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IMPROVE INSTITUTIONAL SAFETY DURING INCARCERATION

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Mary Kate Batistich, William N. Evans, Tyler Giles, and Rebecca Margolit-Chan
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ABSTRACT

We evaluate the impact of Step Up, a cognitive behavioral therapy program administered to inmates at the Lubbock County Detention Center in Lubbock, Texas. Step Up aims to address self-destructive thought and behavior patterns through a combination of group classes, one-on-one counseling, and a structured workbook. We compare individuals over time who enter the Step Up program to a group of eligible and interested nonparticipants in an unbalanced two-way fixed effects framework. Despite similarities in observable characteristics and initial behavior metrics, individuals who enter Step Up exhibit a 49% reduction in monthly behavioral incident rates compared to the untreated group, which is about an 8-percentage point decline. The typical participant spends about 3 months in the program, and we find evidence that the behavioral improvements persist after program completion.

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1 Introduction

In 2022, over 7 million people were admitted to jail, and about 660,000 people were detained in jail on any given day (Zeng, 2023). The typical jail stay is short with 57 percent of stays being one week or less; however, a large and growing number of people spend a significant amount of time in jail, often awaiting trial.¹ Estimates suggest there are currently 448,000 pretrial inmates in local jails, and this number has doubled since the mid 1990s (Sawyer and Wagner, 2024). As a result, 23 percent of jail stays—served by 1.7 million people—last a month or more, about 10 percent last three months or more, and 4.3 percent last a half year or more.² Along with the steady increase in holding people who have yet to be convicted, the average length of stay in jail has also increased considerably, from 22 days in 2005 to 33 days in 2021 (Zeng and Minton, 2021b; Zeng, 2023). Long jail stays have become even more common as local courts deal with case backlogs generated during the COVID-19 pandemic (Diaz, 2023).

The growth in extended jail stays presents a problem for both detained individuals and the detention facility. People in jail are at high risk for mental and behavioral health challenges. About one-quarter of people in jails have a serious mental illness, which is five times the rate in the general population (Bronson and Berzofsky, 2017), and about 63 percent of sentenced jail inmates struggle with drug dependence or abuse (Bronson et al., 2017). These issues can lead to severe negative outcomes for inmates; for example, 1,200 individuals died while in jail in 2019, with suicide and drug overdose as two leading causes (Carson, 2022). In addition, those spending longer periods in jail often do so because they are awaiting trial for a serious offense like an assault and thus have a propensity towards violence.³ These mental and behavioral issues among the detained then spill over to correctional officers and are often cited as one of the primary drivers of the current jail and prison staffing shortage (Heffernan and Li, 2024).

Longer jail stays, the large number of people with these stays, and the high-risk nature of the inmate population also present an opportunity for jails to incorporate more time-intensive,

¹See data collected by the Jail Data Initiative at <https://jaildatainitiative.org/>

²See data collected by the Jail Data Initiative at <https://jaildatainitiative.org/>

³In our data, 45% of jail stays lasting at least 90 days are tied to a violent charge.

longer-term programming to improve life skills. Such programming is relatively common in prison but scarcer in jails; for example, roughly 81 percent of prisons have drug and alcohol dependency counseling (Maruschak et al., 2021), but the comparable number for jails is only 17 percent (Zeng and Minton, 2021a). On the one hand, jail programming has the tantalizing potential to improve current and future well-being among the detained and their detainers. On the other hand, such programming is non-trivial to implement and may generate unintended outcomes; e.g., programming that affords inmates more latitude within a facility may increase opportunities and cues that lead to costly misconduct.

In this paper, we evaluate the impact of *Step Up*, a cognitive behavioral therapy (CBT) program targeted toward individuals with violent backgrounds in the Lubbock County Detention Center (LCDC) in Lubbock, Texas. Over a self-paced 14-to-21 week curriculum, participants learn how to identify their emotions and recognize the physical sensations that correlate with that emotion. The goal is that after learning to recognize their emotions and change their thinking, participants will transform from making negative behavioral choices to adopting rational and constructive behaviors. The underlying philosophy of the program is that although individuals experience negative events (e.g., an unpleasant confrontation with another person), it is one’s interpretation of the event, rather than the event itself, that leads to negative reactions such as anger and violence.

We investigate whether this program improves behavior and well-being in the detention center. Using a sample of eligible medium and maximum security inmates who applied to *Step Up*, we use an unbalanced two-way fixed effects (TWFE) approach to identify the impact of *Step Up* on behavioral incidents over time as participants enter the program. We find that the program is largely successful, decreasing the likelihood that participants experience a behavioral incident within a given 30-day period by 8.2 percentage points, a 49% drop. This decline is largely driven by disciplinary infractions, such as being disrespectful to officers or another inmate, which make up the bulk of behavioral incidents. We also see a statistically significant 1.8 percentage point decrease in physical assaults on other inmates, a 50% decrease from the untreated mean. We supplement this analysis with a difference-in-differences approach using a balanced, propensity score matched

sample. In this analysis we find even stronger impacts over baseline, but compromise on statistical power. Heterogeneity analysis with our main approach shows that individuals with current violent charges and the youngest age group (ages 18-30) have the strongest response to the program.

It is natural to speculate whether our results are driven not by changes in inmate cognition or emotional processing, but by changes in residential or peer environments. For context, LCDC inmates reside in self-contained group areas, or “pods,” and *Step Up* is administered in its own pod. The environmental change that comes with entrance into *Step Up* could bias our findings in either direction, but evidence suggests it is likely to *increase* misconduct. For example, the *Step Up* pod contains a high concentration of individuals with violent backgrounds, *Step Up* participants have more freedoms and opportunities to commit misconduct, and the behavioral expectations in the *Step Up* pod are greater than in other ones so that less severe infractions may now be recorded.⁴ Conversely, inmates interested in *Step Up* may be positively selected, and thus entrance into *Step Up* could mean an improvement in the peer environment, which could reduce misconduct even in the absence of programming-induced changes.

We find reductions in misconduct despite the environmental changes, suggesting that our estimates, if anything, may understate the true effects of the program on inmate cognition and behavior. We also separate the treatment effect into short-term (the first three months after program enrollment) and long-term (after three months) where many will have returned to the main pods. We find that the impact of the program persists into these later months, suggesting the therapy is effective beyond simply the residential change.

Our work contributes to a growing literature on the use of CBT among the criminally-involved. Several randomized control trials (RCTs) have examined its impact on criminal justice-involved populations outside of incarceration. Barnes et al. (2017) and Van Voorhis et al. (2004) found suggestive but at times imprecise benefits of CBT for probationers and parolees, and the READI Chicago program, which combined CBT and employment for men at high risk of gun violence,

⁴Dishion and Andrews (1995) and Poulin et al. (2001) show that therapeutic programs which group at-risk participants together can increase rates of risky behavior; Campbell and Lee (2024) show that participation in educational programming in prison can increase inmate misconduct rates, likely because of increased opportunities. We discuss the increased behavioral expectations in the *Step Up* pod in Section 2.

reduced involvement in violence along some but not all dimensions (Bhatt et al., 2023). Outside of the US context, CBT was shown to reduce criminal and violent behavior among Liberian men (Blattman et al., 2017, 2023). There is less work on the effects of CBT for the incarcerated. An RCT evaluation of “Becoming a Man,” a CBT program within a Chicago juvenile detention center, found substantial reductions in re-admission rates to the facility (Heller et al., 2017). In addition, Arbour (2023) used a quasi-experimental design to identify the effects of prison-based CBT in Canada and found large and statistically significant reductions in six-month recidivism.

However, not all programs are shown to be effective and more could be done to understand the mechanisms that contribute to the positive changes suggested by previous studies (Barnes et al., 2017; Heller et al., 2017; Hill et al., 2011). In particular, there is a need for rigorous evidence about (1) interventions that can reduce violence among those with violent backgrounds or specific behavioral patterns, and (2) the specific practices that can ensure fidelity of implementation if such interventions are to be replicated elsewhere.

This paper contributes to the existing body of knowledge on CBT in several ways. To our knowledge, it is the first evaluation of a behavioral therapy program focused on individuals with violent backgrounds in an adult US jail. The program’s focus on reducing violence is notable. Typically, such programs are less likely to be offered to those with violent backgrounds (Polaschek, 2019; Polaschek and Collie, 2004), yet this population potentially stands to gain much from it. *Step Up* participants and those serving extended jail stays are overwhelmingly pretrial individuals charged with serious offenses, and many will go to prison. For these individuals, violence is often a fact of life before, during, and after incarceration. About half of individuals in state prisons were sentenced for violent offenses (Carson and Anderson, 2014). While in prison, prisoners are frequently subject to violence; within a 6-month window, one-fifth of state prisoners are subject to a physical assault (Wolff and Shi, 2010). After release, many prisoners are rearrested for violent offenses. In one study, 21 percent of released inmates were rearrested within three years for a violent offense (Alper et al., 2018).⁵

⁵There is detailed information about violence in prison and the violent background of prison inmates, and less is known about jails. For certain violent acts, rates appear very similar in the two settings. For example, Buehler

The *Step Up* program is a promising alternative to existing responses to behavioral infractions, such as solitary confinement, where it is estimated that about 4.4% of the incarcerated population are held at any point in time (Matei, 2022). Research shows that solitary confinement may be psychologically damaging (Reiter et al., 2020), does not improve prison safety (Labrecque and Smith, 2019; Lucas and Jones, 2019), and is linked to higher recidivism rates (Luigi et al., 2022). As reforms across the country are beginning to take aim at limiting and mitigating such practices (Resnik et al., 2016), CBT programs like *Step Up* may prove to be a useful and more effective option.

This paper also adds to our understanding of the benefits of jail programming. Such programming is much less common than prison programming, and hence there is much less work on it. Using rigorous quasi-experimental methods, Alsan et al. (2024) found that IGNITE, an education-based program, reduced misconduct and recidivism among soon-to-be released inmates. Our paper validates their broad finding that jail programming can be feasible and effective. We also complement this work by showing that jail programming can be beneficial for a different subset of the jail population; we study a different program in a high-security county jail in which inmates often stay for several months before serving time in prison. While extended pretrial detention is often viewed as a transitional time period and sometimes overlooked for therapy and other programming, it presents an opportunity to make a significant impact, as many individuals will be detained for several months or longer before facing extended incarceration. Our approach also builds on prior research by focusing on the immediate and short-term outcomes on violent behavior and well-being during incarceration.⁶

The rest of this paper proceeds as follows. In Section 2, we provide an overview of the *Step Up* program. In Section 3, we describe our data source and treatment. In Section 4, we describe our empirical strategy. In Section 5, we describe our results and report on key robustness checks. In

(2021) reports that incidents of sexual assault rates (per 1,000 inmates) for prisons and jails in 2018 were 13.5 and 11.9 respectively. The suicide and murder rates in state prisons (per 100,000 inmates) in 2018 were 26 and 10, respectively (Carson, 2021b), while the same numbers in jails were 46 and 4 (Carson, 2021a).

⁶We lack complete information on inmates’ destinations upon exiting the LCDC, but *Step Up* staff estimate that at least half of program participants immediately begin serving prison sentences after their LCDC stay ends. This precludes an analysis of the short-term re-arrest/recidivism effects of *Step Up*.

Section 6, we describe our alternative approach using a balanced, propensity-score matched sample, and describe our results. In Section 7, we discuss implications of our results and conclude.

2 Environment and Program Details

2.1 The Lubbock County Detention Center

The CBT program we study is implemented in the Lubbock County Detention Center (LCDC), one of the largest detention facilities in Texas, supporting Lubbock County and surrounding counties in western Texas with about 1,530 beds and an average daily population around 1,400. The LCDC houses pretrial and pre-sentence individuals, as well as those serving shorter sentences of up to one year. About three-fourths of inmates are pretrial detainees with felony charges.

The LCDC building is comprised of six “clusters” (one for women, five for men), and each cluster contains four to five separate residential areas, or “pods.”⁷ Each pod holds somewhere between 44 and 72 inmates at a time. The facility is constructed for direct supervision where officers observe all cells within a pod from a single station and vantage point that is manned 24 hours per day. This design discourages movement in the hallways outside the pods, and inmates do not typically interact with other pods unless reassigned. Meals and other services such as medication disbursement take place within the pods.

Most pods in the LCDC are designated for general population, but some are specialized for purposes such as solitary confinement or to house inmates with intellectual and developmental disabilities. Correctional officers work within pods on a 60-day rotation. Officers can give bed restrictions of up to 23 hours which are not documented and hence cannot be viewed in the data. More serious offenses including assaults and other behavioral incidents are given a written disciplinary infraction which must then be approved by a supervisor.

⁷The cluster and pod structure can be seen in an aerial view of the LCDC, shown in Appendix Figure B.1.

2.2 The *Step Up* Program

The *Step Up* program launched in the LCDC in 2016 and has had over 1,000 participants to date. *Step Up* is administered in a single pod (Pod 5A); hence, “participating in *Step Up*” means being transferred to reside in Pod 5A. As part of their regular routine, individuals in the *Step Up* pod participate in about 15 hours of group classes per week, led by an LCDC program director and/or graduate student intern. Additional programming includes assigned homework in self-paced workbooks and an optional one hour per week of one-on-one counseling. The workbook takes about 14 to 21 weeks to complete and overlaps with topics discussed in the group classes (communication skills, thinking patterns, problem solving skills, short- versus long-term thinking, etc.).

The overall curriculum is based on cognitive behavioral therapy (CBT), an action-oriented approach to manage cognitive, emotional and behavioral disturbances in life. The underlying philosophy of the program is that although individuals experience negative events (e.g., an unpleasant confrontation with another person), it is one’s interpretation of the event, rather than the event itself, that leads to negative reactions such as anger and violence. This is itself one of the core tenets in social psychology – that people regularly misconstrue the situations in which they find themselves (Ross and Nisbett, 2011). With better coping tools, individuals could be more likely in the future to put a stop to violence, sustain employment, and avoid encounters with law enforcement.

The program teaches individuals how to manage their emotional responses and specifically teaches the skill of not resorting to violence as a means of resolving conflict. As an example, suppose *A* has a heated exchange with *B*. *A* can retaliate with violence but that violence may extend his sentence and delay his reunion with his family; i.e., a violent response will only hurt *A*. Participants view this program as helping them reverse their criminogenic thinking as well as their substance abuse tendencies. As part of the therapy, they are asked to revisit their pasts, which often include unsafe and impoverished childhood environments, to better understand the factors leading them to their current situation. Participants often share personal motivations for success related to improving circumstances for their children and breaking their own intergenerational cycle of crime, violence, and substance abuse.

Outside of the CBT-based programming, the environment within the *Step Up* pod is notably different from other pods where participants would likely reside (e.g., higher security pods or solitary confinement pods). The *Step Up* pod contains detached, movable furniture; decorations and artwork from inmates; and easier access to TV. However, there are additional expectations and behavioral standards placed on those in the *Step Up* pod. These include waking at a fixed, early time in the morning, keeping their cell and common area clean, respecting each other and staff, not cursing, and completing homework assignments on time. Incentives such as movies or popcorn are periodically awarded for good behavior.

2.3 Selection for *Step Up*

Step Up participants primarily come from the pretrial population of the LCDC. The program was designed for those with a violent history or disciplinary issues, and therefore a large share of participants spent time in solitary confinement prior to joining the *Step Up* pod. Many of them also struggle with longtime substance use disorders. The LCDC focuses on the felony pretrial population because they can be in jail for one to two years and as a result have more time to participate in the program, and also because they often have a violent history. From the LCDC’s perspective, this is the group with a potentially large return from a successful program.⁸

Inmates can either self-refer to the program or be referred by a correctional officer. According to open-ended survey responses, interest in the program comes mainly from a desire to become a better person for themselves and others. The LCDC’s security classification is the main eligibility criterion. Classification is determined on entrance to the detention center and is reassessed at least every 60 days during incarceration. It is based on a number of factors including current offense type (assaultive felony, felony, or misdemeanor), any past felony or assaultive felony convictions, known history of escape, and known serious behavior problems. Re-classifications are based on behavior during incarceration. Classification levels range from 1 to 9, with 1 being the highest security. For

⁸Notably, the targeting of this population puts a downward bias on simple correlational estimates of *Step Up*’s benefits. This targeting also contrasts with possible incentives to “cream skim” well-behaved participants for *Step Up* to boost the perceived effectiveness of the program.

details on how classification is determined, see Appendix Figures B.2 and B.3. To be eligible for *Step Up*, an inmate must have a classification between 2 and 5. Those with a classification of 1 can still join *Step Up* if approved by the Director of Programs.⁹ Classification 1 to 2 is considered maximum security, and 3 to 5 is medium security.

During the time period we consider in this analysis, once referred to the program, inmates were entered onto a waitlist that existed due to the program’s limited capacity and high demand. A typical waiting time was about four to six weeks. When a bed became available, the person who was on the waitlist the longest was asked to join, except in some unique circumstances, such as bed restrictions. If an inmate had a bed restriction (e.g., they have a condition that required that they have a bottom bunk, and only a top bunk was available), then they were bypassed for the next person on the waiting list. Similarly, if the individual had a “keep separate” order with someone who is already in *Step Up*, they would not be selected at that time. In these cases, the individual would stay at the top of the waitlist and will be reconsidered upon the next opening.

If someone is pending placement in the Special Housing Unit (solitary confinement), they need to be cleared before being admitted to *Step Up*. Cases of those with Behavioral Management Plans (those who have spent a long time in solitary confinement) need to be approved by the Rehabilitation Coordinator before entering *Step Up*. Finally, Gang Intelligence cases under investigation are not allowed in order to prevent interference with the investigation.

Individuals may express interest in and qualify for *Step Up* but ultimately never enter the program for a number of reasons. Many individuals will exit the LCDC before they reach the top of the waitlist or before circumstances like bed restrictions can be resolved. Others may pull themselves off the waitlist voluntarily or become ineligible due to behavioral issues. In Section 4, we further discuss the usage of these individuals as a comparison group for *Step Up* participants and the implicit identifying assumptions.

⁹We also observe a few cases of those entering with a classification of 6.

3 Data

Our data come from LCDC administrative records. We use the set of inmates booked at LCDC between April 2016 and April 2021 who were eligible and applied to be in *Step Up*. Our data extract ends in February 2022 and it includes demographic information, booking and release dates, charges, security classification at entry and any changes to security classification with dates of change, cell assignment by date, dates of any “keep separate” orders, gang association history, disciplinary incidents by date, and activity participation by date (e.g., Bible Study, Life Skills Class, Alcoholics Anonymous Meeting). There are 2,768 bookings in our extract. To create our estimating sample, we first drop bookings for individuals who participated in *Step Up* in a previous jail stay. We then only keep bookings that are 90 days or longer, so we have enough time to view their experience during incarceration. This produces an analysis sample of 817 people. We place inmates into 30-day “blocks” of stay in the LCDC, and convert the data to a booking-by-30-day-block panel.¹⁰

Table 1 compares our LCDC sample to the jail populations nationwide, in Texas, and in Lubbock County overall. Each estimate from outside the LCDC is calculated using the Annual Survey of Jails 2016-2021.¹¹ Lubbock County, similar to the Texas and nationwide samples, is 83% male. Since *Step Up* takes place in the men’s quarters, our estimating sample consists entirely of men. Our sample has a comparable age distribution to the rest of the Lubbock County, Texas, and U.S. jail populations. It is somewhat more concentrated in men ages 35-44 (41% compared to 30% in Lubbock overall and 28% nationwide) and is less concentrated in the youngest and oldest age groups. The racial distribution in our analysis sample is strikingly similar to the overall Lubbock jail population but differs from the Texas and U.S. samples; in particular, our estimating sample is more heavily Hispanic. Last, the average length of stay for the bookings in our data (349 days) is much longer than the other samples. This is both a mechanical result (we drop stays shorter than 90 days) and because the program we study is specifically targeted at individuals with longer stays.

We observe three main behavioral outcomes for inmates: disciplinary infractions, assaults, and

¹⁰One’s final block in the LCDC may be shorter than 30 days and we weight our estimates accordingly.

¹¹There was no 2019 survey, so we substitute with the Census of Jails for 2019.

suicidal behavior. A disciplinary infraction is any reported non-assault incident of an inmate being uncooperative or violating an LCDC rule. Examples include being disrespectful to staff or being caught with contraband. Assaults are typically a physical assault on another inmate but include assaults on staff and do not include victimization. Suicidal behavior includes suicide attempts as well as cases where an inmate informs an officer or staff member that they are having suicidal ideations and an intervention takes place. We observe date of occurrence for all behavioral incidents. Since our regression analysis is done using individual-by-30 day block observations, we measure outcomes using an indicator for whether at least one incident occurred during those 30 days.

Figure 1 shows trends in these behavioral outcomes over time in one’s stay in the LCDC. Disciplinary infractions are the most common form of behavioral incident, especially for those who recently entered the LCDC; for example, about 14% of inmates commit a disciplinary infraction during their first 30 days of their stay. These infractions, along with suicidal behavior, decline in the first few months after LCDC entrance and then level off. Assaults remain fairly constant over the course of one’s stay: About 3 to 4% of all inmates will commit an assault in any given block since entrance.

Table 2 presents sample statistics for our data and compares observable characteristics for those who enter *Step Up* to those who were eligible and applied to be in *Step Up* but did not enter. *Step Up* participants are similar to nonparticipants on several dimensions: They are approximately the same age and have the same likelihood of having a behavioral incident in the first 30 days of booking. However, they differ on other dimensions, including that treated individuals have a higher average charge count and are more likely to have a violent charge. Treated individuals also tend to spend longer in the LCDC than untreated individuals (13.8 blocks vs. 9.6 blocks). This may be partly explained in that some of the untreated individuals would have become treated had they stayed in the LCDC longer. Instead, they exited before being able to enter the program.

Those who join *Step Up* do so at various times throughout their stay. Figure 2 shows the count of observations in our sample over time in both the treated and untreated group. It also shows the count of individuals in the treated group once they have joined *Step Up*. As the sample is restricted

to those who stay at least 90 days, the first 90 days are flat in terms of counts for the treated and untreated groups. After that, the count drops substantially and most have left the LCDC by 18 to 24 months. The trend then levels off and there is a smaller group of individuals who stay in the LCDC for two years or more.

4 Empirical Strategy

Simple comparisons of behavioral outcomes among *Step Up* participants before and after entering *Step Up* are unlikely to capture the causal effect of program. First, there is a general time trend in behavior as people adjust to confinement (see Figure 1). Second, *Step Up* participants might apply for the program in response to a newfound desire for self-improvement, and thus they might have experienced positive outcomes even in the absence of the program. Hence, a more appropriate strategy is to compare the observed outcome trends for *Step Up* participants before and after program entrance with outcome trends among a group with likely similar motivations for improvement but who did not participate in *Step Up*. We formalize this sort of comparison in a difference-in-differences framework and estimate the following regression model:

$$Y_{itc} = \beta_0 + \beta_1 \text{StepUp}_{itc} + \lambda_i + \mu_t + \eta_c + \epsilon_{itc} \quad (1)$$

Y_{itc} is an indicator for whether individual i had at least one behavioral incident during his t^{th} 30-day block in the LCDC.¹² The variable StepUp_{itc} equals 0 in the blocks before program entrance and equals 1 starting in the first block with any amount of time spent in *Step Up* and remains 1 thereafter. This model controls for individual fixed effects (λ_i), 30-day block relative to LCDC entrance fixed effects (μ_t), and calendar month-by-year fixed effects (η_c).¹³ These sets of fixed effects control for time-invariant individual characteristics, general trends in behavior as inmates adjust

¹²To illustrate, Y_{i1} captures individual i 's behavior during his first 30 days in the LCDC, Y_{i2} captures it during his 31st and 60th days in the LCDC, etc. LCDC stays rarely last a number of days perfectly divisible by 30, so an individual's last block will almost always be less than 30-days. We weight our regressions by number of days in a block so as not to over-represent these shorter blocks.

¹³More specifically, η_c is a set of dummy variables indicating the calendar month-by-year of the first day of block t for individual i .

to the LCDC, and any time-based shocks. The treatment effect is captured by β_1 , which can be interpreted as the average effect of *Step Up* entrance on the likelihood of at least one behavioral incident during a 30-day period. We allow for arbitrary correlation in errors for the individuals.

We estimate this model using the sample containing observations for both *Step Up* participants (the treatment group) and those who were eligible and applied for the program but never entered (the comparison group). Goodman-Bacon (2021) notes that estimation of a two-way fixed effects regression like Equation 1 using a sample that includes “never-treated” observations (i.e., our comparison group) produces an estimate identified by comparisons of post-treatment observations to never-treated ones, but also post-treatment observations to pre-treatment ones. In Section 6, we implement a regression strategy that more explicitly only compares program participants to matched comparison group individuals; our results are qualitatively similar, which we discuss in Section 6.6.

Our two-way fixed effects sample is “unbalanced” in two senses: Individuals contribute differing numbers of observations depending on the length of their stay in the LCDC, and individuals contribute observations at different calendar times. This approach has the advantage of leveraging all the data we have, but it has the disadvantage of overrepresenting those who are in LCDC longer and in *Step Up* longer. To avoid overrepresentation of the exceptional cases where individuals are spending a very long time in the facility, we limit observations to the first 18 30-day blocks of stay in the LCDC.¹⁴

The key assumption in a difference-in-differences specification is that the comparison sample provides an estimate of the outcome trends that would occur in the treated sample had there been no intervention. One way to investigate this assumption is to estimate an event-study specification with separate treatment coefficients in each period before and after the entry into *Step Up*. Results consistent with the key assumption would be the pre-treatment coefficients are jointly zero. Our event-study specification is of the form

¹⁴Alternative limits to these time horizons are considered in Appendix Table A.1.

$$Y_{itc} = \sum_{\substack{k \neq -1 \\ k \geq -4}}^{\leq 5} \alpha_k Period_{itc,k} + \lambda_i + \mu_t + \eta_c + \epsilon_{itc} \quad (2)$$

where the subscripts (i , t , and c), fixed effects (λ , μ , and η), and idiosyncratic error (ϵ_{it}) are defined as before. For those that enter *Step Up*, we define the dummy variable $Period_{itc,k}$ that equals 1 for treated people if block t is k blocks from entry into *Step Up*. We set $k = -1$ as the reference period ($\alpha_0 = -1$) which is the 30-day period before a person enters the program. Like in the difference-in-differences specification, we limit each individual's observations to up to 18 30-day blocks in the LCDC. As endpoints of these samples have few observations, we group $k = 5$ and up into one group and $k = -4$ and below as another. In this model we again cluster standard errors at the individual level.

5 Results

5.1 Main Results

We first report simple means over time for the treated group in Figure 3. Lines of best fit show that in the blocks leading up to program entry, disciplinary infractions and assaults occur at higher rates, with a clear drop in trend after program entry. Suicidal behavior remains low and is relatively constant in both the pre- and post-periods.

Table 3 reports our two-way fixed effects estimates from Equation 1. Column (1) shows that *Step Up* entrance reduces the likelihood of a behavioral incident within a 30-day period by 8.2 percentage points (p.p.). This effect is precisely estimated and large in magnitude; compared to a baseline incident likelihood of 0.169 calculated using all pre-treatment and comparison group observations, this constitutes a 49% reduction, or near halving, of the baseline probability. Columns (2) and (3) show that this reduction is driven by effects on both disciplinary infractions (7.0 p.p.) and assaults (1.8 p.p.), and each of the effects also amounts to about halving as compared to the respective baselines. Column (4) suggests that *Step Up* entrance has no discernible effect on reported suicidal

ideation or attempts.

We limit our sample of person-by-30-day-block observations to an individual’s first 18 blocks of stay in the LCDC. Appendix Table A.1 tests whether our findings are sensitive to this sample selection criteria. Here, we re-estimate Equation 1 while varying this restriction to 12 months, 24 months, and no restriction at all. These results show that our findings are robust to these various cutoffs, with very similar magnitudes and significance for each specification. In the unrestricted sample, we still see large and statistically significant effects, though smaller in magnitude. This is consistent with smaller impacts in the very long term at the LCDC.

Note that our main outcome is an indicator for whether an incident (or type of incident) occurred in a given 30-day block. We find this measure easy to interpret, especially since most individuals do not have more than one incident in a given block. Our results using counts of incidents (or type of incident) by block look quite similar to our main estimates. For example, we see a 0.087 decline in incident counts coming from a baseline of 0.203. We report the full set of main results with counts as our outcome measure in Appendix Table A.2.

5.2 Heterogeneity

We next explore heterogeneity of effects by participant characteristics. We report our results in Table 4.¹⁵ Columns (1) and (2) show that those incarcerated on violent charges have somewhat higher baseline likelihoods of behavioral incidents. Panel (a) conveys that these same individuals experience steeper decreases in incidents upon entering *Step Up*, with a decrease of 51% of their baseline, compared to a 44% decrease for those without a violent charge. While these results are only suggestive as we cannot reject the null hypothesis that the coefficients are the same, it suggests that there are high returns to the program for those incarcerated on violent charges.¹⁶ The results suggest that this difference is driven by steeper decline in disciplinary infractions, as seen in panel (b).

We also consider heterogeneity by security classification as assigned at admission to the LCDC.

¹⁵Figures showing these coefficients are in Appendix Figures A.1 through A.4.

¹⁶The z-statistic on a fully interacted regression is -0.934.

Here, we split the sample between those with security classifications at LCDC entrance from 1 to 3 (high security) and those with one of 4 or more (lower security). High security inmates are either currently booked on an assaultive felony charge, have a past assaultive felony conviction, or a past escape history; otherwise, they will have a lower security classification.¹⁷ These results are shown in columns (3) and (4) of Table 4. Interestingly, we see larger effects for lower security inmates, which may initially seem to contrast with the columns (1) and (2) findings of larger effects for those with a violent charge. Table A.3 reconciles these findings and shows no effect for high security inmates without violent charges, a group classified as high security because of a past violent conviction or escape attempt. This suggests that individuals with violent backgrounds can benefit greatly from *Step Up*, but those with serious longstanding violent or behavioral issues may struggle to engage with and benefit from the program.

Our heterogeneity findings may also be connected to age. Columns (5) through (7) of Table 4 split the sample into three age groups: 18-30, 31-40, and 41 and over. Panel (a) reveals that *Step Up* has the strongest impacts on young inmates, with a decrease of 11.8 p.p. (56%).¹⁸ The difference in effects between the 18-30 group and older inmates is statistically significant.¹⁹ The effect on the oldest age group, ages 41 and over, is the smallest but still significant at the 90 percent confidence level. The difference in effects for this group compared to the 18-40 group is also statistically significant.²⁰ Most research on CBT for criminogenic behavior has focused on teenage boys and young men (e.g., Heller et al. (2017); Blattman et al. (2017); Bhatt et al. (2023)). Here we provide evidence that CBT has an effect on behavior for those ages 41 and over, albeit less dramatic than for the younger age groups.

Panel (b) shows that young inmates have the highest baseline likelihood of disciplinary infractions and the strongest reduction in disciplinary infractions upon *Step Up* entrance. Likewise, this age group also has the biggest decline in assaults upon treatment, with a decrease of 58.4%. These

¹⁷See Figures B.2 and B.3 for the security classification forms.

¹⁸Table A.4 shows that the biggest effect are among young inmates with violent charges in particular.

¹⁹The z-statistic on the interaction term in a fully interacted regression is -1.487.

²⁰The z-statistic on the interaction term between post and an indicator for ages 41 and over in a fully interacted regression is 1.980.

effects decrease as the age group increases, with no discernible effect of treatment on assaults for the oldest age group, but we note that this group also had a relatively low baseline mean incidence rate (2%). Across all subgroups, Panel (d) reveals no meaningful effect of the program on suicidal ideation or attempts.

5.3 Effects Over Time

Figure 4 presents our event study estimates from Equation 2. Panel (a) examines effects on all behavioral incidents, while the remaining panels examine each type of incident individually. Across all four panels, estimates of the effect of *Step Up* entrance before it should conceivably matter are indistinguishable from zero, suggesting that our difference-in-differences estimates are not driven by pre-existing differential trends in behavior before program entrance. Panel (a) reveals a large and immediate drop in the rate of behavioral incidents upon entering *Step Up* (6.9 p.p. decline in the first 30-day block after entrance). This effect holds over time and only attenuates slightly but remains distinguishable from zero after 3 blocks (90 days). Panel (b) conveys a similar story for disciplinary infractions. Panel (c) focuses on assaults; here, our estimates are less precise due to their relative infrequency, but all post-period coefficients are below zero and several are on the margin of statistical significance. Last, as in Table 3, Panel (d) shows no effects on suicidal behavior.

Figure 4 presents suggestive evidence that the effects of *Step Up* entrance on behavior remain stable even months later, but such a hypothesis needs formal testing, which we perform in Table 5. Here, we split coefficients for the post period into blocks 1 to 3 (short term) and blocks 4 or more (long term).²¹ Column (1) shows short- and long-term effects on incident rates of 8.8 and 6.4 p.p., respectively, and we are unable to reject the null hypothesis of the equality of these effects at the 5 percent level (p-value = 0.074). Column (2) similarly shows consistent effects over time on disciplinary infractions, and we are again unable to reject the null hypothesis that the short- and long-term coefficients are identical. In column (3), the long-term effect on assaults is about half the short-term effect and statistically insignificant; however, as with Panel (c) of Figure 4, the

²¹Note that, as this is an unbalanced panel, the sample gets smaller as block number increases. In block 4, only 394 out of the original 495 in the sample (80%) remain in the LCDC.

relative rarity of assaults and smaller number of long-term observations makes detection of this effect difficult.²² Overall, these results are consistent with the program’s impact persisting over time, even as individuals exit the program and even as the sample becomes more selected toward individuals staying longer in the LCDC.

6 Alternative Approach: Balanced & Matched Sample

Individuals stay in the LCDC for varying lengths of time, and *Step Up* participants enter the program at different points during their stay; as a result, the pools of individuals identifying the dynamic treatment estimates from Section 5.3 vary by period. Conceptually, this has the potential to be problematic; for example, it is possible that our seeming lack of pretrends in Figure 4 obscures non-parallel behavioral trends between those in the LCDC for several periods before program entrance and our comparison group.²³ On the flip side, our long-term estimates of the program’s effectiveness may be misleading if the sample of those who remain the LCDC for a substantial amount of time after *Step Up* entrance are substantially different than the average *Step Up* participant.

We therefore supplement our unbalanced two-way fixed effects results with ones obtained using a balanced panel and matched sample. Here, each treated observation is matched with a control observation staying sufficiently long in the LCDC to adequately compare to their match. The matching process is based on a propensity score predicting *Step Up* participation, which can further ensure the comparison sample is comparable. We describe the construction of this matched sample in detail below.

²²We note that a long-term reduction in assaults of 1 p.p. is still a meaningful decline of 28% from the baseline.

²³To make this idea concrete: Suppose those who are in the LCDC for several periods before entering *Step Up* have differential downward pretrends in behavioral incidents prior to program entrance. New LCDC entrants are typically worse behaved (see Figure 1); hence, as those who enter *Step Up* soon after LCDC entrance get added into the sample, their higher incident rates might counteract and obscure the pretrends among the longer stayers.

6.1 Construction of Sample

We first restrict our sample to those who stay in the LCDC for at least six 30-day blocks. We then restrict our sample of *Step Up* participants to those who are in the LCDC for at least three 30-day blocks before and after program entrance. This ensures that all treatment and comparison group individuals contribute to at least three pre- and post-period estimates. Using this sample, we then estimate a logit model predicting *Step Up* participation based on a set of baseline characteristics: four race-ethnicity categories, age, age squared, booking month-by-year indicators, the number of charges indicators (binned), the share of charges that are bondable, an indicator for having any violent charges for the current booking, security classification at entrance indicators, and indicators for any behavioral incident in each of the individual’s first three 30-day blocks in the LCDC.

Using the predicted probabilities from this logit, we match each treated individual to their nearest untreated match, with replacement. This approach matches the 219 treated individuals to 219 untreated counterparts. Within each matched pair, we assign the post period to begin in the same block of stay as their treated counterpart. For example, if a participant enters *Step Up* during their fourth block at LCDC, their untreated match’s post period will start in their fourth block of stay as well. By construction, each individual in our matched sample will have at least 3 pre- and 3 post-treatment 30-day block observations.

Table 6 describes baseline characteristics of the full sample of 219 inmates treated with *Step Up* and their matched counterparts. Column (1) provides means and standard deviations for our full sample, while columns (2) and (3) provide them for treated and untreated individuals in the matched sample, and column (4) provides the p-value on the difference. Those who participated in *Step Up* are somewhat less likely to be black, more likely to be Hispanic, and more likely to have had a behavioral incident in their 1st block at the LCDC. These characteristics are only very weakly predictive of treatment in our logit regression, which explains why our matching procedure doesn’t prioritize their balance between treatment and matched units. Unlike the unbalanced panel sample analyzed in Sections 4 and 5, the untreated and treated groups in the balanced and matched sample have very similar lengths of stay in the LCDC, similar charge counts, and similar rate of

having any violent charges for their current booking.

This balance and matching approach contains an inherent tradeoff: Expanding the required number of pre- and post-treatment blocks gives better insight into any pretends and long-term program effects, but we lose observations and statistical precision in the process. Our main estimates further restrict the sample after matching to those with at least 4 pre- and 6 post-period 30-day blocks, giving us a balanced panel of 10 blocks with 78 matched pairs. To check for robustness, we vary the pre and post period restrictions from 3 pre- and 3 post-periods (least restrictive) to 6 pre- and 7 post-periods (most restrictive). We discuss the implications of altering the pre- and post-block sample sizes in Section 6.5.

Figure 5 presents simple visual estimates of the effect of *Step Up* using the balanced and matched sample. Panel (a) uses the least restrictive subsample (3 pre and 3 post) while Panel (b) uses the most restrictive one (6 pre and 7 post). Panel (a) shows similar levels and trends in behavioral incidents among the two groups prior to program entrance; behavioral incidents then fall precipitously after treatment begins but remain high among the comparison sample. Panel (b) conveys a similar story although with more noise in the rates for both groups, which is expected given the strong sample requirements and smaller number of observations.

Appendix Figure A.5 shows the distribution of *Step Up* status (i.e., still in or exited the program) among program participants in each block in these balanced samples. In our most restrictive subsample, about half of program participants have exited the program by the last post period. Since length of participation in *Step Up* may be endogenous, we do not explore heterogeneity by participation length or split outcomes by active participation vs. post-participation status.

6.2 Matched Sample Empirical Approach

We implement an event study model comparing differences in behavioral events by 30-day block of stay between *Step Up* participants and their matched nonparticipants as participants enter the program. We estimate the following:

$$Y_{imt} = \alpha + \sum_{\substack{k \leq 5 \\ k \neq -1 \\ k \geq -4}} \beta_k Period_{imt,k} + \lambda_i + \gamma_m + \mu_t + \epsilon_{imt} \quad (3)$$

Y_{imt} measures behavioral outcomes for individual i in matched pair m during their t^{th} 30-day block in the LCDC. In our main estimates, Y_{imt} is an indicator for at least one behavioral incident during that 30-day block. The dummy variable $Period_{imt,k}$ indicates whether individual i 's t th 30-day block is k blocks relative to *Step Up* entrance. Our coefficients of interest are β_k which show the difference between *Step Up* participants and nonparticipants in each period relative to *Step Up* entrance, with period $k = -1$ as the reference period. Our main estimates are bounded between -4 and 5 , as in our unbalanced TWFE estimates, and we test the sensitivity of these bounds in Appendix Figures A.6 and A.7. We include individual fixed effects λ_i , matched pair fixed effects γ_m , and block of stay fixed effects μ_t ; the individual and matched pair fixed effects are separately identified because comparison sample individuals contribute multiple observations when used as a match for multiple treatment individuals. Standard errors are clustered at the individual booking level.

Similarly, we implement a difference-in-differences strategy comparing *Step Up* participants to the set of matched nonparticipants, capturing the average treatment effect across post period months. We estimate the following:

$$Y_{imt} = \beta_0 + \beta_1 StepUp_{mt} + \lambda_i + \gamma_m + \mu_t + \epsilon_{imt} \quad (4)$$

where Y_{imt} is an outcome for individual i in matched pair m and month t . Note that Equation 4 is equivalent to Equation 3 except for the grouping of the post months into one indicator.

6.3 Event Study Results

Figure 6 presents our event study estimates from Equation 3. Panel (a) shows that trends in incident rates between treated and untreated individuals track closely in the pre-period; once the treated

group begins program participation in block 0, their behavioral incident rate drops relative to the comparison group. All coefficients in the post period are below zero, ranging from -6.4 to -20.5 p.p., where the pre-period mean for the full sample is 14.1%. Post-period blocks 0 through 3 are statistically significant. Given the sample size and infrequency of behavioral incidents, the impacts on indicators for occurrence by month are less obvious visually, particularly when we break the outcome down into incident types in panels (b) through (d).

All estimates presented so far measure outcomes using an indicator for at least one behavioral incident in a given 30-day block. While such estimates give a sense of whether the effect of program participation persists or weakens over time, they don't clearly convey the *cumulative* effect of the program and total number of incidents prevented over one's entire jail stay. Figure 7 re-estimates Equation 3, but this time uses the total cumulative number of behavioral incidents committed by individual i by the end of 30-day block t as the outcome. Panel (a) shows that, despite tracking pre-trends, the treated group experiences a decline in accrual of new incidents relative to the comparison group in the months after joining *Step Up*. This effect is driven by disciplinary infractions as we see in panel (b), which are the bulk of behavioral incidents, as well as assaults in panel (c) which are less frequent at baseline. We see a possible decline in cumulative suicidal incidents as well in panel (d).

6.4 Difference-in-Differences Results

We next turn to regression estimates for our difference-in-differences model using the balanced and matched sample. Here we consider an average effect of the program across post-period blocks. Table 7 reports the coefficients β_1 from our estimations of Equation 4. Note that this is the same as pooling the coefficients from Equation 3 and Figure 6. The outcome Y_{imt} in column (1) is an indicator for any incident during month t for individual i in matched pair m . Columns (2) through (4) split the incidents by type. We see in column (1) that once joining *Step Up*, participants experience a reduction in monthly behavioral incident rates by 9.6 p.p. relative to a baseline of 14.1%, or a decrease of 68%.

Most of this decrease comes from disciplinary infractions, which make up the bulk of behavioral incidents. In column (2) we see that *Step Up* participants have a 8.1 p.p. decrease in having any disciplinary infraction each month, relative to a baseline of 12.2%, or a decrease of 66%. They also show a decrease in monthly assault rates by 2.4 p.p., completely offsetting the baseline, although with a large standard error. While the coefficient on suicidal behavior is negative, the baseline is low and the result is not statistically different from zero.

6.5 Sensitivity to Panel Length

One trade-off we consider in choosing panel size is between length of observation time and number of individuals in the sample. Since some individuals only have a little time in the LCDC before or after *Step Up* participation, many get dropped when requiring 4 30-day blocks before *Step Up* and 6 30-day blocks after entrance, as we do in our main estimates. We show the variation in sample size and observation counts for various balanced panels in Appendix Table A.5. If we wanted to reduce our observation of the post period from 6 blocks to 5, we would have 23 more matched pairs and 258 more observations.

We show the impact of balanced panel choice on our event study estimates in Appendix Figures A.6 and A.7, and on our difference-in-differences estimates in Appendix Tables A.6 through A.9. Table A.6 shows the coefficient of interest from Equation 4, where the number of post periods increases across the columns and the number of pre-periods increases down the rows. Our main estimate is in the fourth column of the second row. We see that reducing the post period time horizon by one month increases the magnitude of the coefficient by 0.4 p.p., and reducing it by three blocks increases the magnitude of the coefficient by 3.7 p.p. This could mean that the *Step Up* program has stronger impacts in the earlier months, or it could mean that the program is more effective on individuals who do not stay in the LCDC for a long time after entering *Step Up*.

We see increases in the magnitude of the coefficient when we decrease the number of pre-periods to 3 or increase it to 4 or 5. We are unable to draw any conclusions on whether joining the program later during one's stay has stronger or weaker effects on behavior. However, overall in Table A.6,

we can be confident that the balanced panel selection is not driving the results, as all coefficients are statistically significant and range from -7.9 and -16.2 p.p..

6.6 Comparison of Unbalanced TWFE and Matched Sample Results

Comparing the results from Table 7 to the main results in Table 3, we see that the matched sample difference-in-differences approach produces larger impacts on overall behavioral incidents (9.6 p.p. or 68% vs. 8.2 p.p. or 49%), driven by both disciplinary infractions and assaults. It is possible that the improved comparison group and balanced panel for the matched sample approach gives a clearer picture of the effects of the program. However, the 95% confidence intervals for these two estimates considerably overlap, so we cannot reject the null that the estimates are the same. The fact that we get similar results with two distinct approaches to defining treatment and comparison samples is encouraging.

7 Discussion and Conclusion

In this paper, we provide evidence on the impact of a CBT program on behavioral outcomes and well-being for high-security inmates spending at least several months incarcerated in a local jail. With a two-way fixed effects approach comparing treated participants to a sample of eligible, interested inmates who did not participate, we find that the program leads to statistically significant decreases in behavioral incidents, including disciplinary infractions and assaults. We do not find statistically significant impacts on suicidal behavior, which is an infrequent event.

We can calculate back-of-the-envelope estimates of the program’s impact on an individual’s total behavioral outcomes during their stay at the LCDC based on the results in Table 3. The mean stay in the LCDC after entering *Step Up* is 239 days or 8.47 blocks, and the average total incidents per booking is 1.81 (including 1.41 disciplinary infractions, 0.34 assaults, and 0.07 reports of suicidal behavior). The program’s impact on total incidents is therefore a reduction of 0.69 incidents or 38%, including a reduction of disciplinary infractions by 0.59 (42%) and of assaults by 0.15 (45%).

If the program were to begin at an inmate’s entry into the jail, average exposure would instead be 398 days or 13.8 blocks, leading to a decrease in behavioral incidents by 1.13 (63%), including a decrease in disciplinary infractions by 0.97 (69%) and assaults by 0.25 (73%).

The annual cost of implementing the program including supplies, curriculum books, and personnel is \$77,700. Given a program capacity of 48 participants, and an average length of stay in the program of 139 days, the program can serve about 126 individuals per year.²⁴ Thus the average cost per person to participate in *Step Up* is \$618. Since the program leads to 0.15 fewer assaults per person, the cost of an assault within the jail would need to be \$4,120 for the program to break even. Given that the public cost of an assault is \$37,457 (in 2023 USD), the program likely passes a cost-benefit test based on the assaults reduction alone.²⁵

The evidence in this paper shows that a therapy known to reduce violent behavior works well *during* incarceration. It’s possible that the institutional setting provides an opportunity for more consistent and intensive treatment and higher persistence rates relative to a community-based program, with a low cost. Our findings highlight the potential of using pretrial detention time to improve one’s behavior in the near term, increase safety within institutions, and potentially benefit public safety as well.

²⁴The median length of stay in our sample is 92 days.

²⁵Estimate is from Miller et al. (2021) updated to 2023 USD using the Consumer Price Index.

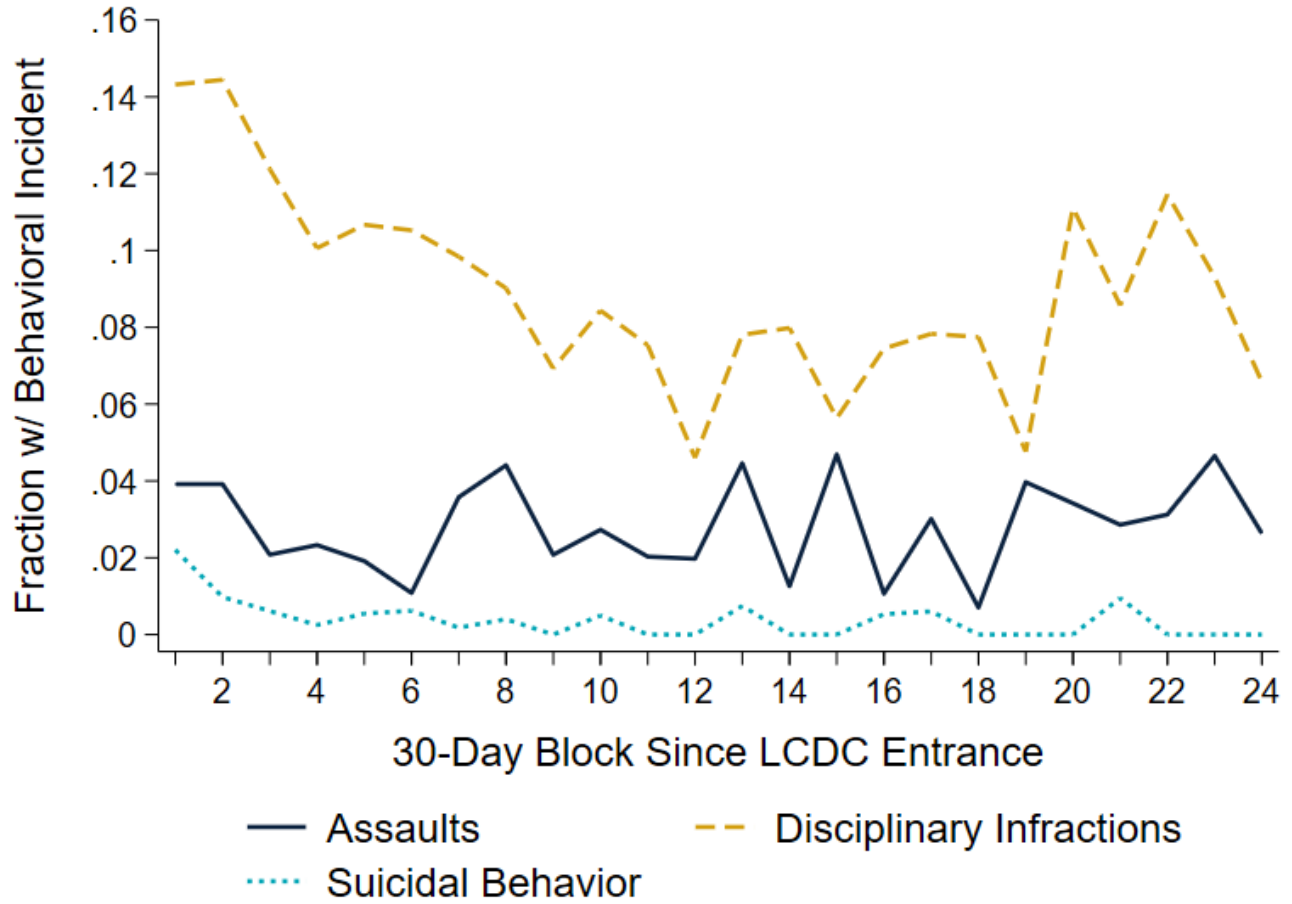
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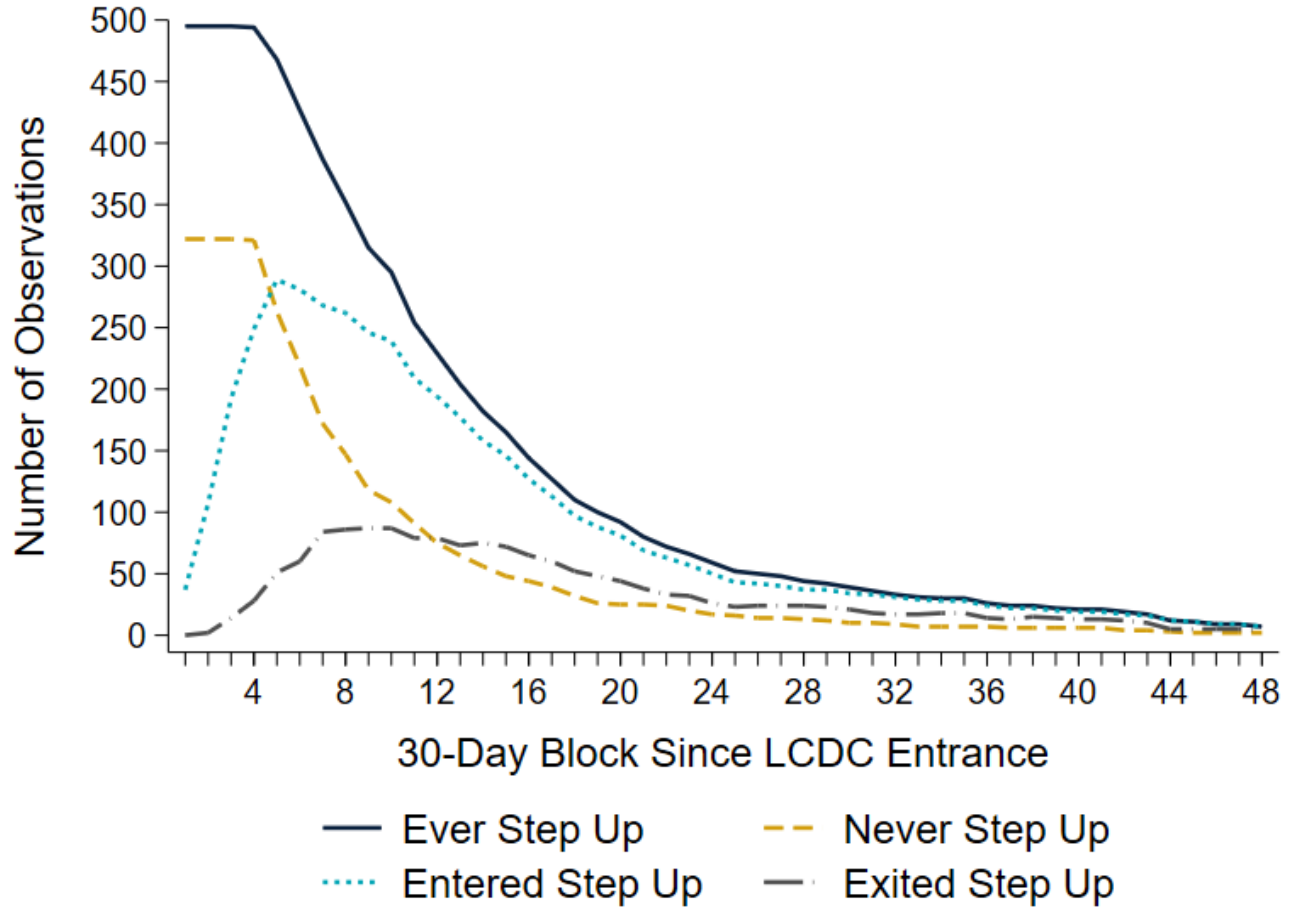
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Figure 1: Behavioral Incident Rates By 30-Day Block of Stay in LCDC



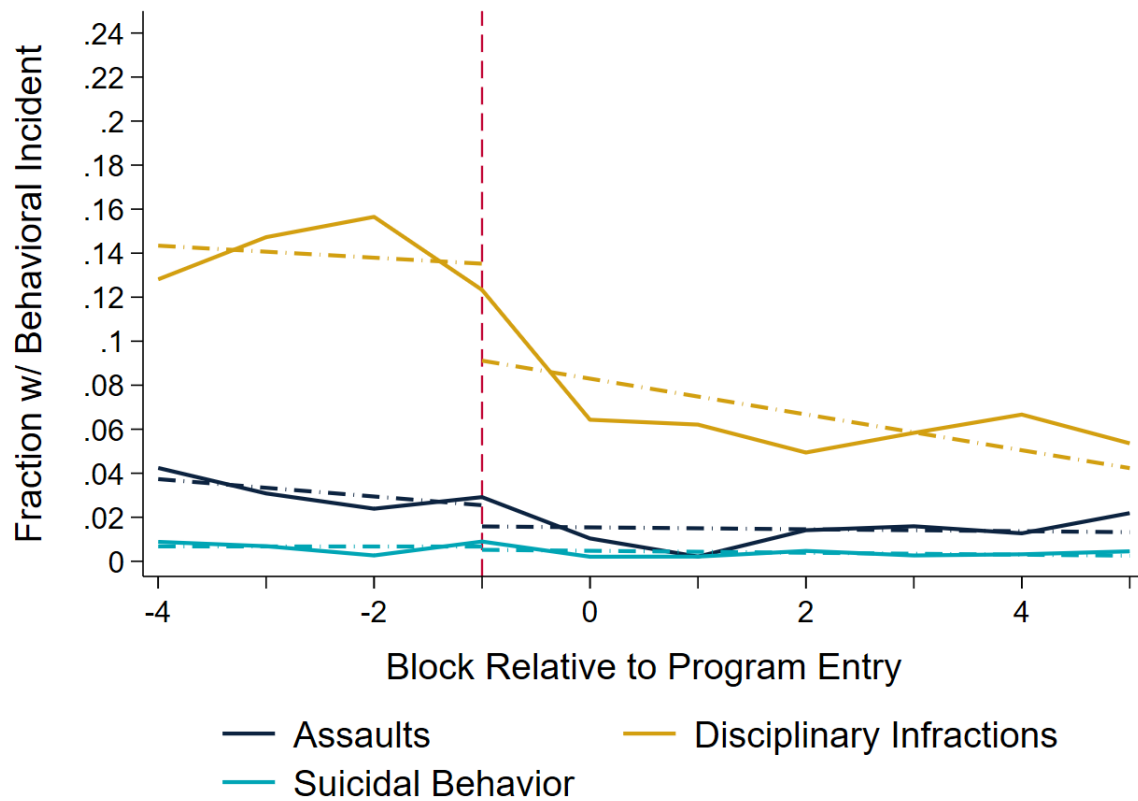
Notes: This figure shows the fraction of inmates involved in at least one behavioral incident during each 30-day block since admittance to the Lubbock County Detention Center (LCDC). The figure begins in block 1 with the full sample of 817 unique bookings of individuals staying 3 blocks or longer. The sample size decreases across the blocks as people exit the LCDC.

Figure 2: Observation Counts By 30-Day Block of Stay in LCDC, by Treatment Status



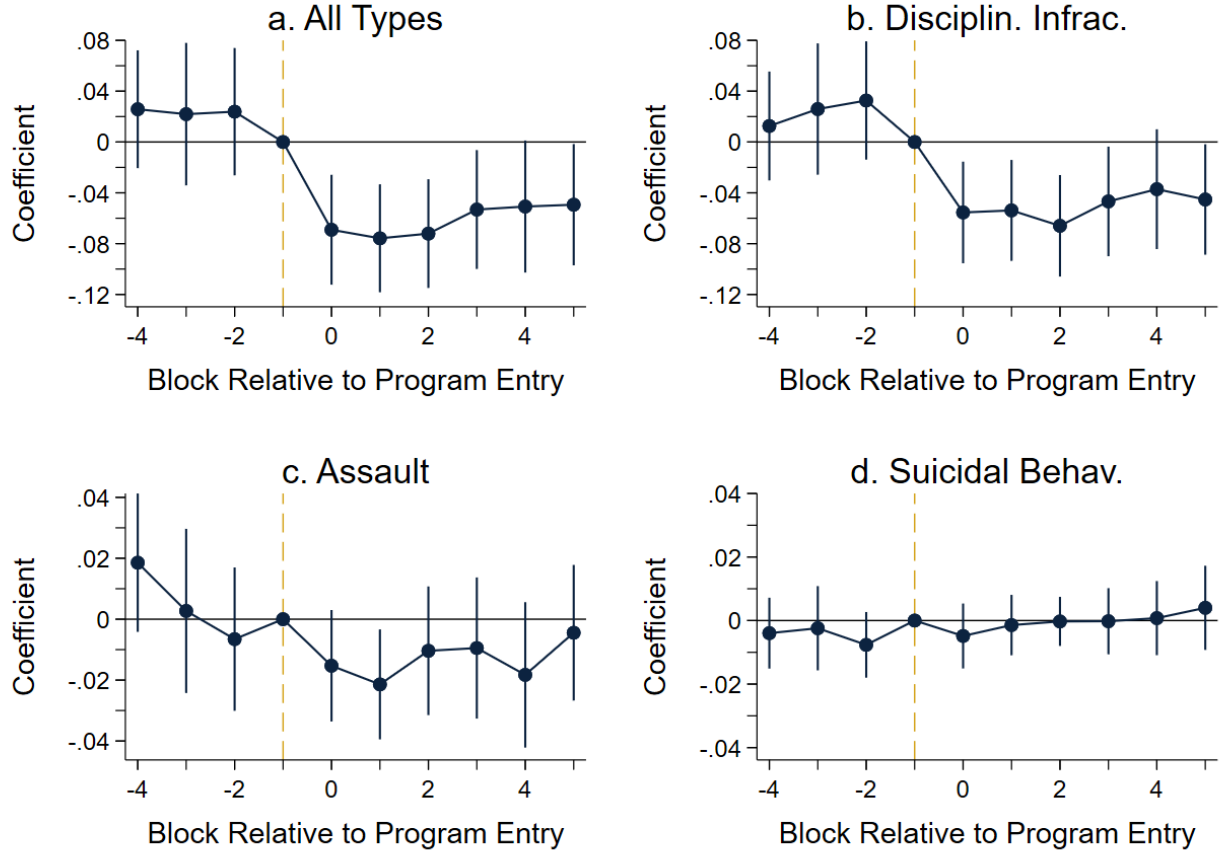
Notes: This figure shows the number of observations in our sample by 30-day block since LCDC entrance. This sample consists of 817 LCDC inmates staying at 90 days (i.e., three 30-day blocks). The solid navy blue line shows observation counts among those who enter the *Step Up* program during their stay, while the long-dashed gold line shows observation counts among those who never enter *Step Up* during their stay. The short-dashed light blue line shows observation counts corresponding to *Step Up* participants who have entered the program by the given 30-day block since LCDC entrance. The long-dashed and dotted gray line shows observation counts corresponding to *Step Up* participants who have entered and exited *Step Up* by the given 30-day block since LCDC entrance. By block 12, only 499 out of 817 individuals (37%) remain. By block 3, 192 of the 495 eventually treated individuals (39%) have entered the program.

Figure 3: Behavioral Incident Rates By Time Relative to *Step Up* Entrance



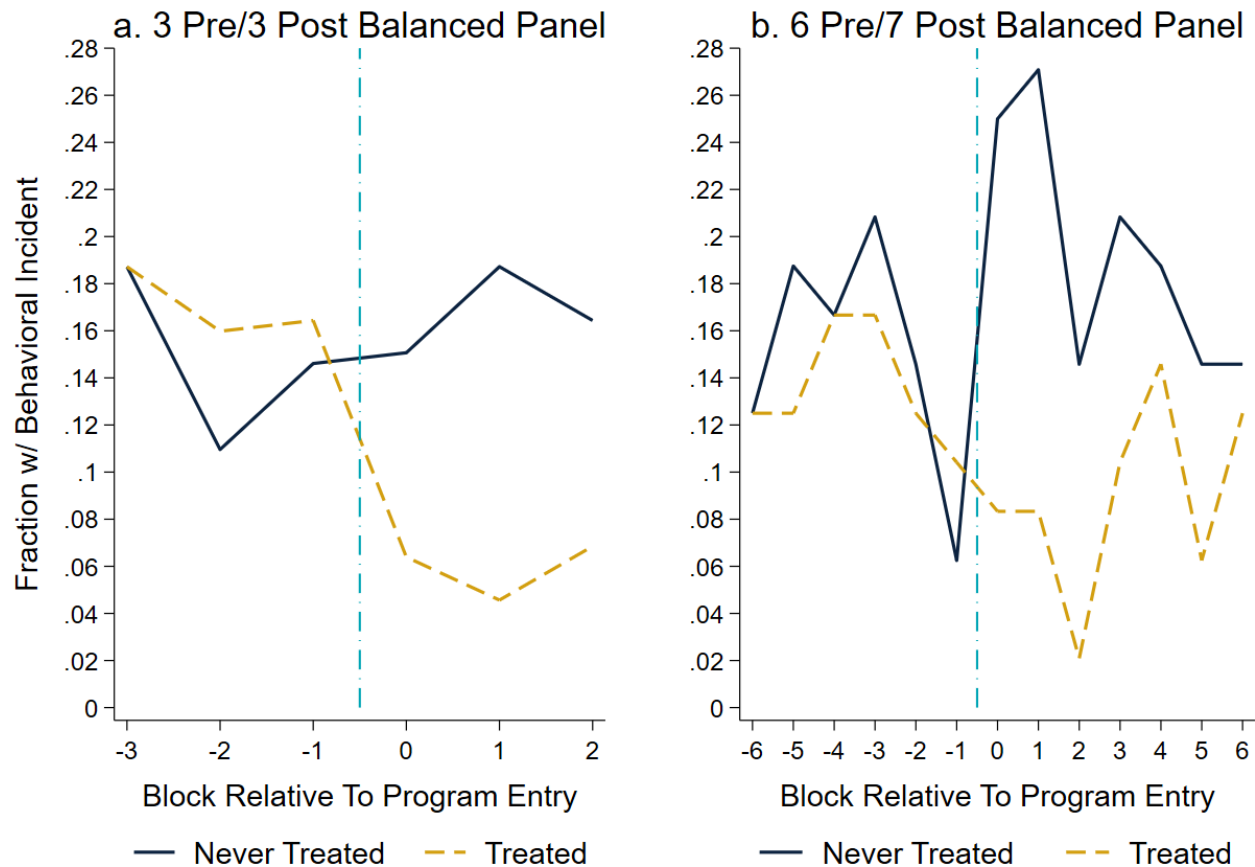
Notes: This figure presents behavioral incident rates among Step Up participants before and after program entrance. The solid lines represent the fraction of inmates involved in a behavioral incident within 30-day blocks of stay in LCDC, relative to the time of program entry for the treated group. Only the first 18 blocks of a person's stay are included. The dotted lines are lines of best fit for both the pre- and post-periods. The endpoints represent the grouped means of all relative time periods less than -4 at the lower end and greater than 5 at the upper end.

Figure 4: Impact of *Step Up* on Behavioral Incidents, Event Study Coefficients, Unbalanced Panel



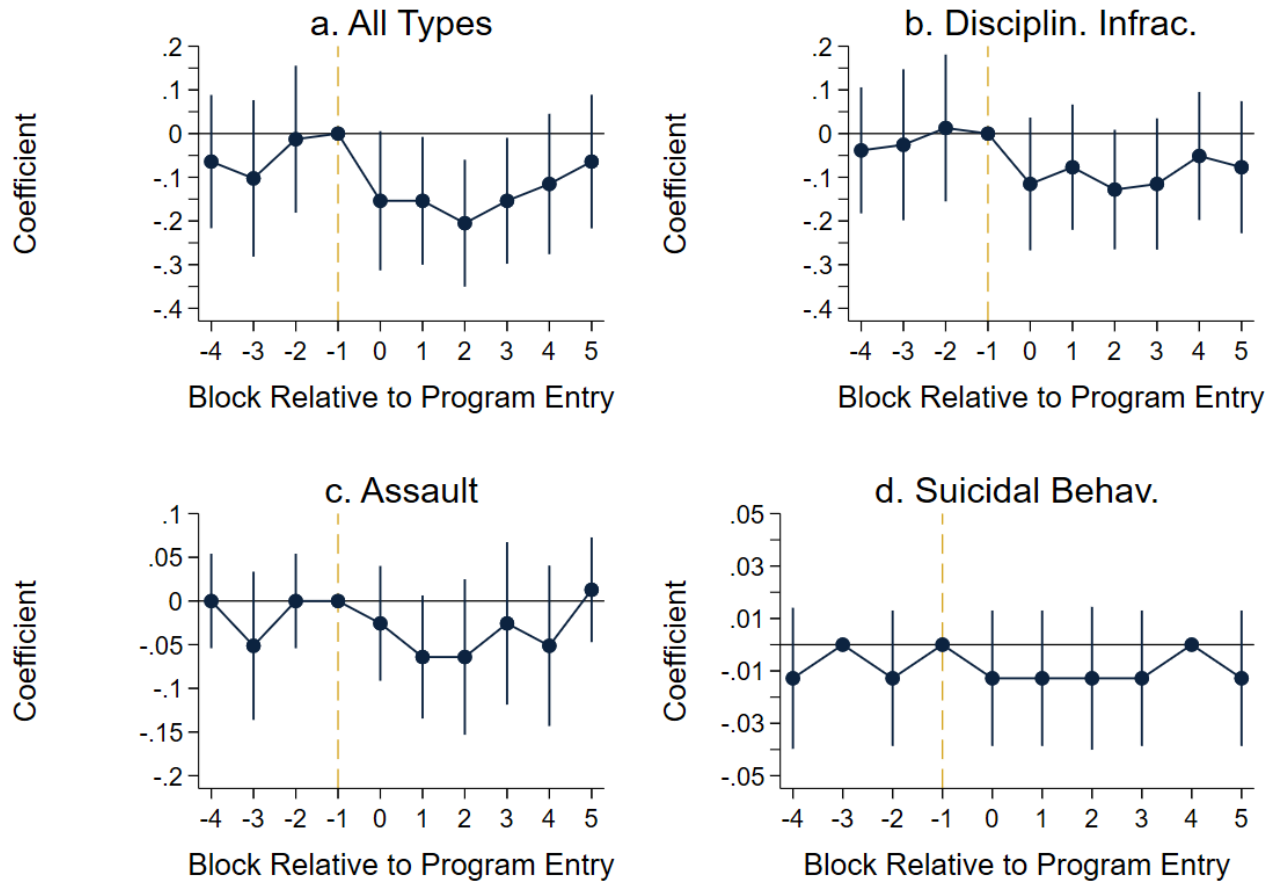
Notes: These figures visualize results from estimation of Equation 2. Each coefficient and corresponding 95% confidence interval within a panel comes from the same regression. The outcome in Panel (a) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Panels (b) through (d) focus on each type of incident separately. The estimating sample consists of individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC; further sample information is given in Section 4. Standard errors are clustered at the individual level.

Figure 5: Behavioral Incident Rates by Relative Month, Treated vs. Untreated, Matched Sample



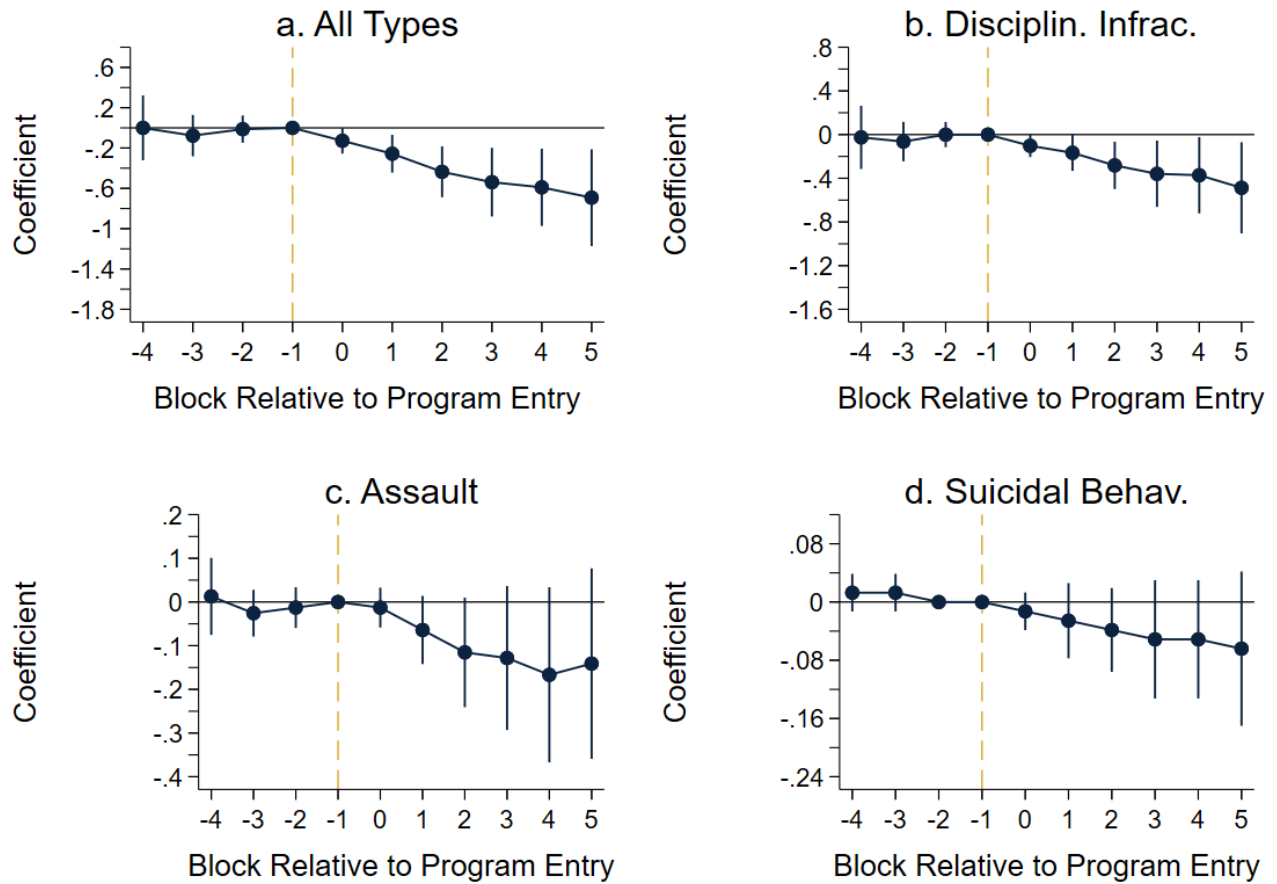
Notes: These figures show the fraction of inmates involved in at least one behavioral incident by 30-day block relative to the post period among the matched and balanced sample. The solid navy blue lines show treatment individual averages and the dashed gold lines show comparison individual averages. Panel (a) uses a sample of individuals with at least 3 pre and post periods, while Panel (b) uses a sample with at least 6 pre and 7 post periods.

Figure 6: Impact of *Step Up* on Behavioral Incidents, Event Study Coefficients, Balanced & Matched Sample



Notes: These figures visualize results from estimation of Equation 3. Each coefficient and corresponding 95% confidence interval within a panel comes from the same regression. The outcome in Panel (a) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Panels (b) through (d) focus on each type of incident separately. The estimating sample is the matched sample of 156 described in Section 6.1. Standard errors are clustered at the individual level.

Figure 7: Impact of *Step Up* on Cumulative Number of Behavioral Incidents, Event Study Coefficients, Balanced & Matched Sample



Notes: These figures visualize results from estimation of Equation 3. Each coefficient and corresponding 95% confidence interval within a panel comes from the same regression. The outcome in Panel (a) is the cumulative number of behavioral incidents (disciplinary infraction, assault, or suicidal behavior) committed since admittance to the Lubbock County Detention Center (LCDC). Panels (b) through (d) focus on each type of incident separately. The sample is the matched sample of 156 described in Section 6.1. Standard errors are clustered at the individual level.

Table 1: Jail Population Characteristics Comparison

Variable	Annual Survey of Jails, 2016-2021			LCDC Data, 2016-2021
	National	Texas	Lubbock County	Analysis Sample
Non-Hispanic White	0.648 (0.01)	0.510 (0.03)	0.309 (0.01)	0.313 (0.46)
Non-Hispanic Black	0.215 (0.01)	0.179 (0.02)	0.213 (0.01)	0.197 (0.40)
Hispanic	0.088 (0.01)	0.281 (0.03)	0.473 (0.01)	0.481 (0.50)
Other/Unknown Race	0.049 (0.01)	0.031 (0.01)	0.004 (0.00)	0.009 (0.09)
Male	0.835 (0.01)	0.823 (0.02)	0.831 (0.00)	1.000 (0.00)
Age 18-24	0.164 (0.01)	0.152 (0.01)	0.145 (0.03)	0.093 (0.29)
Age 25-34	0.362 (0.01)	0.346 (0.00)	0.361 (0.00)	0.334 (0.47)
Age 35-44	0.276 (0.01)	0.294 (0.03)	0.300 (0.02)	0.406 (0.49)
Age 45-54	0.128 (0.00)	0.137 (0.03)	0.131 (0.00)	0.124 (0.33)
Age 55-64	0.059 (0.00)	0.059 (0.02)	0.050 (0.00)	0.038 (0.19)
Age 65+	0.013 (0.00)	0.013 (0.00)	0.013 (0.00)	0.005 (0.07)
Mean Length of Stay (Days)	35.258 (5.02)	30.162 (10.22)	29.893 (6.68)	348.520 (281.81)

Notes: The table presents means and standard deviations of demographic characteristics based on the inmate population. Columns (1), (2), and (3) are derived from the Annual Survey of Jails for the years 2016 to 2021, supplemented by the Census of Jails for 2019 due to the absence of an Annual Survey in that year. National and Texas estimates are weighted according to the sampling weight adjusted for nonresponse. Columns (1) through (3) include juveniles in calculations for Non-Hispanic White, Non-Hispanic Black, Hispanic, Other/Unknown Race, and Mean Length of Stay. However, the percentages for Male and age breakdowns do not include juveniles. Column (4) contains data from the 817 individuals in our sample.

Table 2: Descriptive Statistics, Unbalanced Two-Way Fixed Effects Sample

Variable	Not Treated (1)	Treated (2)	t-Test P-Value (3)
Non-Hispanic White	0.267 (0.44)	0.343 (0.48)	0.021
Non-Hispanic Black	0.208 (0.41)	0.190 (0.39)	0.524
Hispanic	0.519 (0.50)	0.457 (0.50)	0.083
Other/Unknown Race	0.006 (0.08)	0.010 (0.10)	0.556
Age	36.478 (9.30)	36.370 (9.00)	0.868
Year of Booking	2018.078 (1.43)	2017.832 (1.41)	0.016
Month of Booking	6.553 (3.45)	6.335 (3.22)	0.359
Total Charges Count	3.601 (2.08)	4.193 (2.66)	0.001
Any Violent Charges	0.388 (0.49)	0.473 (0.50)	0.017
Missing Charges	0.019 (0.14)	0.024 (0.15)	0.594
Bondable Share of Charges	0.104 (0.24)	0.103 (0.23)	0.943
Classification at Entrance	3.478 (1.30)	3.325 (1.18)	0.082
Any Incident in Block 1	0.199 (0.40)	0.180 (0.38)	0.498
Any Incident in Block 2	0.220 (0.42)	0.158 (0.36)	0.023
Any Incident in Block 3	0.146 (0.35)	0.133 (0.34)	0.610
Length of Stay in LCDC (Blocks)	9.581 (7.64)	13.766 (10.07)	0.000
Length of Stay in LCDC (Days)	273.025 (229.42)	397.630 (301.41)	0.000
Blocks Post <i>Step Up</i> Entrance	0.000 (0.00)	8.469 (7.64)	0.000
Days Post <i>Step Up</i> Entrance	0.000 (0.00)	239.335 (229.17)	0.000
Observations	322	495	

Notes: Sample further described in Section 4 is all individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*.

Table 3: Impact of *Step Up* on Behavioral Incidents, Two-Way Fixed Effects Estimates, Unbalanced Panel

	Parameter Estimates (Standard Errors)			
	All Types	Disciplinary	Assaults	Suicidal
	(1)	(2)	(3)	(4)
<i>Step Up</i>	-0.082*** (0.014)	-0.070*** (0.013)	-0.018*** (0.006)	0.001 (0.003)
Baseline	0.169	0.135	0.036	0.008
Adjusted R ²	0.125	0.125	0.040	0.063
Observations	8,402	8,402	8,402	8,402

Notes: This table presents results from estimation of Equation 1. The outcome in Column (1) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Columns (2) through (4) focus on each type of incident separately. The estimating sample consists of individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC; further sample information is given in Section 4. Standard errors are clustered at the individual level.

Table 4: Heterogeneous Impacts of *Step Up* on Behavioral Incidents, Two-Way Fixed Effects Estimates, Unbalanced Panel

	Parameter Estimates (Standard Errors)						
	Violent Charges		Security Classification		Age Group		
	No	Yes	Lower	High	18-30	31-40	41+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) All Types							
<i>Step Up</i>	-0.068*** (0.018)	-0.094*** (0.021)	-0.114*** (0.021)	-0.065*** (0.018)	-0.118*** (0.028)	-0.087*** (0.021)	-0.045** (0.020)
Baseline	0.156	0.184	0.160	0.175	0.211	0.172	0.120
Adjusted R ²	0.124	0.125	0.123	0.126	0.111	0.109	0.153
Observations	4,413	3,989	3,486	4,916	2,355	3,613	2,434
(b) Disciplinary Infractions							
<i>Step Up</i>	-0.054*** (0.017)	-0.084*** (0.019)	-0.097*** (0.021)	-0.054*** (0.016)	-0.094*** (0.026)	-0.074*** (0.019)	-0.041** (0.020)
Baseline	0.128	0.142	0.137	0.133	0.161	0.139	0.100
Adjusted R ²	0.113	0.138	0.116	0.134	0.113	0.106	0.161
Observations	4,413	3,989	3,486	4,916	2,355	3,613	2,434
(c) Assaults							
<i>Step Up</i>	-0.021** (0.010)	-0.015 (0.009)	-0.028*** (0.009)	-0.011 (0.009)	-0.031* (0.016)	-0.019* (0.010)	-0.002 (0.005)
Baseline	0.032	0.041	0.028	0.042	0.059	0.031	0.020
Adjusted R ²	0.057	0.021	0.052	0.032	0.041	0.038	0.014
Observations	4,413	3,989	3,486	4,916	2,355	3,613	2,434
(d) Suicidal Behavior							
<i>Step Up</i>	0.005 (0.003)	-0.004 (0.006)	0.003 (0.004)	0.000 (0.004)	-0.003 (0.006)	0.008 (0.006)	-0.005 (0.004)
Baseline	0.005	0.011	0.005	0.010	0.007	0.009	0.007
Adjusted R ²	0.027	0.082	0.037	0.071	0.047	0.091	0.049
Observations	4,413	3,989	3,486	4,916	2,355	3,613	2,434

Notes: This table presents results from estimation of Equation 1 among different subsamples. The outcome in Panel (a) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Panels (b) through (d) focus on each type of incident separately. The estimating sample consists of individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC; further sample information is given in Section 4. Standard errors are clustered at the individual level.

Table 5: Short & Long-Term Impacts of *Step Up* on Behavioral Incidents, Two-Way Fixed Effects Estimates, Unbalanced Panel

	Parameter Estimates (Standard Errors)			
	All Types	Disciplinary	Assaults	Suicidal
	(1)	(2)	(3)	(4)
<i>Step Up</i> Blocks 1-3	-0.088*** (0.014)	-0.073*** (0.013)	-0.020*** (0.006)	0.000 (0.003)
<i>Step Up</i> Block ≥ 4	-0.064*** (0.018)	-0.058*** (0.016)	-0.010 (0.008)	0.004 (0.005)
Wald p-val. 1-3 vs. ≥ 4	0.074	0.226	0.128	0.152
Adjusted R ²	0.125	0.125	0.040	0.063
Observations	8,402	8,402	8,402	8,402

Notes: This table presents results from estimation of a two-way fixed effects regression like Equation 1 but with separate treatment effects for the first three periods after *Step Up* entrance and the periods thereafter. The outcome in Panel (a) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Panels (b) through (d) focus on each type of incident separately. Sample further described in Section 4 is all individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC.

Table 6: Descriptive Statistics, Balanced & Matched Sample

	Full Sample (1)	Matched Sample		
		<i>Step Up</i> (2)	Non- <i>Step Up</i> (3)	T-Test P-Value (4)
White Non-Hispanic	0.305 (0.46)	0.274 (0.45)	0.260 (0.44)	0.747
Black Non-Hispanic	0.198 (0.40)	0.187 (0.39)	0.256 (0.44)	0.085
Hispanic	0.487 (0.50)	0.521 (0.50)	0.420 (0.49)	0.035
Age	36.430 (9.46)	36.854 (9.84)	34.379 (9.81)	0.009
Year of Booking	2017.980 (1.39)	2017.689 (1.28)	2017.845 (1.30)	0.209
Month of Booking	6.446 (3.30)	6.479 (3.26)	6.594 (3.14)	0.709
Total Charges Count	4.195 (2.60)	4.526 (2.98)	4.261 (2.28)	0.308
Any Violent Charges	0.469 (0.50)	0.525 (0.50)	0.580 (0.49)	0.250
Bondable Share of Charges	0.101 (0.23)	0.101 (0.23)	0.093 (0.21)	0.701
Classification at Entrance, 1-6	3.333 (1.22)	3.320 (1.24)	3.397 (1.41)	0.541
Any Incident in Month 1	0.168 (0.37)	0.169 (0.38)	0.073 (0.26)	0.002
Any Incident in Month 2	0.173 (0.38)	0.155 (0.36)	0.187 (0.39)	0.376
Any Incident in Month 3	0.150 (0.36)	0.160 (0.37)	0.123 (0.33)	0.274
Length of Stay in LCDC (Blocks)	15.369 (9.74)	19.068 (11.30)	18.397 (9.58)	0.503
Blocks Post Step Up Entrance	6.934 (8.06)	11.329 (8.54)	. .	.
Observations	817	219	219	

Notes: This table shows descriptive statistics for the full and matched samples described in Sections 3 and 6.1.

Table 7: Impact of *Step Up* on Behavioral Incidents, Difference-in-Differences Estimates, Balanced & Matched Sample

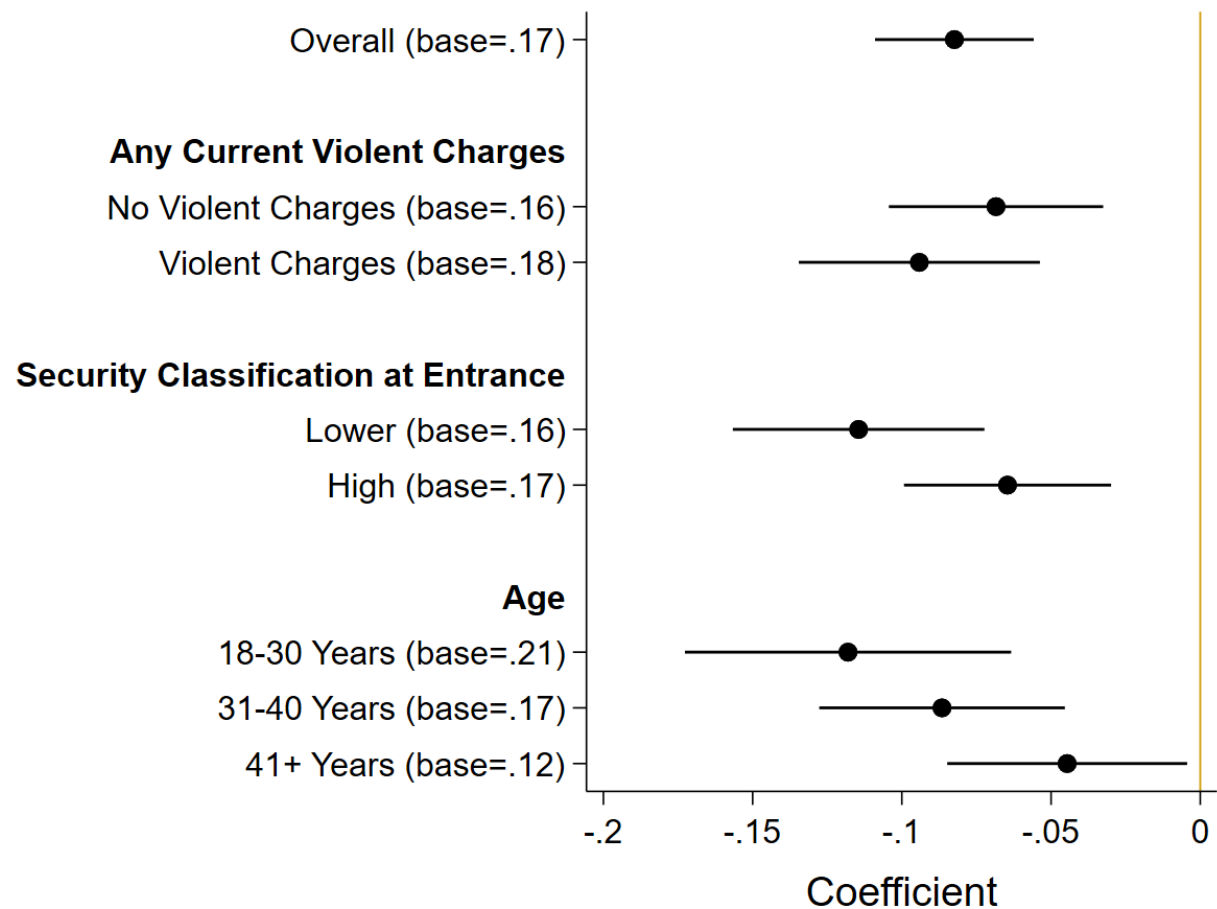
	All Types	Disciplinary	Assault	Suicidal
	(1)	(2)	(3)	(4)
<i>Step Up</i>	-0.096*** (0.036)	-0.081** (0.037)	-0.024 (0.021)	-0.004 (0.007)
Baseline	0.141	0.122	0.022	0.003
Adjusted R ²	0.095	0.093	0.019	-0.009
Observations	1,560	1,560	1,560	1,560

Notes: This table shows results from the estimation of Equation 4. The outcome in Column (1) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Columns (2)-(4) focus on each type of incident separately. Description of the balanced and matched is found in Section 6.1. Standard errors are clustered at the individual level.

A Empirical Appendix

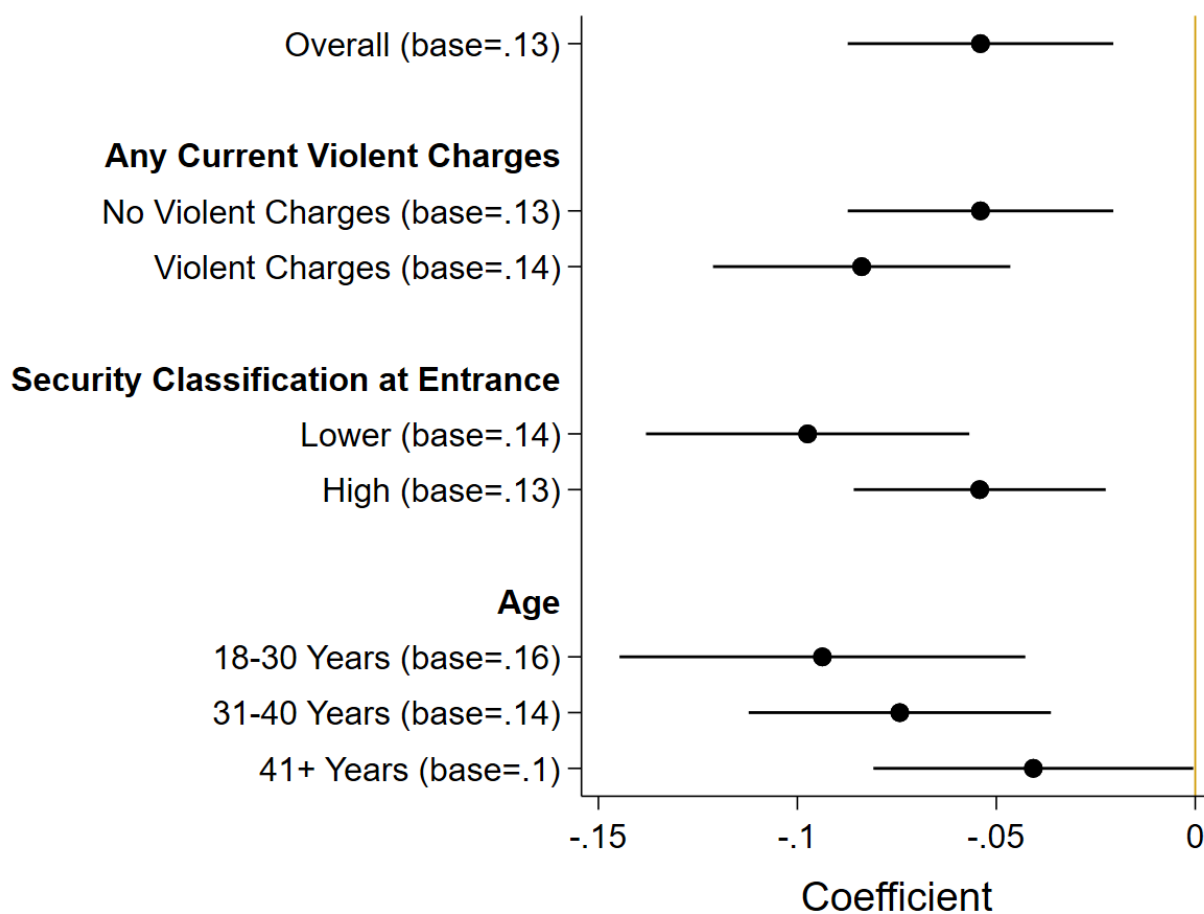
A.1 Appendix Figures

Figure A.1: Heterogeneous Impacts of *Step Up* on Behavioral Incidents, Two-way Fixed Effects, Unbalanced Panel



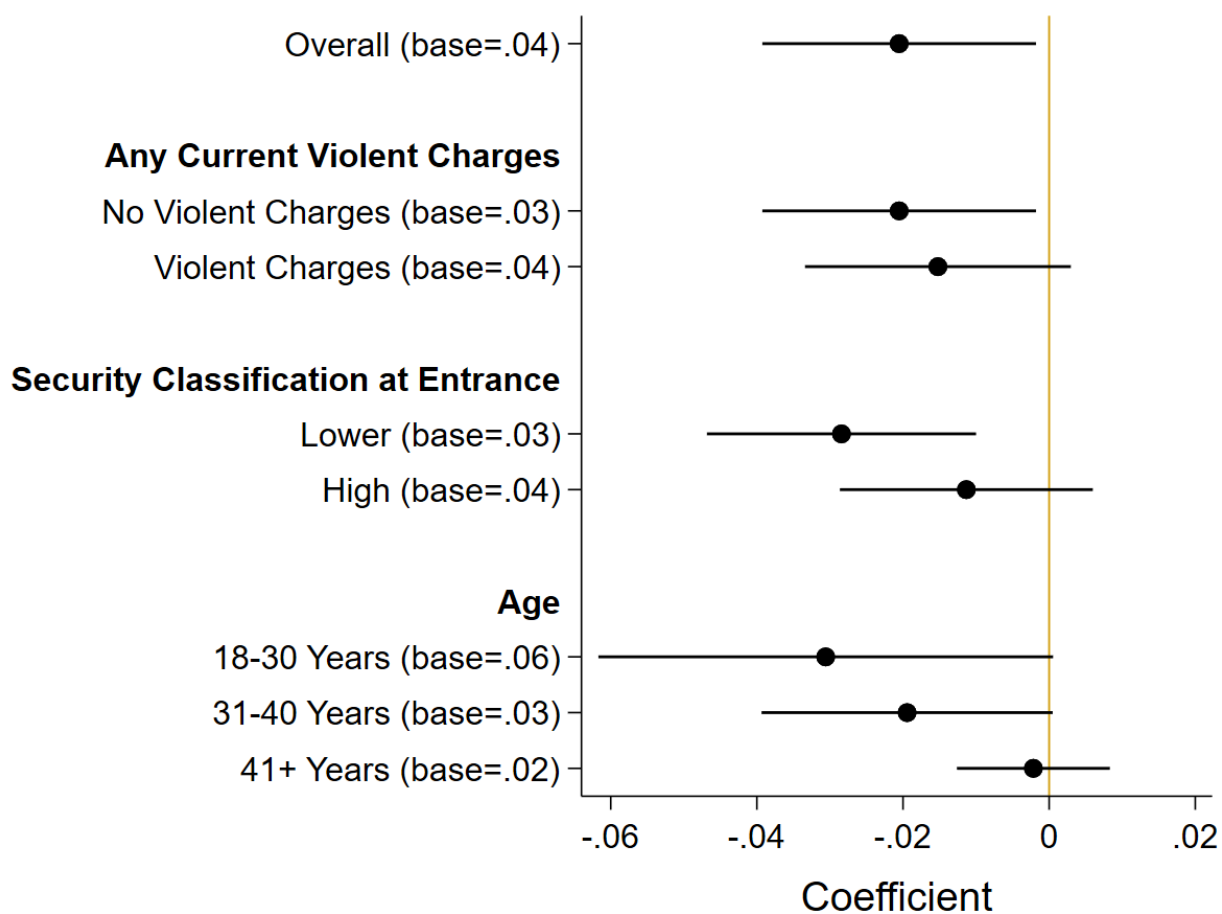
Notes: This figure shows the coefficients for our main estimates by subgroup as described in Section 5.2. Results are reported also in Table 4.

Figure A.2: Heterogeneous Impacts of *Step Up* on Disciplinary Infractions, Two-way Fixed Effects, Unbalanced Panel



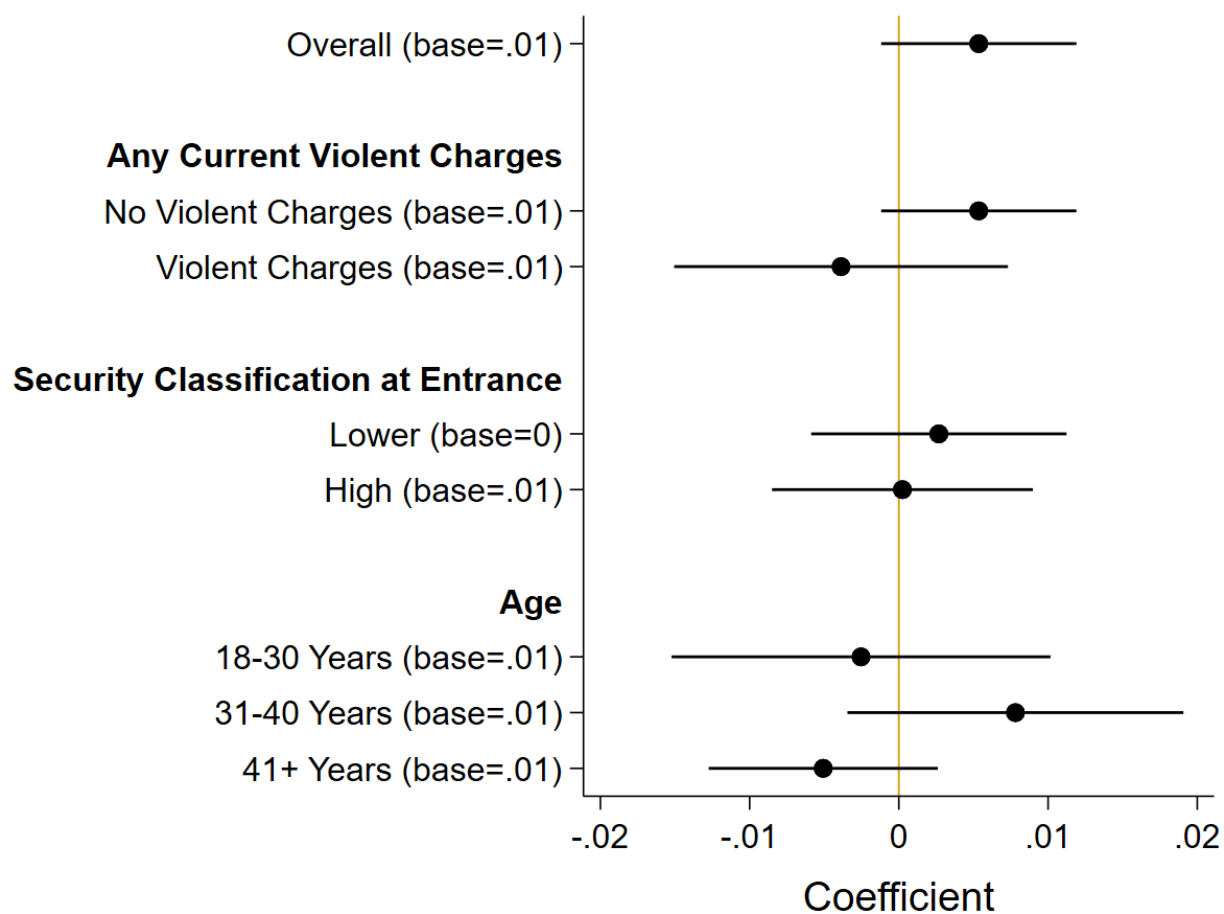
Notes: This figure shows the coefficients for our main estimates by subgroup as described in Section 5.2. Results are reported also in Table 4.

Figure A.3: Heterogeneous Impacts of *Step Up* on Assaults, Two-way Fixed Effects, Unbalanced Panel



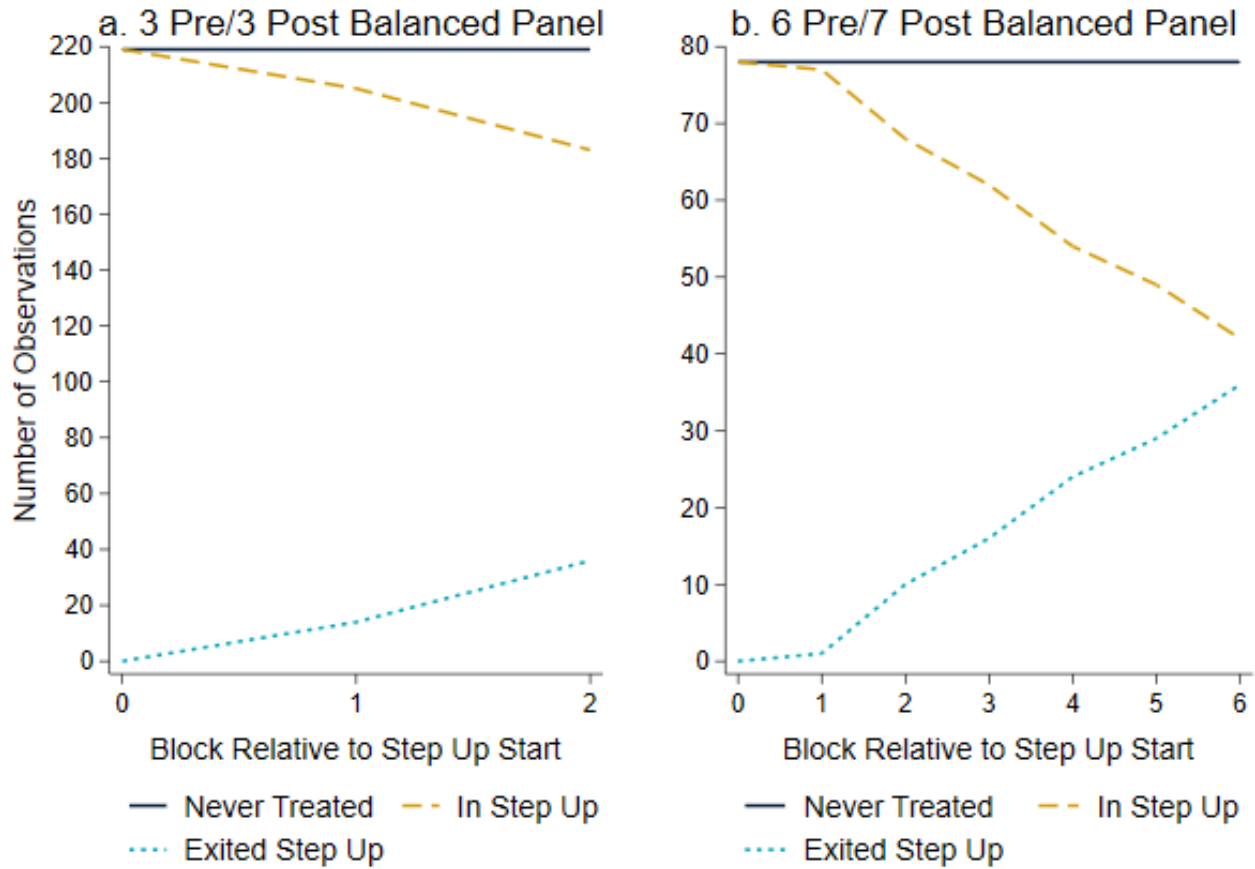
Notes: This figure shows the coefficients for our main estimates by subgroup as described in Section 5.2. Results are reported also in Table 4.

Figure A.4: Heterogeneous Impacts of *Step Up* on Suicidal Behavior, Two-way Fixed Effects, Unbalanced Panel



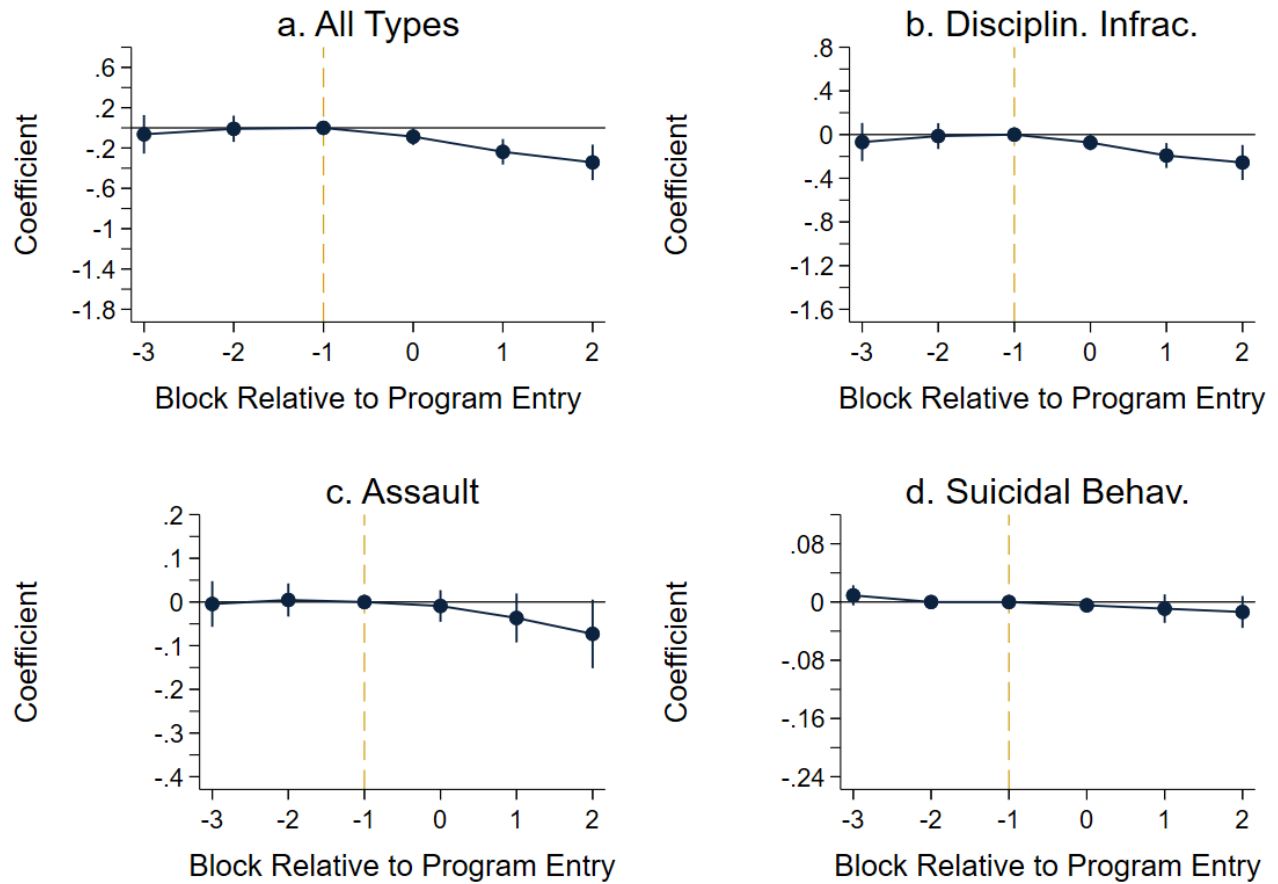
Notes: This figure shows the coefficients for our main estimates by subgroup as described in Section 5.2. Results are reported also in Table 4.

Figure A.5: Observation Counts by Program Status, Balanced & Matched Sample



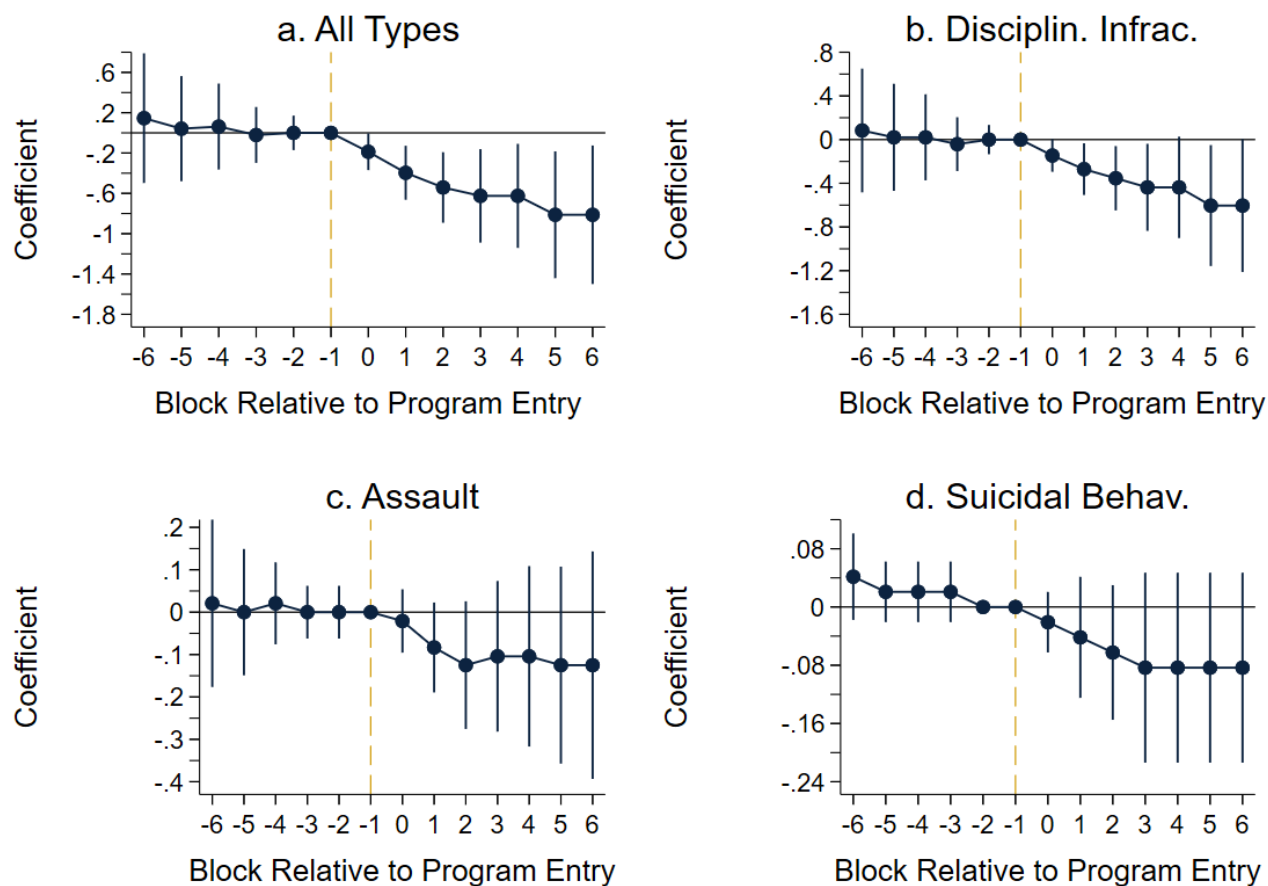
Notes: These figures show the distribution of *Step Up* program status in each 30-day block since program entrance. Panel (a) is the full matched sample ($n=300$). Panel (b) is the most restricted matched sample which is balanced with 6 pre-periods and 7 post-periods ($n=64$). This figure shows the count of *Step Up* participants who are currently in the program (“*In Step Up*”) or who are no longer in the program (“*Post Step Up*”) each month after they enter the program. It also shows the count of untreated each month, which is constant at 219 in panel (a) and 48 in panel (b).

Figure A.6: Impact of *Step Up* on Cumulative Number of Behavioral Incidents, Event Study Coefficients, 3 Pre/3 Post Balanced Panel



Notes: These figures visualize results from estimation of Equation 3. Each coefficient and corresponding 95% confidence interval within a panel comes from the same regression. The outcome in Panel (a) is the cumulative number of behavioral incidents (disciplinary infraction, assault, or suicidal behavior) committed since admittance to the Lubbock County Detention Center (LCDC). Panels (b) through (d) focus on each type of incident separately. The estimating sample is the matched sample of 156 described in Section 6.1. Standard errors are clustered at the individual level.

Figure A.7: Impact of *Step Up* on Cumulative Number of Behavioral Incidents, Event Study Coefficients, 6 Pre/7 Post Balanced Panel



Notes: These figures visualize results from estimation of Equation 3. Each coefficient and corresponding 95% confidence interval within a panel comes from the same regression. The outcome in Panel (a) is the cumulative number of behavioral incidents (disciplinary infraction, assault, or suicidal behavior) committed since admittance to the Lubbock County Detention Center (LCDC). Panels (b) through (d) focus on each type of incident separately. The sample is the matched sample of 112 described in Section 6.5.

A.2 Appendix Tables

Table A.1: Impact of *Step Up* on Behavioral Incidents, Two-Way Fixed Effects Estimates, Unbalanced Panel, Varied Time Horizons

		All Types (1)	Disciplinary (2)	Assaults (3)	Suicidal (4)
Up to 12 Blocks in LCDC	<i>Step Up</i>	-0.091*** (0.015)	-0.076*** (0.014)	-0.019*** (0.007)	-0.000 (0.004)
	Baseline	0.171	0.136	0.036	0.008
	Adjusted R ²	0.126	0.125	0.038	0.065
	Observations	7,186	7,186	7,186	7,186
Up to 18 Blocks in LCDC	<i>Step Up</i>	-0.082*** (0.014)	-0.070*** (0.013)	-0.018*** (0.006)	0.001 (0.003)
	Baseline	0.169	0.135	0.036	0.008
	Adjusted R ²	0.125	0.125	0.040	0.063
	Observations	8,402	8,402	8,402	8,402
Up to 24 Blocks in LCDC	<i>Step Up</i>	-0.075*** (0.013)	-0.065*** (0.012)	-0.016** (0.007)	0.002 (0.003)
	Baseline	0.167	0.133	0.036	0.008
	Adjusted R ²	0.124	0.125	0.039	0.062
	Observations	9,008	9,008	9,008	9,008
Unrestricted (All Blocks in LCDC)	<i>Step Up</i>	-0.067*** (0.012)	-0.058*** (0.011)	-0.013** (0.006)	0.002 (0.003)
	Baseline	0.165	0.131	0.036	0.008
	Adjusted R ²	0.122	0.119	0.033	0.063
	Observations	9,899	9,899	9,899	9,899

Notes: This table presents results from estimation of Equation 1 among different samples. The top three panels limit observations to the first 12, 18, and 24 30-day blocks of jail stays, respectively. The bottom panel includes all 30-day block observations. Our baseline sample selection limits observations to the first 18 30-day blocks. The outcome in Column (1) is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Columns (2)-(4) focus on each type of incident separately.

Table A.2: Impact of *Step Up* on Behavioral Incident Counts, Two-Way Fixed Effects Estimates, Unbalanced Panel

	Parameter Estimates (Standard Errors)			
	All Types	Disciplinary	Assaults	Suicidal
	(1)	(2)	(3)	(4)
<i>Step Up</i>	-0.087*** (0.019)	-0.071*** (0.017)	-0.017** (0.007)	0.001 (0.004)
Baseline	0.203	0.158	0.037	0.008
Adjusted R ²	0.144	0.145	0.044	0.064
Observations	8,402	8,402	8,402	8,402

Notes: This table presents results from the estimation of Equation 1 with incident counts (rather than indicators) as the outcome. The outcome in Column (1) is the count of total incidents (whether disciplinary infraction, assault, or suicidal behavior) within a 30-day block. Columns (2) through (4) focus on each type of incident separately. The estimating sample consists of individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC; Further sample information is given in Section 4. Standard errors are clustered at the individual level.

Table A.3: Heterogeneous Impacts of *Step Up* on Behavioral Incidents, By Violent Charges & Security Classification, Two-Way Fixed Effects Estimates, Unbalanced Panel

	Parameter Estimates (Standard Errors)			
	Lower Security		High Security	
	No Violent Charges (1)	Violent Charges (2)	No Violent Charges (3)	Violent Charges (4)
<i>Step Up</i>	-0.106*** (0.026)	-0.144*** (0.043)	-0.026 (0.027)	-0.086*** (0.024)
Baseline	0.149	0.189	0.165	0.182
Adjusted R ²	0.115	0.136	0.125	0.123
Observations	2,520	966	1,893	3,023

Notes: This table presents results from estimation of Equation 1 among different subsamples. Columns (1) through (4) condition the sample as such: lower security classification at LCDC admittance without a violent charge (1), lower security classification at LCDC admittance with a violent charge (2), high security classification at LCDC admittance without a violent charge (3), and high security classification at LCDC admittance with a violent charge (4). The outcome in each column is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. The estimating sample consists of individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC; further sample information is given in Section 4. Standard errors are clustered at the individual level.

Table A.4: Heterogeneous Impacts of *Step Up* on Behavioral Incidents, By Violent Charges & Age, Two-Way Fixed Effects Estimates, Unbalanced Panel

	Parameter Estimates (Standard Errors)					
	No Violent Charges			Violent Charges		
	Ages 18-30 (1)	Ages 31-40 (2)	Ages 41+ (3)	Ages 18-30 (4)	Ages 31-40 (5)	Ages 41+ (6)
<i>Step Up</i>	-0.071* (0.042)	-0.092*** (0.029)	-0.044** (0.022)	-0.144*** (0.039)	-0.080** (0.031)	-0.038 (0.038)
Baseline	0.184	0.176	0.105	0.243	0.167	0.144
Adjusted R ²	0.103	0.132	0.119	0.125	0.089	0.188
Observations	1,153	1,816	1,444	1,202	1,797	990
	Lower Security			High Security		
	Ages 18-30 (1)	Ages 31-40 (2)	Ages 41+ (3)	Ages 18-30 (4)	Ages 31-40 (5)	Ages 41+ (6)
<i>Step Up</i>	-0.195*** (0.052)	-0.092*** (0.031)	-0.054* (0.032)	-0.078** (0.034)	-0.080*** (0.029)	-0.032 (0.027)
Baseline	0.216	0.158	0.109	0.207	0.183	0.128
Adjusted R ²	0.109	0.114	0.157	0.124	0.102	0.157
Observations	872	1,578	1,036	1,483	2,035	1,398

Notes: This table presents results from estimation of Equation 1 among different subsamples determined by inmate age, security classification at LCDC entrance, and possession of a violent charge. The outcome in each column is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. The estimating sample consists of individuals entering the LCDC between April 2016 and April 2021 and who were eligible for and interested in *Step Up*. Observations are limited to up to the first 18 blocks after entering the LCDC; further sample information is given in Section 4. Standard errors are clustered at the individual level.

Table A.5: Observation Counts By Panel Size, Balanced & Matched Sample

		3 Post	4 Post	5 Post	6 Post	7 Post
3 Pre-	Observations	2,628	2,436	1,968	1,728	1,660
	Matched Pairs	219	174	123	96	83
	Unique IDs	300	226	154	118	103
4 Pre-	Observations	2,478	2,208	1,818	1,560	1,452
	Matched Pairs	177	138	101	78	66
	Unique IDs	243	180	131	99	85
5 Pre-	Observations	2,160	1,998	1,560	1,364	1,272
	Matched Pairs	135	111	78	62	53
	Unique IDs	190	150	106	82	71
6 Pre-	Observations	1,962	1,800	1,518	1,344	1,248
	Matched Pairs	109	90	69	56	48
	Unique IDs	152	121	93	74	64

Notes: This table reports the counts of observations, matched pairs, and unique IDs for various balanced panel sizes. The sample and matching approach are described in Sections 3 and 6.

Table A.6: Impact of *Step Up* on Behavioral Incidents, By Panel Size, Difference-in-Differences Estimates, Balanced & Matched Sample

		3 Post	4 Post	5 Post	6 Post	7 Post
3 Pre-	<i>Step Up</i>	-0.131*** (0.029)	-0.111*** (0.032)	-0.115*** (0.033)	-0.104*** (0.036)	-0.103*** (0.034)
	Adjusted R ²	0.106	0.103	0.091	0.091	0.101
	Observations	2,628	2,436	1,968	1,728	1,660
4 Pre-	<i>Step Up</i>	-0.133*** (0.031)	-0.114*** (0.034)	-0.100*** (0.034)	-0.096*** (0.036)	-0.087** (0.033)
	Adjusted R ²	0.078	0.080	0.090	0.095	0.110
	Observations	2,478	2,208	1,818	1,560	1,452
5 Pre-	<i>Step Up</i>	-0.141*** (0.035)	-0.129*** (0.034)	-0.108*** (0.034)	-0.099*** (0.035)	-0.079** (0.030)
	Adjusted R ²	0.071	0.073	0.082	0.100	0.120
	Observations	2,160	1,998	1,560	1,364	1,272
6 Pre-	<i>Step Up</i>	-0.162*** (0.038)	-0.151*** (0.038)	-0.122*** (0.036)	-0.113*** (0.036)	-0.090*** (0.033)
	Adjusted R ²	0.088	0.088	0.095	0.106	0.124
	Observations	1,962	1,800	1,518	1,344	1,248

Notes: This table reports the coefficient of interest from Equation 4 for various balanced panel sizes. Each coefficient comes from a separate regression. The outcome in each regression is an indicator for occurrence of at least one behavioral incident (disciplinary infraction, assault, or suicidal behavior) within a 30-day block. The sample and matching approach are described in Sections 3 and 6.

Table A.7: Impact of *Step Up* on Disciplinary Infractions, By Panel Size, Difference-in-Differences Estimates, Balanced & Matched Sample

		3 Post	4 Post	5 Post	6 Post	7 Post
3 Pre-	<i>Step Up</i>	-0.110*** (0.029)	-0.092*** (0.031)	-0.091*** (0.033)	-0.089** (0.035)	-0.080** (0.034)
	Adjusted R ²	0.112	0.116	0.096	0.096	0.100
	Observations	2,628	2,436	1,968	1,728	1,660
4 Pre-	<i>Step Up</i>	-0.109*** (0.031)	-0.094*** (0.035)	-0.073** (0.035)	-0.081** (0.037)	-0.070* (0.036)
	Adjusted R ²	0.082	0.085	0.089	0.093	0.103
	Observations	2,478	2,208	1,818	1,560	1,452
5 Pre-	<i>Step Up</i>	-0.103*** (0.036)	-0.093*** (0.035)	-0.069* (0.035)	-0.072* (0.037)	-0.061* (0.034)
	Adjusted R ²	0.077	0.078	0.084	0.101	0.114
	Observations	2,160	1,998	1,560	1,364	1,272
6 Pre-	<i>Step Up</i>	-0.128*** (0.040)	-0.129*** (0.039)	-0.096*** (0.036)	-0.092** (0.037)	-0.079** (0.034)
	Adjusted R ²	0.092	0.088	0.094	0.105	0.116
	Observations	1,962	1,800	1,518	1,344	1,248

Notes: This table reports the coefficient of interest from Equation 4 for various balanced panel sizes. Each coefficient comes from a separate regression. The outcome in each regression is an indicator for occurrence of at least one disciplinary infraction within a 30-day block. The sample and matching approach are described in Sections 3 and 6.

Table A.8: Impact of *Step Up* on Assaults, By Panel Size, Difference-in-Differences Estimates, Balanced & Matched Sample

		3 Post	4 Post	5 Post	6 Post	7 Post
3 Pre-	<i>Step Up</i>	-0.024*	-0.022	-0.027	-0.019	-0.022
		(0.014)	(0.016)	(0.018)	(0.019)	(0.019)
	Adjusted R ²	0.026	0.009	0.018	0.017	0.016
	Observations	2,628	2,436	1,968	1,728	1,660
4 Pre-	<i>Step Up</i>	-0.030*	-0.024	-0.032*	-0.024	-0.023
		(0.016)	(0.019)	(0.019)	(0.021)	(0.022)
	Adjusted R ²	0.026	0.009	0.017	0.019	0.023
	Observations	2,478	2,208	1,818	1,560	1,452
5 Pre-	<i>Step Up</i>	-0.040*	-0.037*	-0.041*	-0.036	-0.024
		(0.020)	(0.022)	(0.023)	(0.024)	(0.024)
	Adjusted R ²	0.031	0.026	0.028	0.042	0.051
	Observations	2,160	1,998	1,560	1,364	1,272
6 Pre-	<i>Step Up</i>	-0.041**	-0.031	-0.033	-0.033	-0.021
		(0.021)	(0.021)	(0.021)	(0.022)	(0.022)
	Adjusted R ²	0.044	0.032	0.029	0.042	0.051
	Observations	1,962	1,800	1,518	1,344	1,248

Notes: This table reports the coefficient of interest from Equation 4 for various balanced panel sizes. Each coefficient comes from a separate regression. The outcome in each regression is an indicator for occurrence of at least one assault within a 30-day block. The sample and matching approach are described in Sections 3 and 6.

Table A.9: Impact of *Step Up* on Suicidal Behavior, By Panel Size, Difference-in-Differences Estimates, Balanced & Matched Sample

		3 Post	4 Post	5 Post	6 Post	7 Post
3 Pre-	<i>Step Up</i>	-0.002 (0.005)	-0.002 (0.006)	-0.004 (0.005)	-0.005 (0.004)	-0.005 (0.003)
	Adjusted R ²	-0.003	-0.004	-0.006	-0.006	-0.007
	Observations	2,628	2,436	1,968	1,728	1,660
4 Pre-	<i>Step Up</i>	-0.004 (0.004)	-0.004 (0.005)	-0.003 (0.006)	-0.004 (0.007)	-0.003 (0.007)
	Adjusted R ²	-0.005	-0.006	-0.007	-0.009	-0.009
	Observations	2,478	2,208	1,818	1,560	1,452
5 Pre-	<i>Step Up</i>	-0.007 (0.007)	-0.007 (0.008)	-0.005 (0.009)	-0.001 (0.009)	0.001 (0.010)
	Adjusted R ²	-0.004	-0.004	-0.007	-0.010	-0.011
	Observations	2,160	1,998	1,560	1,364	1,272
6 Pre-	<i>Step Up</i>	-0.005 (0.007)	-0.004 (0.008)	-0.002 (0.009)	-0.003 (0.008)	-0.001 (0.008)
	Adjusted R ²	-0.007	-0.007	-0.008	-0.010	-0.011
	Observations	1,962	1,800	1,518	1,344	1,248

Notes: This table reports the coefficient of interest from Equation 4 for various balanced panel sizes. Each coefficient comes from a separate regression. The outcome in each regression is an indicator for occurrence of at least one instance of suicidal behavior within a 30-day block. The sample and matching approach are described in Sections 3 and 6.

B Facility and Program Documents

Figure B.1: Aerial View of the LCDC



Notes: This is an aerial view of the LCDC accessed from Google Earth on April 26, 2024.

Figure B.2: Initial Security Classification Form

CLASSIFICATION NOTICE					
NAME: (last)		(first)		(middle)	
JAIL NO:		D.O.B.	RACE:	SEX:	SSN:
Charge(s)	1)	2)	3)		
You have been placed in the following classification:					
This classification is determined by: current/past convictions; current/past institutional behavior; pending charges or holds in other jurisdictions (if any); sentenced or unsentenced; and/or any other information that may be deemed appropriate with regard to your personal security or the security of the facility. NOTE: Your classification can change when: charges are altered or reduced; you are sentenced; due to administrative hearings; due to regular periodic review; and / other reasons recommended by the jail staff. Appeal Process: any inmate dissatisfied with his or her classification must appeal his / her classification, in writing, within ten (10) days of the primary classification or reclassification by addressing his/her appeal as APPEAL OF CLASSIFICATION and directing it to: the Sheriff or his designee.					
Classified by:					Date:
Comments by staff:					

Booking Date:

Override Reason:

High Risk: (Check)

☐ Assaultive

☐ Escape

☐ Suicidal

☐ Mental

☐ Gang Leader

☐ Other _____

Special Condition: (check)

☐ Protective Custody

☐ Medical

☐ Juvenile

☐ Handicap/Disabled

☐ Body Fluid watch

☐ Other _____

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Figure B.3: Security Reclassification Form

Texas Reclassification Tree

Inmate No. _____

Name: _____

Are Classification review Circumstances Aggravating? or Mitigating?

High Risk Flags

☐ Assaultive

☐ Escape

☐ Suicidal

☐ Mental

☐ Gang Member

☐ Other _____

Special Condition Flags

☐ Protective Custody

☐ Medical

☐ Juvenile

☐ Handicap/Disabled

☐ Body Fluid Watch

☐ Other _____

Override Reason: _____

Override Assignment: (circle) 1 2 3 4 5 6 7 8 9 Date: _____

Review Officer: _____ Next Review Date: _____

Comments: _____

____ No Change in Current Classification (check)