covariates computed using average inmate characteristics; the right figure shows covariates that describe the facility's environment. All independent covariates are standardized to have mean zero and standard deviation one. Horizontal bars show 95% heteroskedasticity-robust confidence intervals.

Table 1: Summary Statistics for Inmate Stays at PADOC SCIs between 2010–2020

		All Stays			New Sentences			Analysis Sample		
		N (1000s)	Mean	SD	N (1000s)	Mean	SD	N (1000s)	Mean	SD
Race (%)	White	177.5	47.4	49.9	91.9	48.8	50.0	61.3	50.6	50.0
	Black	177.5	43.4	49.6	91.9	42.4	49.4	61.3	40.1	49.0
	Other	177.5	9.2	28.9	91.9	8.8	28.3	61.3	9.3	29.1
Age (%)	18–24	177.5	16.7	37.3	91.9	24.0	42.7	61.3	23.5	42.4
	25–29	177.5	19.1	39.3	91.9	19.9	40.0	61.3	19.6	39.7
	30–34	177.5	17.0	37.5	91.9	16.4	37.0	61.3	16.3	37.0
	35–39	177.5	12.8	33.4	91.9	12.0	32.5	61.3	12.2	32.7
	≥ 40	177.5	34.1	47.4	91.9	27.2	44.5	61.3	28.4	45.1
Custody (%)	2	177.5	10.6	30.8	91.9	19.9	39.9	61.3	23.2	42.2
	3	177.5	24.3	42.9	91.9	46.4	49.9	61.3	49.8	50.0
	4	177.5	10.7	30.9	91.9	20.3	40.2	61.3	18.9	39.1
Offense (%)	Murder	160.2	4.1	19.9	91.9	5.3	22.4	61.3	3.3	17.9
	Rape or SA	160.2	4.6	21.0	91.9	6.4	24.6	61.3	6.3	24.2
	Other Violent	160.2	25.5	43.6	91.9	24.6	43.1	61.3	23.5	42.4
	Drugs	160.2	26.0	43.9	91.9	24.7	43.1	61.3	26.3	44.0
	Other Misc	177.5	35.9	48.0	91.9	39.0	48.8	61.3	40.6	49.1
Assessments	RST	91.7	5.4	1.7	90.2	5.4	1.7	60.9	5.3	1.7
	TCU	92.2	4.0	3.1	90.2	4.0	3.1	60.8	4.0	3.1
Sentence in Months	Minimum	159.1	4.6	77.9	90.8	34.6	84.0	60.9	25.5	73.8
	Maximum	159.0	63.3	149.7	90.7	91.4	183.4	60.9	74.3	168.9
Transfer (%)		114.3	25.3	43.5	90.8	24.6	43.1	61.3	0.0	0.0
Exit (%)		177.5	92.4	26.5	91.9	89.1	31.2	61.3	93.0	25.6
Misconduct	Total Major	177.5	0.7	3.1	91.9	1.0	3.8	61.3	0.4	1.7
	Total Minor	177.5	1.2	4.6	91.9	1.7	5.7	61.3	0.9	3.2
Excess Months		81.6	8.8	12.7	77.4	8.7	11.8	57.0	7.4	10.2
Reentry (3-years, %)	Any	145.6	41.6	49.3	71.0	35.2	47.8	51.4	35.8	47.9
	Parole	145.6	28.8	45.3	71.0	24.6	43.0	51.4	25.3	43.5
	Crime	145.6	15.1	35.8	71.0	12.8	33.4	51.4	12.7	33.3
	Technical	145.6	19.5	39.6	71.0	16.0	36.7	51.4	16.4	37.1
	Serious Crime	145.6	6.3	24.4	71.0	4.2	20.1	51.4	4.0	19.6
Reentry (5-years, %)	Any	115.3	49.3	50.0	56.4	42.5	49.4	42.1	42.5	49.4
	Parole	115.3	32.6	46.9	56.4	28.4	45.1	42.1	28.9	45.3
	Crime	115.3	23.5	42.4	56.4	19.6	39.7	42.1	19.3	39.5
	Technical	115.3	22.0	41.4	56.4	18.5	38.9	42.1	18.7	39.0
	Serious Crime	115.3	12.4	33.0	56.4	8.5	27.9	42.1	8.3	27.6

Notes. All stays refers to all individual stays that take place in one of the 26 male-only PADOC SCIs in operation between 2010–2020. While offense categories were grouped from over 500 offense descriptions, there are only 430 offense descriptions in the analysis sample. Parole refers to individual stays that start with a parole violation but are not associated with a crime; crime are stays from a new conviction, either by a convicted parole violators (CPV) or due to a new sentence imposed by a court; technical are stays from a technical parole violator (TPV); serious crime are stays from a new sentence imposed by a court. The analysis sample refers to individuals who start their sentence at classification and are then transferred to their permanent location for the rest of their stay.

Table 2: Balance of Demographics vs Prison Minor Misconduct Effects

	% White	% Black	% Other	% Single	% Married	% Other	Age
μ_j	0.091	0.273	-0.364	0.011	-0.035	0.025	0.003
	(0.157)	(0.162)	(0.107)	(0.154)	(0.131)	(0.113)	(0.033)
N	61,283	61,283	61,283	60,885	60,885	60,885	61,283
R^2	0.392	0.327	0.134	0.183	0.079	0.116	0.408
Average	50.6	40.1	9.3	75.2	14.1	10.7	34.4

Notes. Each column shows the result of a regression of the indicated demographic on minor misconduct effects (per 100). All specifications include the following design controls: Year-month of admission, priors, offense, sentence length, custody level, committing county, misconduct during classification, misconduct during prior admissions, and, when available, TCU and RST scores. TCU score is the Texas Christian University drug screening score (higher means more at risk); RST score is a risk assessment score computed by PADOC based on an inmate's characteristics and history (higher means more risky); custody level is 1–5 (higher is higher security; all inmates in the sample are classified 2–4). Robust standard errors in parentheses.

Table 3: Balance of Predicted Outcome vs Minor Misconduct Effects

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
μ_j	0.002	0.001	0.002	0.033	0.011	0.026	0.007	0.012
	(0.002)	(0.001)	(0.001)	(0.030)	(0.019)	(0.018)	(0.012)	(0.008)
N	60,885	60,885	60,885	60,885	60,885	60,885	60,885	60,885
R^2	0.319	0.411	0.415	0.379	0.372	0.378	0.360	0.342
Average	7.4	0.40	0.81	42.1	28.7	19.1	18.6	8.2

Notes. Each column shows the result of a regression of the indicated predicted outcomes on minor misconduct effects (per 100). Predicted outcomes are from a regression of each indicated outcome on age, race (white, black, other), and marital status (single, married, other). Excess months are total months between minimum sentence and release date; total misconduct is all misconduct after classification; any reentry means entry to a PADOC SCI to serve any amount of time; parole means reentry through a parole violation that is not associated with a new crime; crime means reentry associated with any new conviction, either a convicted parole violator (CPV) or a new sentence imposed by a court; technical means reentry as a technical parole violator (TPV); serious crime means reentry because of a new sentence imposed by a court. All specifications include the design controls specified in Table 2. Robust standard errors in parentheses.

Table 4: Misconduct Effect Validation for Monthly Minor Misconduct

		Pooled Estimate ("Jump")	Wald Test (No pre-trend)
Sample	N	$\widehat{\lambda}$	p-value
Movers	2,714,916	0.994 (0.064)	0.720
Full Sample	6,827,318	1.091 (0.055)	0.720
Balanced	1,780,953	1.066 (0.073)	0.395
New Sentence	1,470,765	1.147 (0.082)	0.928
Non-Stacked	1,224,854	0.877 (0.083)	0.796
Non-Temporary	1,160,763	0.926 (0.084)	0.980
No IBT Candidates	347,306	1.031 (0.152)	0.861

The table shows the estimated difference-in-differences (pooled) coefficient based on the regression specification in Equation (6) for each indicated sample. Standard errors in parentheses are clustered by inmate-stay i. Next are the p-values from a Wald test of the null that all pre-transfer coefficients ($t = -6, \ldots, -2$) are equal to 0. "Movers" include all inmate moves (stacked); "full sample" adds non-movers to the estimation; "balanced" includes only inmates with at least 6 months in both their destination and origin facilities before and after their transfer; "new sentence" considers only inmates who enter through a new sentence; "non-stacked" includes only an inmate's first non-classification transfer; non-temporary excludes transfers marked as temporary or medical; "no IBT candidates" drops inmates of custody level 2 and inmates with a life sentence of custody level 2 or 3.

Table 5: Indicated Outcome on Prison Minor Misconduct Effects

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
μ_j	0.243	0.089	0.291	−0.029	−0.459	0.388	−0.434	0.329
	(0.036)	(0.007)	(0.016)	(0.228)	(0.213)	(0.193)	(0.188)	(0.133)
N	56,971	61,283	61,283	42,094	42,094	42,094	42,094	42,094
R^2	0.311	0.149	0.138	0.125	0.111	0.052	0.086	0.096
Average	7.4	0.40	0.82	42.5	28.9	19.3	18.7	8.3
90–10	0.67	0.24	0.80	−0.08	−1.26	1.06	−1.19	0.90

Notes. Each column shows the result of a regression of the indicated outcomes on minor misconduct effects (per 100). Excess months are total months between minimum sentence and release date; total misconduct is all misconduct after classification; any reentry means entry to a PADOC SCI to serve any amount of time; parole means reentry through a parole violation that is not associated with a new crime; crime means reentry associated with any new conviction, either a convicted parole violator (CPV) or a new sentence imposed by a court; technical means reentry as a technical parole violator (TPV); serious crime means reentry because of a new sentence imposed by a court. All specifications include the design controls specified in Table 2. 90–10 shows the impact of assigning an inmate to a facility in the top vs. the bottom decile of prison misconduct effects. Robust standard errors in parentheses.

Table 6: Policy Simulation of Assignment Rule Partly Based on Misconduct Effects

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
Average Change	−0.31	−0.12	−0.37	0.03	0.61	−0.54	0.58	−0.45
Average Outcome	7.42	0.41	0.82	42.79	29.1	19.40	18.90	8.40
Reduction (%) Amongst Inmates								
Reassigned	−4.2	−28.5	−45.4	0.1	2.1	−2.8	3.1	−5.4
All	−0.8	−5.7	−9.1	0.0	0.4	−0.6	0.6	−1.1

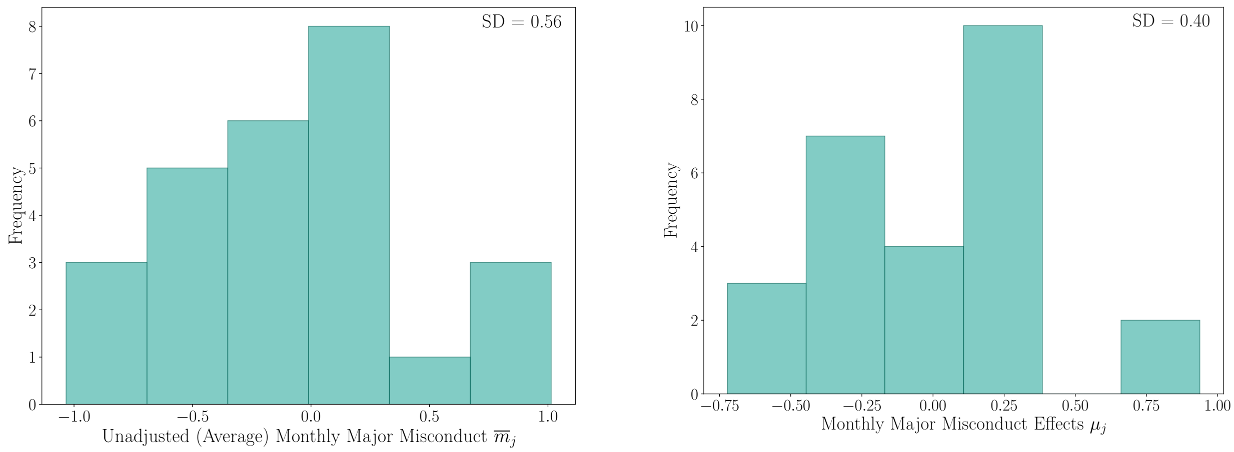
Notes. Each column shows the result of the effect that the policy simulation in Section 4.4 would have on the indicated outcome; see Table 5 for notes on these outcomes. The simulation assumes 400 inmates enter and 400 leave each month; 20% of entering inmates are assigned to the lowest misconduct effect facilities each month until they are full. The reduction is indicated only for those 20% of inmates; the average is indicated for all 400 entering inmates.

Appendix A. Figures and Tables

Appendix Figure A1: Distribution of Major Misconduct Across PADOC SCIs

(a) Average Misconduct

(b) Monthly Misconduct Effects

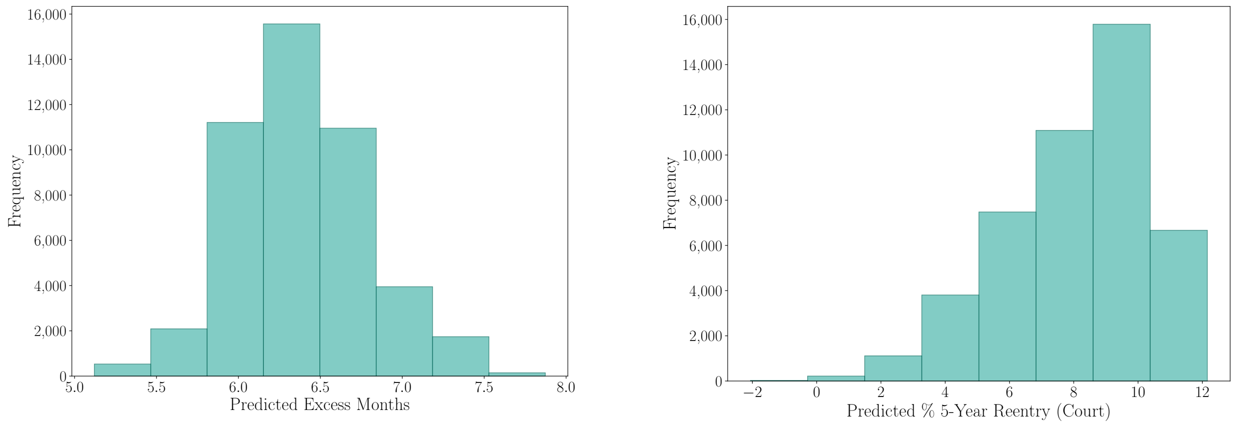


The figures show the distribution of monthly major misconduct (per 100 inmates) estimated for inmates' initial facility assignment, centered around the sample average. The left figure shows the average for each prison $\overline{m}_j$ across inmates in the analysis sample (see Table 1). The right figure shows the estimated misconduct effects μ_j based on Equation (1) using that same sample, shrunk applying an empirical Bayes correction.

Appendix Figure A2: Distribution of Predicted Outcomes

(a) Excess Months

(b) % 5-Year Reentry (Court)



Each figure shows the distribution of the indicated predicted outcome. See Appendix Table A4 for details.

Appendix Figure A3: Event Studies for Samples in Table 4

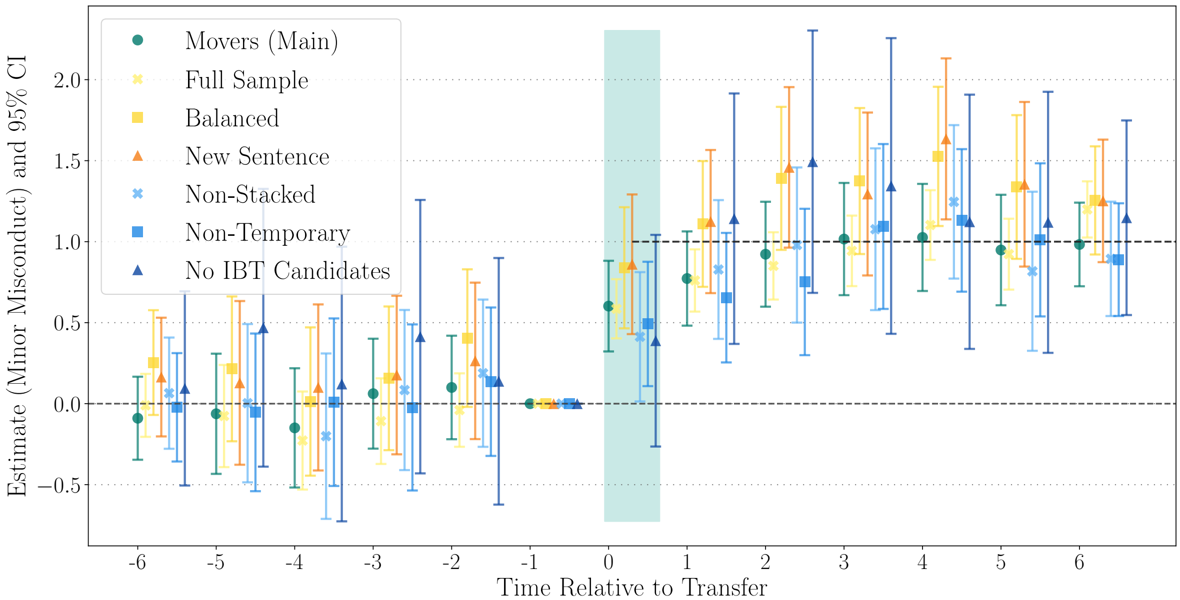
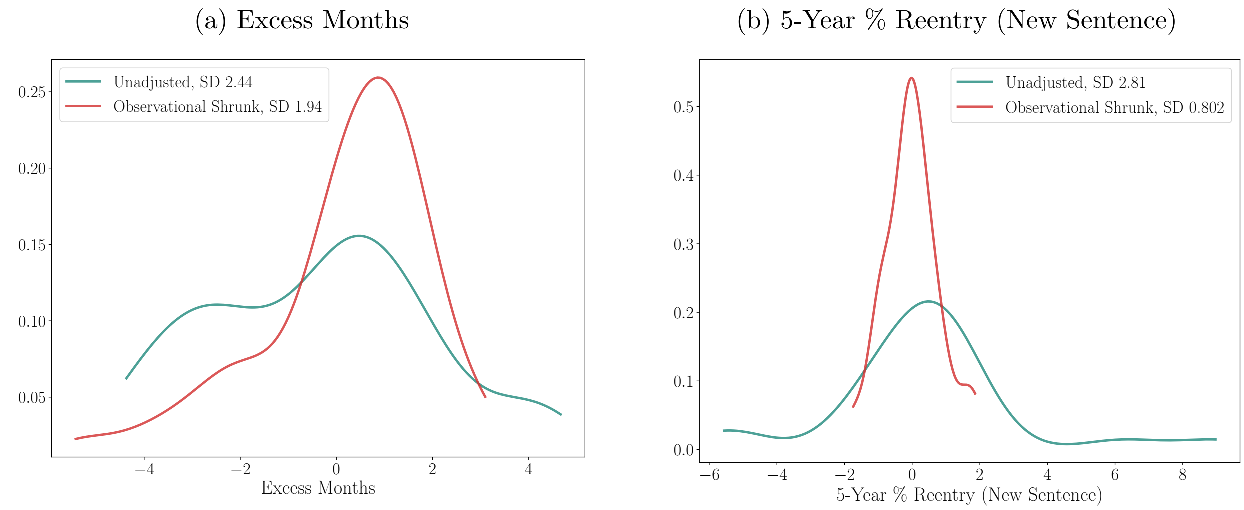


Figure plots several sets of estimated event study coefficients $\widehat{\lambda}_{r(i,t)}$ analogous to Figure 4b (with those same estimates as a reference). Each set corresponds to the estimates of a different sample; see Table 4 for notes on each sample. Standard errors are clustered by inmate-stay i.

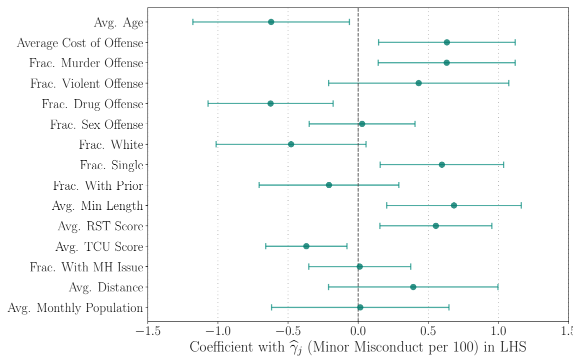
Appendix Figure A4: Distribution of Outcomes Across PADOC SCIs



The figure shows the distribution of average excess time in prison and 5-year reentry from a new sentence for each inmate's initial facility assignment, centered around the sample average. Overlay is the distribution of the corresponding excess time or 5-year reentry observational estimates based on Equation (1). All inmate stays in the analysis sample (see Table 1) are included for the estimation.

Appendix Figure A5: Bivariate Regressions of Raw Monthly Minor Misconduct on Selected Characteristics

(a) Average Inmate (Peer) Characteristics



(b) Facility Characteristics

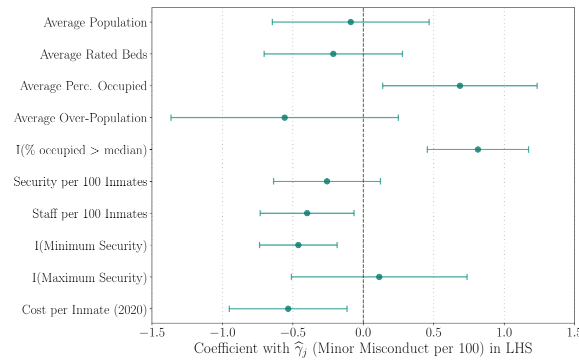


Figure shows weighted bivariate OLS regression results of estimated average monthly minor misconduct on a set of prison-level characteristics. Weights are given by the number of inmate-stay-months in each prison. The left figure show the results for covariates computed using average inmate characteristics; the right figure shows covariates that describe the facility's environment. All independent covariates are standardized to have mean zero and standard deviation one. Horizontal bars show 95% heteroskedasticity-robust confidence intervals.

Appendix Figure A6: Prison Misconduct Effects μ_j vs Average Capacity %

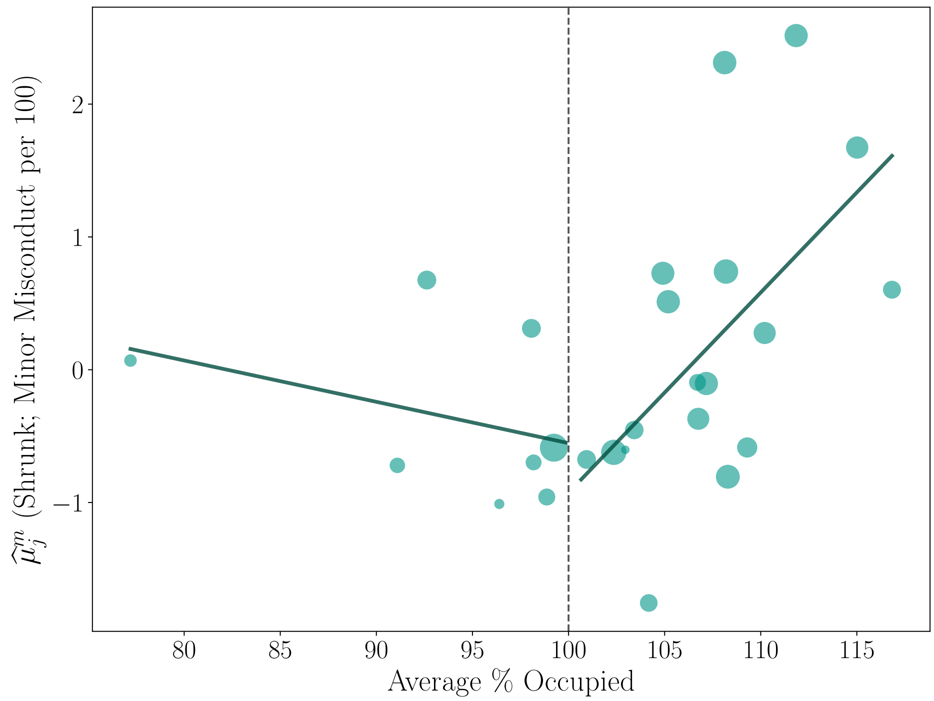
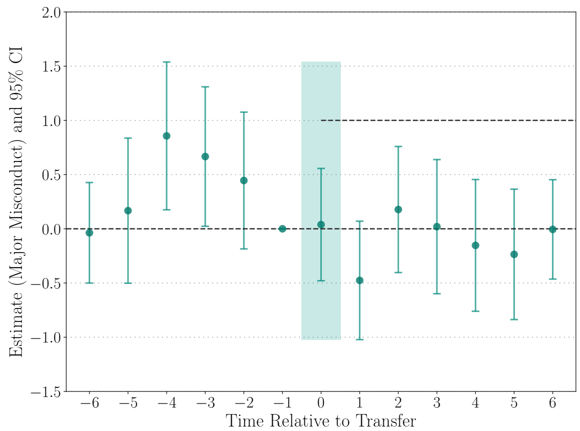


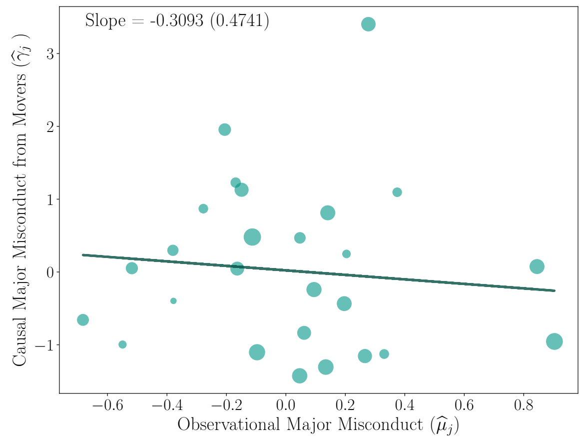
Figure shows a scatter of the estimated prison minor misconduct effects on average capacity as a % of rated bed space across PADOC SCIs.

Appendix Figure A7: Forecast and Event Study Estimates for Major Misconduct

(a) Forecast Event Study



(b) "Movers" $\widehat{\gamma}$ and Observational $\widehat{\mu}$



Left figure plots the estimated coefficients and corresponding 95% confidence intervals from an event study based on Equation (6). The dependent variable m_{it} is total major misconduct in the inmate's end-of-month location $j(i,t)$; this means that period 0 the month of the transfer is only a partial treatment month. For each period I estimate $\widehat{\lambda}_{r(i,t)}$ with $r(i,t)$ the number of months at t relative to i's transfer during their stay. $\widehat{\lambda}_{-1}$ is normalized to 0; $\widehat{\lambda}_{-6}$ and $\widehat{\lambda}_{6}$ also group periods before or after 6 months relative to the transfer, respectively. For each inmate-stay, Δ_i is the difference in destination and origin observational misconduct, $\mu_{d(i)} - \mu_{o(i)}$. Standard errors are clustered by inmate-stay i. The right figure shows a scatter of the estimated $\widehat{\gamma}_j$ from Equation (3) vs the observational estimates $\widehat{\mu}_j$. The size of each dot is weighted by the size of the corresponding facility; a linear fit based on a weighted bivariate regression is overlayed. In both the left and right figures, the observational estimates $\widehat{\mu}_j$ are shrunk by applying an empirical Bayes correction.

Appendix Table A1: Most Common Incidents of Major and Minor Misconduct

	Major	
Description	*N*	%
Using abusive, obscene, or inappropriate language to or about an employee	24,903	27.8
Threaten an employee or their family with bodily harm	11,637	13.0
Fighting	11,083	12.4
Possession or use of dangerous or controlled substance	9,903	11.0
Assault	9,229	10.3
Other (27 groups)	22,984	25.6
	Minor	
Description	*N*	%
Refusing to obey an order	66,888	43.2
Presence in an unauthorized area	23,043	14.9
Possession of contraband (...)	20,154	13.0
Destroying, altering, tampering with, or damaging property	9,753	6.3
Lying to an employee	7,008	4.5
Other (14 groups)	28,096	18.1

Notes. The table shows the 5 most common major and minor misconducts from 2010–2020 among all inmates who started a new sentence (see Table 1) during that time period in one of PADOC's 26 male-only SCIs.

Appendix Table A2: Balance of Demographics vs Prison Major Misconduct Effects

	% White	% Black	% Other	% Single	% Married	% Other	Age
μ_j^{Major}	−0.764	1.476	−0.713	−0.371	0.188	0.182	0.023
	(0.454)	(0.467)	(0.302)	(0.455)	(0.388)	(0.339)	(0.097)
N	61,283	61,283	61,283	60,885	60,885	60,885	61,283
R^2	0.392	0.327	0.134	0.184	0.079	0.116	0.408
Average	50.6	40.1	9.3	75.2	14.1	10.7	34.4

Notes. Each column shows the result of a regression of the indicated demographic on major misconduct effects (per 100). All specifications include All specifications include the design controls specified in Table 2. Robust standard errors in parentheses.

Appendix Table A3: Balance of Predicted Outcome vs Major Misconduct Effects

	Excess Months	Misconduct (Total) Major	Minor	5-year Reentry (%) Any	Parole	Crime	Technical	Serious Crime
μ_j^{Major}	0.001	0.003	0.008	0.031	-0.026	0.052	-0.024	0.006
	(0.007)	(0.002)	(0.004)	(0.087)	(0.055)	(0.053)	(0.036)	(0.023)
N	60,885	60,885	60,885	60,885	60,885	60,885	60,885	60,885
R^2	0.319	0.411	0.415	0.379	0.372	0.378	0.360	0.342
Average	7.4	0.40	0.81	42.1	28.7	19.1	18.6	8.2

Notes. Each column shows the result of a regression of the indicated predicted outcomes on major misconduct effects (per 100). See Table 3 for notes on each predicted outcome. All specifications include the design controls specified in Table 2. Robust standard errors in parentheses.

Appendix Table A4: Indicated Outcome on Demographics

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
I(Race, White)	0.366	−0.195	−0.476	9.944	5.543	6.502	3.974	4.512
	(0.092)	(0.014)	(0.027)	(0.802)	(0.743)	(0.645)	(0.642)	(0.453)
I(Race, Black)	0.000	0.000	0.000	8.158	1.866	7.245	0.868	2.990
				(0.816)	(0.756)	(0.656)	(0.653)	(0.460)
I(Race, Other)	−0.472	−0.309	−0.664	0.000	0.000	0.000	0.000	0.000
	(0.151)	(0.023)	(0.046)					
I(Marital, Single)	0.773	0.018	−0.027	3.077	2.258	0.586	1.548	0.389
	(0.129)	(0.022)	(0.044)	(0.714)	(0.661)	(0.574)	(0.571)	(0.403)
I(Marital, Married)	0.000	−0.030	−0.079	0.000	0.000	0.000	0.000	0.000
		(0.026)	(0.051)					
I(Marital, Other)	1.302	0.000	0.000	5.382	2.977	2.203	2.468	0.442
	(0.178)			(0.991)	(0.918)	(0.797)	(0.793)	(0.559)
Age on Admission	0.048	−0.015	−0.035	−0.846	−0.522	−0.513	−0.333	−0.200
	(0.004)	(0.001)	(0.001)	(0.024)	(0.022)	(0.019)	(0.019)	(0.014)
N	56,628	60,903	60,903	41,875	41,875	41,875	41,875	41,875
R^2	0.005	0.018	0.024	0.037	0.017	0.022	0.010	0.008
Average	7.4	0.40	0.82	42.5	28.9	19.3	18.7	8.3

Notes. Each column shows the result of a regression of the indicated outcomes on inmate demographics. Excess months are total months between minimum sentence and release date; total misconduct is misconduct for entire stay; any reentry means coming back to a PADOC SCI to serve any amount of time; new sentence reentry means coming back specifically by being sentenced of a new crime; any additional sentence means serving any additional time for a new sentence (i.e. not only parole time). Robust standard errors in parentheses.

Appendix Table A5: Outcomes vs Alternative Measures of Prison Misconduct Effects

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
Panel A. Major Misconduct Effects								
μ_j^{Major}	1.257	0.304	0.722	0.347	−0.912	1.209	−0.594	1.340
	(0.100)	(0.020)	(0.044)	(0.649)	(0.604)	(0.549)	(0.532)	(0.378)
90–10	1.05	0.25	0.60	0.29	−0.76	1.01	−0.50	1.12
Panel B. Unadjusted Minor Misconduct Averages								
$\mu_j^{\text{Unadjusted}}$	0.148	0.073	0.228	0.213	−0.195	0.362	−0.178	0.344
	(0.032)	(0.006)	(0.013)	(0.203)	(0.188)	(0.171)	(0.164)	(0.116)
90–10	0.52	0.26	0.80	0.75	−0.69	1.27	−0.62	1.21
Panel C. Unadjusted Minor Misconduct Averages without Design Controls								
$\mu_j^{\text{Unadjusted}}$	0.431	0.159	0.395	2.032	1.013	1.372	0.750	0.764
	(0.036)	(0.007)	(0.015)	(0.199)	(0.184)	(0.163)	(0.159)	(0.113)
90–10	1.52	0.56	1.39	7.14	3.56	4.82	2.64	2.69
Panel D. Unshrunk Minor Misconduct Effects								
μ_j^{Unshrunk}	0.239	0.087	0.285	−0.025	−0.442	0.376	−0.419	0.317
	(0.035)	(0.007)	(0.015)	(0.222)	(0.207)	(0.188)	(0.183)	(0.130)
90–10	0.65	0.24	0.77	−0.07	−1.20	1.02	−1.14	0.86
N	56,989	61,302	61,302	42,107	42,107	42,107	42,107	42,107
Average	7.4	0.40	0.82	42.5	28.9	19.3	18.7	8.3

Notes. See notes in Table 5. Robust standard errors in parentheses.

Appendix Table A6: Indicated Outcome on Minor Misconduct Effects for Selected Samples

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
				Panel A. Excludes Aged 18–20 and Race 'Other"				
μ_j	0.274	0.090	0.288	0.007	−0.480	0.475	−0.394	0.421
	(0.039)	(0.008)	(0.016)	(0.246)	(0.229)	(0.209)	(0.202)	(0.145)
N	49,627	53,809	53,809	36,319	36,319	36,319	36,319	36,319
				Panel B. First-time admissions				
μ_j	0.251	0.098	0.322	−0.016	−0.314	0.276	−0.362	0.293
	(0.040)	(0.009)	(0.019)	(0.260)	(0.241)	(0.219)	(0.212)	(0.150)
N	44,141	47,631	47,631	32,313	32,313	32,313	32,313	32,313
				Panel C. No past misconduct				
μ_j	0.240	0.076	0.251	0.001	−0.499	0.431	−0.484	0.320
	(0.036)	(0.006)	(0.014)	(0.233)	(0.218)	(0.197)	(0.193)	(0.136)
N	54,328	58,353	58,353	40,204	40,204	40,204	40,204	40,204
				Panel D. Censor Outcomes on March, 2020				
μ_j	0.236	0.090	0.295	0.089	−0.687	0.559	−0.405	0.462
	(0.034)	(0.007)	(0.016)	(0.318)	(0.296)	(0.273)	(0.263)	(0.201)
N	48,292	60,095	60,095	20,726	20,726	20,726	20,726	20,726

Notes. See variable notes in Table 5. Robust standard errors in parentheses.

Appendix Table A7: Indicated Outcome on Minor Misconduct Effects Excluding Selected Facilities

		Misconduct (Total)		5-year Reentry (%)				
	Excess Months	Major	Minor	Any	Parole	Crime	Technical	Serious Crime
Panel A. Excludes Facilities Prioritized for Inmates with Mental or Physical Disabilities								
μ_j	0.332	0.099	0.313	−0.168	−0.562	0.234	−0.638	0.392
	(0.046)	(0.010)	(0.022)	(0.288)	(0.269)	(0.244)	(0.236)	(0.167)
Panel B. Excludes Facilities for Deaf and Hard of Hearing Inmates								
μ_j	0.309	0.102	0.309	−0.162	−0.513	0.291	−0.434	0.387
	(0.038)	(0.008)	(0.017)	(0.239)	(0.223)	(0.203)	(0.197)	(0.141)
Panel C. Excludes Facilities for Vision Impaired Inmates								
μ_j	0.342	0.099	0.304	−0.184	−0.580	0.301	−0.479	0.383
	(0.037)	(0.008)	(0.017)	(0.238)	(0.222)	(0.202)	(0.196)	(0.140)

Notes. See variable notes in Table 5. Robust standard errors in parentheses.

Appendix Table A8: Indicated Outcomes vs Minor Misconduct Effects under Alternative Designs

	Excess Months	Misconduct (Total) Major	Minor	5-year Reentry (%) Any	Parole	Crime	Technical	Serious Crime
Panel A. Age and Race as Design Controls								
μ_j	0.245	0.087	0.290	−0.071	−0.462	0.338	−0.457	0.313
	(0.037)	(0.007)	(0.016)	(0.234)	(0.219)	(0.198)	(0.193)	(0.137)
90–10	0.76	0.27	0.90	−0.22	−1.43	1.05	−1.42	0.97
Panel B. Prison Security Level as a Design Control								
μ_j	0.242	0.088	0.290	−0.006	−0.396	0.351	−0.385	0.301
	(0.036)	(0.007)	(0.015)	(0.229)	(0.214)	(0.194)	(0.188)	(0.134)
90–10	0.83	0.30	1.00	−0.02	−1.37	1.21	−1.33	1.04
Panel C. Pre-IADSS Prison Entries Only								
μ_j	0.219	0.086	0.281	0.029	−0.429	0.408	−0.457	0.390
	(0.038)	(0.008)	(0.017)	(0.236)	(0.221)	(0.200)	(0.194)	(0.137)
90–10	0.55	0.22	0.71	0.07	−1.08	1.03	−1.15	0.99
N	56,384	60,514	60,514	42,094	42,094	42,094	42,094	42,094
Average	7.4	0.40	0.82	42.5	28.9	19.3	18.7	8.3

Notes. See notes in Table 5. Robust standard errors in parentheses.

Appendix Table A9: Facility Characteristics Across Deciles of Monthly Minor Misconduct

	Unadjusted Misconduct		Observational Misconduct	
	Bottom	Top	Middle	Top
Age	36.1	33.0	34.2	33.0
% Murder	5.0	23.8	5.8	23.8
% Violent	24.9	26.5	27.3	26.5
% Drug	25.7	12.9	21.8	12.9
% Rape or SA	8.4	14.7	6.8	14.7
% White	54.8	46.5	48.3	46.5
% Single	69.4	77.4	76.8	77.4
% With Prior	22.5	22.5	25.3	22.5
Minimum Sentence	42.6	123.1	42.2	123.1
Distance from County	133.8	197.0	86.4	197.0
Percent Occupied	98.9	113.5	106.4	113.5
Population (1000s)	2.1	2.3	1.8	2.3
Security per 100	23.9	24.4	23.0	24.4

Notes. The table computes, among the facilities in the first and tenth deciles of prsison misconduct effects, the average of the indicated characteristic. Age, % Drug (inmates with a drug-related offenses), % Rape or SA (inmates with a rape or sexual assault offense), % white, % single, % with prior, minimum LoS (length of stay), and distance from (committing) county are all computed among inmates in the analysis sample (see Table 1). Percent occupied and population are from monthly population reports and are the 2019 census of state correctional facilities (United States Bureau Of Justice Statistics, 2022); all three are weighted by the number of inmates in the analysis sample whose initial assignment was at that facility or facility-month.

Appendix B. Forecast Event Study

Assuming Equation (5) holds, Equation (3) can be re-written as

$$m_{it} = \alpha_i + \tau_t + \rho_{r(i,t)} + \gamma_{j(i,t)} + \varepsilon_{it} \tag{10}$$

where $E[\varepsilon_{it}|i,j,t] = 0$. Recall $d(i), o(i)$ are the destination and origin facilities for individual-stay i and re-write Equation (10) as

$$\begin{aligned}
m_{it} &= \alpha_i + \gamma_{o(i)} + \tau_t + \rho_{r(i,t)} + 1(r(i,t) \geq 0) \times (\gamma_{d(i)} - \gamma_{o(i)}) + \varepsilon_{it} \\
&= \widetilde{\alpha}_i + \tau_t + \rho_{r(i,t)} + 1(r(i,t) \geq 0) \times \frac{\gamma_{d(i)} - \gamma_{o(i)}}{\mu_{d(i)} - \mu_{o(i)}} (\mu_{d(i)} - \mu_{o(i)}) + \varepsilon_{it} \\
&= \widetilde{\alpha}_i + \tau_t + \rho_{r(i,t)} + 1(r(i,t) \geq 0) \times \lambda_{\text{DiD}} \times \Delta_i + \varepsilon_{it}
\end{aligned}$$

where $\Delta_i = \mu_{d(i)} - \mu_{o(i)}$ and $\widetilde{\alpha}_i \equiv \alpha_i + \gamma_{o(i)}$. Note λ_{DiD} is equivalent to λ in the differenced regression

$$(\gamma_j - \gamma_k) = \lambda(\mu_j - \mu_k) + (\eta_j - \eta_k) \tag{11}$$

with weights π_{jk} equal to the share of inmate months transferred to j from k. Finally, for the event study in Equation (7), in practice I estimate

$$\lambda_{r(i,t)} = \sum_{s=-T}^{T} \lambda_s \cdot 1(r(i,t) = s) + \lambda_{-T-1} 1(r(i,t) < -T) + \lambda_{T+1} 1(r(i,t) > T) \tag{12}$$

with $T = 5$ to estimate a 6-month window around the transfer.