“THE LONG-TERM CONSEQUENCES OF BEING PLACED IN RESTRICTIVE HOUSING”

Key words: Restrictive housing; solitary confinement; inequality; incarceration; registry data

ABSTRACT

Being placed in restrictive housing is widely considered one of the most devastating experiences a human can endure, yet virtually no research provides a strong test of how this experience affects core indicators of prisoner reentry such as employment and recidivism. In this article, we use Danish registry data, which allow us to link penal conditions to individual outcomes even years after release, to test the long-term effects of having been placed in restrictive housing. Results from difference-in-differences analyses indicate that Danish inmates placed in restrictive housing experience markedly larger drops in employment and markedly larger increases in the risk of being convicted of a new crime in the three years following prison release than do Danish inmates who were not placed in restrictive housing, including those who were sanctioned for a serious in-prison offense but not placed in restrictive housing. Because being placed in restrictive housing is so common, these results indicate that restrictive housing placement is a possible moderator of the effects of incarceration that merits more attention from criminologists.

INTRODUCTION AND BACKGROUND

Research generally indicates that being placed in restrictive housing,[1] which is often referred to colloquially as being in solitary confinement or "the hole," does serious psychological harm to inmates (e.g., Andersen 2004; Cloud et al. 2015; Gawande 2009; Grassian 2006; Guenther 2013; Haney 2003, 2018; Reiter 2016; Smith 2006). Data limitations, however, make it difficult to precisely estimate this causal effect, leading to some debate in the field about whether placement in restrictive housing does indeed harm inmate mental health (e.g., Kapoor and Trestman 2016; Labrecque 2016; Morgan et al. 2016). During the actual time that inmates are in restrictive housing, they experience a range of problems including trouble sleeping, impaired concentration, lethargy, irrational anger, depression, hallucinations, and suicidal ideation and suicide attempts (Andersen et al. 1996; Haney and Lynch 1997; Kaba et al. 2014; Kupers 1999; Rhodes 2004). While many of these problems increase with time in isolation, some studies have argued that profound psychological trauma can begin even a few days after being placed in restrictive housing.[2] And for many, these effects do not fully dissipate after their release from restrictive

[1] We use the term "restrictive housing" rather than the term "solitary confinement" throughout. Following previous research, we consider an inmate in restrictive housing whenever they "are removed from the general population of the institution and confined to their cells for more than 22 hours per day" (Kapoor and Trestman 2016:200).

[2] As Koch (2014:101-102) notes, "symptoms often occur after only a few days. Most common are problems of concentration, restlessness, failure of memory, sleeping problems, and impaired sense of time and ability to follow the rhythm of day and night…Nightmares and anxiety are very common…Suicide attempts are often made."

housing, leading to poor mental health outcomes months and even years into the future (Andersen 2004; Grassian 2006; Haney 2003; Smith 2006; but see Kapoor and Trestman 2016).[3]

While the consequences of placement in restrictive housing for mental health have been broadly tested, albeit with imperfect data, evidence regarding the broader consequences of having ever been placed in restrictive housing is sparse in comparison. This is unfortunate for a number of reasons. First, if the often-extreme psychological difficulties that result from being placed in restrictive housing are indeed due to that experience, these difficulties are likely to spill out to other domains, making it difficult for inmates to refrain from criminal activity, gain and maintain employment, and pro-socially engage with society more broadly (Andersen 2004; Gordon 2013; Grassian 2006; Haney 2003; Smith 2006). Given how well-documented the effects of poor mental health are on labor market outcomes (e.g., Ettner et al. 1997) and criminal activity (e.g., Moffitt 1993), moreover, it is reasonable to expect long-term consequences of restrictive housing placement on these broader outcomes, assuming, again, a causal effect on mental health.[4] Second, knowing how restrictive housing placement affects former inmates' outcomes not just in terms of mental health but also in other domains provides a more complete assessment

[3] As Haney (2018:297) notes, "[p]risoners may develop extreme habits, tendencies, perspectives, and beliefs that are difficult or impossible for them to relinquish once they are released. Although their adaptations may have been functional under condition of isolation…they are highly dysfunctional in the social world most prisoners…re-enter."
[4] Although we see mental health problems induced (or exacerbated) by social isolation, and to a lesser degree, sensory deprivation, as the core driver of the consequences of restrictive housing placement for post-release adjustment, for long stints in restrictive housing, reductions in programming access may also drive those effects.

of the positive and negative long-term consequences of restrictive housing than we currently have. As a large number of inmates cycle through restrictive housing each year, this is not merely an academic concern, as better identifying its consequences provides insights into how one highly prevalent condition of confinement affects how former prisoners fare after release. Third, the evidence regarding the positive effects of restrictive housing in terms of inmate safety and subsequent behavior is at best mixed (e.g., Morris 2016), suggesting the need for a broader assessment of the unintended consequences of restrictive housing placement for inmates.

Important though testing the long-term consequences of restrictive housing placement is,[5] doing so is exceptionally difficult because it requires (1) information on inmates who are placed in restrictive housing, as well as those who are not, (2) that is linked with information on core post-release outcomes and pre-admission outcomes, and (3) has relatively low levels of attrition. Each of these conditions in and of itself would represent challenges, as the number of studies using data including any information on prison conditions (e.g., Kreager et al. 2016), linking administrative data on prisoners to their post-release outcomes (e.g., Loeffler 2013), or having low attrition among this population (e.g., Western et al. 2016) is quite small. And, as such, it is perhaps unsurprising that we know of no other dataset that could be used to test the long-term effects of being placed in restrictive housing on former inmates' post-release outcomes.

[5] Many of these issues also apply to testing the short-term consequences of restrictive housing placement.

In order to circumvent these problems, we use a unique dataset that combines two administrative data sources: (1) Danish registry data, which includes information on key demographic controls and employment; and (2) data from the Danish Prison and Probation Service, which includes controls for prior contact with the criminal justice system, as well as information on restrictive housing placement and other in-prison sanctions and convictions. We focus on employment and conviction risk because they are central indicators of how successful prisoner reentry has been and because so much research on the consequences of incarceration focuses on them (e.g., Petersilia 2003; Travis 2005; Western 2006; Western et al. 2015). These unique data allow us to use a difference-in-differences model to provide the most rigorous test to date of the consequences of restrictive housing placement for post-release outcomes.

The results from these models provide support for two key conclusions. First, although Denmark is rarely considered punitive, rates of restrictive housing placement in Danish closed prisons and American prisons—which represents the most punitive of criminal justice systems—are similar, especially after adjusting for differences in the average duration of incarceration. Although this may be surprising to some, Denmark has long been known to have high rates of restrictive housing placement and this result, hence, confirms that fact (e.g., Reiter et al. 2016). Second, restrictive housing placement leads to worse post-release outcomes in terms of the probability of employment and of being convicted. Restrictive housing placement is, simply put, a major obstacle to successful reentry for formerly incarcerated individuals in Denmark.

These results are important for three reasons. First, they contribute to research on the costs of restrictive housing by using high-quality data and rigorous methods that control for unobserved heterogeneity to show that restrictive housing placement has long-term consequences for the chance of being employed or convicted. There are well-known ethical reasons to oppose the use of restrictive housing. Our results show that the practice may also be counterproductive, as placing prisoners in restrictive housing can significantly compromise their chance of successfully reintegrating into society in two vitally important dimensions after release.

Second, these results call us to rethink effect heterogeneity in the consequences of incarceration for core life-course outcomes. Research shows negative effects of incarceration on labor market prospects (e.g., Pager 2003; Western 2002, 2006; but see Loeffler 2013), health (e.g., Massoglia 2008; Massoglia and Pridemore 2015; Schnittker and John 2007), and family life (e.g., Comfort 2008; Lopoo and Western 2005). Because incarceration is so heavily concentrated among already-marginalized populations, these individual-level effects, moreover, have implications for inequality (e.g., Wakefield and Uggen 2010; Western 2006). Yet important as these average effects are, a growing body of research seeks to consider effect heterogeneity in the consequences of incarceration (e.g., Turney 2017), and some speculate that the conditions of confinement represent a key driver of any heterogeneity that exists (Wildeman and Muller 2012; Wildeman and Wang 2017). Unfortunately, testing how conditions of confinement affect the outcomes of the formerly incarcerated has hitherto been difficult, making it unclear whether

these conditions induce heterogeneity in incarceration's effects. By showing that being placed in restrictive housing, one key condition of confinement experienced by a large share of inmates in any given year, moderates the effects of incarceration on individual's employment and risk of re-conviction in the next three years, we provide an important starting point for conversations about how conditions of confinement may significantly moderate incarceration's consequences.

Third, these results also provide insight into how we think about the mechanisms driving the effects of incarceration. As many researchers have noted (e.g., Muller and Wildeman 2012), deciphering whether the consequences of incarceration for life-course outcomes are driven by the stigma of incarceration or by the transformation produced by incarceration has proven a tall order because while experimental manipulations make it possible to test for stigma (Pager 2003), few designs using observational data make it possible to differentiate the effects of stigma from the effects of transformation. In this study, by holding incarceration constant—since our entire sample had been incarcerated—and varying exposure to restrictive housing, we show that for some former prisoners, the transformation induced by incarceration is quite large and negative.

Taken together, these results from our analyses suggest that as criminologists return to prisons and jails (e.g., Kreager and Kruttschnitt 2018; Kreager et al. 2017; Walker 2016; Wildeman et al. 2018), they must consider the effects of restrictive housing not only on those who experience it for years on end, but also on those who experience it relatively briefly.

RESTRICTIVE HOUSING IN DENMARK

The Experience of Being in Restrictive Housing in Denmark

On the most basic level, restrictive housing is used for quite similar reasons in virtually all contexts, with inmates ending up in restrictive housing to: (1) preserve the safety of other inmates, guards, or individuals outside of the institution (administrative segregation); (2) punish an inmate for some specific infraction (disciplinary segregation); or (3) preserve the safety of an inmate, either voluntarily or involuntarily (protective custody). Throughout the course of this manuscript, we focus on the first two types of restricting housing placement—being placed in administrative segregation and being placed in disciplinary segregation—since those forms of restrictive housing are most consistent with broader discussions of restrictive housing.

As we noted above, within the Danish context we focus on two types of restrictive housing, each of which we discuss in more detail here.[6] First, restrictive housing may be used as a preventative measure. Here, a prisoner may be segregated from the prison community to prevent him or her from committing crimes or to prevent administratively unwanted acts (anti-social and often repeated acts which are incommensurable with staying in the prison community)

[6] There are two special cases of restrictive housing in Denmark. First, some prisoners ask to be placed in restrictive housing because they fear for their safety. They are placed in separate units, and conditions of confinement are less harsh than if they were in restrictive housing (doors remain unlocked for a few hours each day, for example, if security and the prison staff permit it). Being in restrictive housing voluntarily is associated with being labelled a "snitch" (Minke 2012). Second, there are (few) prisoners whom the prison staff find so hard to handle that they are placed in extraordinarily long restrictive housing spells. We do not have information specifically on these two special cases of restrictive housing in our data and, hence, we do not focus on them throughout this article.

and in health-related instances (to prevent the spread of contagious diseases, for example). This type of restrictive housing is required to undergo weekly evaluation since, as in many other contexts, there is no set end date to this type of restrictive housing. Second, restrictive housing may be used as a disciplinary measure. Here, a prisoner may be segregated from the prison community as a sanction for having committed a disciplinary infraction. Disciplinary infractions that can lead to solitary confinement as a punitive measure include (attempted) escape (for example during weekend leave); possession of contraband, such as alcohol, drugs, or weapons; refusing to provide a urine sample for alcohol and drug testing; violent behavior (or the threat hereof) towards other prisoners or staff; vandalism; and other serious and repeated infractions. In Denmark, disciplinary segregation cannot exceed consecutive four weeks for any offense.

Restrictive housing in Denmark takes place in one of five types of cells, reflecting similar heterogeneity in restricting housing as other contexts (Kapoor and Trestman 2016): A cell in an administrative segregation unit; the inmate's own cell; a cell in a local jail; an observation cell (with little inside of it, all of which is nailed to the floor); or in a security cell (which only contains a bed that may be used for restraining the prisoner using belts and foot-straps). Observation and security cells are only used for the most severe cases; prison staff is required to observe the prisoners placed here at regular intervals (Langsted et al. 2011).

Danish accounts of being in restrictive housing are admittedly somewhat limited, but anecdotal and qualitative evidence suggests that the experience is much the same in Denmark as

it is in most other developed democracies (e.g., Koch 2014; Smith 2006). Qualitative research (Minke 2012) describes the administrative segregation unit of one of the high security (closed) Danish prisons. This unit is known among prisoners as "the hole," which is language that is consistent with how restrictive housing is discussed in the United States. It is very silent, and all doors are always locked. Cells are approximately 86 square feet, and they have wire mesh in front of the window. As is the case with restrictive housing more broadly, inmates are kept in their cell 22-23 hours per day. The walls are undecorated; there is minimal furniture. Outside each cell door there is a blackboard with the prisoner's ID number written on it in chalk along with a note on the reason for being placed in administrative segregation. Other prisoners who are also placed in the unit can observe this information when they leave their cell for exercise, which also takes place in solitude in a separate small yard that has bars or wire mesh as a roof.

Although there are many core ways in which the purpose and experience of restrictive housing align in Denmark and other developed democracies, there are two differences in the use of restrictive housing across contexts that merit attention. First, there are no Danish "supermax" prisons. And so for American prisoners living in long-term restrictive housing as a condition of their confinement, there is no Danish equivalent. This difference is especially important because most of the existing research on the consequences of restrictive housing in the US context and in other developed democracies has focused not on inmates who are in restrictive housing for short periods but on those in long-term restrictive housing (e.g., Briggs et al. 2003; Haney 2003;

O'Keefe et al. 2013; Shalev 2009; Toch 1975 [2007]; Travis, Western, and Redburn 2014:183-188; Zinger et al. 2001; but see Morris 2016; Useem and Piehl 2006). Our analyses relate not to this group but to inmates who are usually in the general population and will be in a correctional facility for only a year or two. We see this as a benefit of studying the Danish system, as these are the inmates who will be back out in society in short order and, hence, whose adjustment to post-prison life is both substantively and theoretically interesting. But it is still worth noting.

Second, Danes who are detained pretrial spend virtually all of their time in restrictive housing. This has been standard practice in Denmark for decades, a practice that has repeatedly been criticized by international human rights organizations (Engbo and Smith 2012). Most pretrial detainees in Denmark are kept in individual cells in local jails. Because of the risk of collusion, they are not allowed to communicate with one another, they only have the right to one daily hour of outdoor exercise, and they often have restricted visitation, mail, and phone calls.

The Prevalence and Duration of Restrictive Housing in Denmark

In order to show how common restrictive housing is in Denmark, we used data on all Danes who experienced incarceration from 2006-2013 (more on this later). In order to provide some context into how unusual these rates of restrictive housing placement are, Table 1 compares the Danish results to results based on surveys during the years 2011-2012 in the US (Beck 2015).

In Denmark, roughly 0.6 percent of inmates across all facility types were in restrictive housing last night. This is dramatically lower than the 1.9 percent in American prisons and 2.2 percent in American jails. Because most (although not all) American prisoners who received the paper form of the survey, which did not include a question about the type of housing they were living in, were in restrictive housing (Beck 2015:16), however, the true share of American prisoners in restrictive housing is closer to 4.4 percent, meaning that on any given night American prisoners were seven times as likely to be in restrictive housing as Danish prisoners.

[Insert Table 1 about here.]

Although the share of Danish and American inmates in restrictive housing on any day differs, the difference in the share of Danes sent to restrictive housing in the last year in closed prisons—the most direct comparison to American prisons—and American prisons is small. Roughly 20.3 percent of Danes in a closed prison had ever been placed in restrictive housing in the last year, which corresponds closely with the 18.1 percent observed in American prisons. The similarity also applies to jails, where around 17 percent of both Danes and Americans had been placed in restrictive housing. Danish open prisons had much lower cumulative rates of restrictive housing placement, and while some of this difference is driven by shorter stays in this type of correctional facility, even adjusting for this, large differences in restrictive housing persist.

Focusing just on Danish closed prisons and American prisons, Table 1 further shows that the reason American prisoners are so much more likely to be in restrictive housing than Danes in

closed prisons (although they have a similar cumulative prevalence of being placed in restrictive housing) is because the average duration of restrictive housing placement is much longer in the US than in Denmark. Only 0.8 percent of Danish inmates in closed prisons had ever been placed in restrictive housing for more than two weeks, while 8.6 percent of American prisoners had spent at least two weeks in restrictive housing (reasonably assuming those in restrictive housing units at the time of the survey would ultimately spend at least two weeks in restrictive housing).

DATA, MEASURES, AND ANALYTIC STRATEGY

Data

In this manuscript, we rely on two sources of administrative data. First, we rely on Danish registry data (see Andersen 2018 for a description). There are three key features of the registry data for our analyses. First, they include information on the population of Danes, meaning there is virtually no attrition. Especially in this area, where attrition rates for most of the core datasets used hover around 20% (e.g., Wakefield and Wildeman 2013; Western 2002), this is no small benefit. Second, the data include a rich set of information about the family life, educational background, mental health, employment history, and criminal history of the population. Finally, the data can be linked to other datasets with the Danish equivalent of a social security number.

The second dataset, which has never before been made available for research purposes, includes information on all Danish prisoners from 2006-2013. From these data, we keep all

spells of imprisonment which started in 2006 or later and which had ended by the end of 2013.[7] For the purposes of our analyses, a couple of components of the dataset merit special attention. First, because the data include complete information on where each inmate was each day, we can precisely estimate the duration of restrictive housing placement for each inmate who experienced it. Second, the data include information on all infractions individuals were sanctioned for while incarcerated. And, as such, the data allow us to consider the long-term outcomes of three distinct types of inmates: (1) those who were never sanctioned for their behavior while incarcerated; (2) those who were sanctioned but never placed in restrictive housing; and (3) those who were sanctioned and eventually placed in restrictive housing. This is a unique feature of the dataset that facilitates our analysis, as it allows us to compare inmates who are placed in restrictive housing to the prison population as a whole, and then to compare inmates who are placed in restrictive housing to a more similar group—those who received some sort of sanction for their behavior while incarcerated but were not placed in restrictive housing. The strength of the first comparison is that it shows how incarcerated individuals in general fare in Denmark and contrast these "normal" levels and trajectories in outcomes with what we observe for individuals who are placed in restrictive housing. The first comparison thus shows how select the "treatment group" is relative to inmates in general. But exactly because the treatment group is so select, likely also

[7] For the abovementioned analyses of the prevalence of solitary confinement in the prison population shown in Table 1, we kept all prisoners in the data, including those with imprisonments extending beyond our data window.

on factors that we do not observe in the data, it is unclear how much of the difference between the outcomes of the treatment group and the general inmate population can be attributed to the treatment. Although our analytic strategy, which we describe below, effectively estimates the effects of restrictive housing placement using either of these reference groups, because we see the sanctioned but not placed in restrictive housing as more directly comparable to those who were placed in restrictive housing, much of our discussion focuses on those comparisons.

Measures

Using these two datasets, we construct our dependent, explanatory, and control variables.

Our key dependent variables are criminal conviction and employment.

Criminal Conviction. We obtain criminal convictions from official court transcripts which include information on offense dates, allowing us to focus on the timing of the offense (court delays thus do not influence our results). With these official conviction data, we observe whether (0 = "No", 1 = "Yes") each person in the data violated the penal code (for which he or she was at a later point convicted) within a window of two-to-three years before imprisonment and three years following release from prison. We do not include offenses committed during the last year before imprisonment in our pre-admission measure, as offenses at this point are likely to be treated as co-offenses to the case that eventually sent the offenders to prison (and, as a result, offenses in this time window could thus be endogenous to the imprisonment we observe).

Although we could have also considered incarceration as a dependent variable, we choose to focus on new convictions only because we see this as a more appropriate gauge of recidivism than being picked up for technical violations of parole. We also run supplementary analyses which instead consider arrest; these results yield estimates comparable to those herein.

Employment. We obtain employment status from official tax records. In Denmark, there is full third-party reporting of incomes to the tax authorities—employers are required to report salaries, fringes, bonuses, severance pays, board fees, stock options, salaries during leave, and even non-taxable salaries directly to the tax authorities—and any income from legal labor work is thus counted on these records. With these official tax records, we observe whether (0 = "No", 1 = "Yes") each person in the data had any income from employment in the formal labor market before and after imprisonment. Because tax records are filed annually, we cannot identify with certainty whether formal labor income during the year when imprisonment occurs was generated before or after the imprisonment spell. Therefore, we divide annual labor income by 12 to obtain average monthly labor income within years, and we then ignore average labor income during the last 11 months before imprisonment and the first 11 months following release from prison. Doing so ensures that we do not conflate the timing of income with the timing of imprisonment but still maintain a balanced data structure for all individuals in the data.

Figure 1 presents descriptive statistics of our main outcome variables.

[Insert Figure 1 about here.]

Explanatory Variable. Our key explanatory variable is whether each inmate experienced (1) no sanctions during their incarceration, (2) some sanctions but no restrictive housing placement, and (3) sanctions including restrictive housing placement. The second experience—some sanctions but no placement—includes non-solitary disciplinary actions which, according to the Danish Criminal Law, includes fines, warnings, conditional solitary confinement, and the confiscation of contraband (Langsted et al. 2011). Table 2 displays descriptive statistics on the share and characteristics of inmates who experienced restrictive housing placement. Table 2 also provides descriptive statistics on inmates who experienced: sanctions but no restrictive housing; no sanctions; and no restrictive housing (which lumps both reference groups together). As we noted earlier, our data do not contain information on protective custody. As such, our analyses focus on the effects of placement in disciplinary segregation and administrative segregation.

[Insert Table 2 about here.]

Control Variables. All analyses also include a host of controls. Most of these are listed in Table 2; all controls are described more in the supplementary online appendix. The control variables include an array of information about the inmates, including admission and release year, age, gender, and other demographic controls; their prior and recent contact with the mental health care system (defined as whether they were referred by their general physician to the mental health care system and used the referral) this ever happened before this incarceration ("ever contact") and within the last three years prior to this incarceration("recent contact"); and

whether they had experienced any incarceration before the current one (0 = "No", 1 = "Yes"); as well as information about their current incarceration. Specifically, we control for sentence length and length of incarceration, the type of crime which caused the incarceration, and whether the inmate was held in a closed prison, an open prison, or a jail. As we noted in Table 1, these distinctions are essential given the different rates of restrictive housing across facility type.

Analytic Strategy

Statistical Model. We rely on difference-in-differences (DID) models to measure the effect of restrictive housing placement on criminal convictions and employment. In practice, DID models exploit the panel structure of the data to evaluate differences in outcome trajectories rather than just differences in post-release outcome levels.[8] There are three main strengths of this approach. First, by focusing on pre-admission outcomes in addition to post-release ones, the DID approach explicitly takes into account that there are differences between those who experience restrictive

[8] Fitted to individual level (i) panel data, the additive DID model may be specified in our context as $y_{it} = \alpha_{it} + \beta_1 restrictive_{it} + \beta_2 post_{it} + \beta_3 restrictive \times post_{it} + \beta_4 X_i + \varepsilon_{it}$ where *t = 0* prior to incarceration and *t = 1* following release from prison. *y* is the outcome. *Post* is a dummy variable equal to 0 before and 1 after incarceration, and *restrictive* is a dummy variable equal to 1 for those who experienced restrictive housing and 0 otherwise. *X* is a set of controls, and de-meaning all continuous variables in *X* allow us to interpret the intercept (α) as the pre-incarceration outcome level for the non-restrictive housing group. $\beta_1$measures the difference between the groups prior to incarceration. β_2 then measures the change in outcome of the non-restrictive housing group from before to after incarceration, and β_3, the parameter of specific interest in our setup, measures any additional change in the outcome for the restrictive housing group. ε is the error term. We estimate all models using OLS and because we have two observations for each individual incarceration experience, we cluster standard errors at the individual-by-spell level. Because our outcome variables are binary we also check whether our main results are sensitive to our choice of estimator. These results are available in the online supplementary materials with other robustness checks.

housing and those who do not—differences that would persist even absent restrictive housing. If we did not take such pre-existing differences into account it would be impossible to evaluate how much of the differences in post-release outcomes were driven by conditions of confinement and how much were driven by pre-existing differences between the groups. Second, and related to the first strength, the DID approach exploits the panel structure of the data to net out the impact of any stable individual characteristics that are not observed in the data. If we focused only on post-release outcomes and even if we controlled extensively for observed background characteristics, any remaining difference in outcomes could still be driven by unobserved characteristics, which would pose a serious threat to our identification of the effects of restrictive housing. Third, the DID approach explicitly takes time trends in the outcomes into account. In our setup, this means that we take into consideration that there are general and well-documented effects of imprisonment on the outcomes we study (e.g., Western 2006), even in the absence of restrictive housing. If we focused only on the change in outcomes for the treatment group, it would be impossible to evaluate how much of the change was brought about by experiencing the treatment and how much was driven by the passage of time. The key identifying assumption of the DID estimator is therefore the assumption of common trends. This assumption states that, in our setup, the outcome trajectory of the non-restrictive housing group adequately represents what the outcome trajectory of the restrictive housing group *would have been* in the absence of restrictive housing. This assumption is fundamentally untestable, yet from the outcome plots

(Figure 1) there is no reason to suspect differing outcome trajectories before the incarceration—our identifying assumption is then that this would also hold true across the incarceration experience if the groups did not differ in the conditions of that experience.

No statistical model is without caveats. The caveats of DID models are that (1) they require panel data and (2) they cannot address unobserved time-varying traits. In our setup, we overcome caveat (1) by relying on highly detailed Danish administrative data that has a panel structure. But caveat (2) is more challenging as it concerns time-varying unobserved heterogeneity. We aim to overcome caveat (2) by restricting part of our analysis to only include individuals who were sanctioned for in-prison behavior, a point we return to shortly. We thereby aim to minimize the risk that unobserved time-varying individual factors influence our results.

With the DID estimator we measure the changes in outcomes in absolute terms on the scale of the dependent variable, which in our case means in percentage points. Yet there are substantial differences in the levels of criminal convictions and employment prior to incarceration for those who experienced restrictive housing and those who did not. Therefore, absolute scales are not ideal for our study, as they could place us in a situation where the same percentage point change would be relatively more impactful for either one of the groups when evaluated against their pre-incarceration levels. We therefore combine the estimates from the DID models to obtain the relative pre-to-post change in outcomes for each group. We then attribute the difference in the differences in the outcomes to the effect of restrictive housing.

Main Analysis. For each outcome, we run two sets of models, each of which proceeds in two stages. In the first, we provide a descriptive assessment of how different the relative change from before to after incarceration in the cumulative probability of conviction and employment is for those who experienced restrictive housing and those who did not. This stage essentially replicates the analyses from Figure 1, with the addition of focusing on post-release levels of outcomes relative to pre-admission levels (and statistically testing the changes). In the second, we adjust for the entire list of controls, which will provide more precise parameter estimates.

In one set of models, we compare the outcomes of individuals who experienced restrictive housing to those of all inmates who did not. We do this to show how incarcerated individuals in general fare in Denmark and contrast these "normal" levels and trajectories in outcomes with what we observe for individuals who are placed in restrictive housing. In a second set of models, we compare the outcomes of individuals who experienced restrictive housing to those of inmates who did not but who did experience some other serious sanction. As discussed, we see the models limited to these arguably more similar groups as offering the most plausible estimates of the effect of restrictive housing from our analyses. Although the differences between inmates who experienced restrictive housing and individuals who received only other sanctions are still statistically significant and substantial in some cases—an issue we return to in more detail soon—the difference between these two groups of individuals is much smaller than the

difference between individuals who were ever placed in restrictive housing and those who received no sanctions. As such, we see using this reference group as a strong analytic step.

Of course, it could be the case that inmates who were placed in restrictive housing and who were given other sanctions committed fundamentally different types of infractions while in captivity. If that were the case, we might expect our lack of individual-level information on the reason for being punished for an in-prison infraction to be especially problematic. As Table 3, which uses aggregate data on in-prison infractions that resulted in restrictive housing placement or some other sanction, indicates, there are some differences between these two groups in terms of their types of infractions. Interestingly, with the exception of violations of the penal code, which make up a relatively small share of all offenses (4.4%),[9] the largest differences are found for possessing illegal drugs and weapons—with 13.2% of those in restrictive housing committing this offense and 20.5% of those not committing this offense—and possessing other illegal goods, most often cell phones—with 41.6% of those in solitary committing this offense and 34.2% of those not committing this offense. When combined, these two categories make up 54.8% of the cases of restrictive housing and 54.7% of cases that received other sanctions. When combined with the small differences across other categories, it would be reasonable to conclude that the types of offenses that land inmates in restrictive housing or lead to other sanctions differ little.

[Insert Table 3 about here.]

[9] Violence and threats comprise the majority of violations of the penal code in Danish prisons and jails.

Although no data exist on the relationship between infraction type and length of restrictive housing, through personal correspondence with the Danish Prison and Probation Service, we learned that there is vast heterogeneity in the length of restrictive housing imposed for specific offenses. And while some offenses, such as violence and threats, tend to bring longer stays in restrictive housing (sometimes in excess of a week), more seemingly minor offenses can also bring quite long stints in restrictive housing. Refusing a drug test, for instance, comes with a standard five days in restrictive housing. And being caught with a cell phone for a third time in a closed prison comes with a mandatory sentence of 28 days in restrictive housing.

Supplementary Analyses. We run five sets of supplementary analyses to provide a detailed picture of how the effects of restrictive housing are structured. *First*, we estimate results by pretrial detention status. Here, we split the analyses into those who experienced pretrial detention while awaiting trial and those who did not. We do so because virtually all individuals who experience pretrial detention in Denmark do so in restrictive housing in a jail. As such, all inmates who experienced pretrial detention had *de facto* experienced restrictive housing prior to admission. *Second*, we estimate results where only punishment cell confinement is counted as restrictive housing. Being placed in a punishment cell indicates restrictive housing due only to serious disciplinary infractions rather than some other issues. *Third*, because we expect the effects of restrictive housing to run through strained mental health, as discussed in the Introduction and Background, we estimate the effect of restrictive housing on the risk of

experiencing contact with the mental healthcare system outside the prison system. Danish prisons have their own hospitals and their own system for prescribing drugs to inmates. We do not have information on the use of these in-prison systems in our data.[10] Instead, our data only indicate whether people have contact/consultancy with the general mental healthcare system before and after release, and our results for mental healthcare use should be viewed as extreme lower bounds. Note that we do not control for recent contact with the mental health care system in this supplementary analysis, as this information is contained in the outcome. *Fourth*, we estimate results for other definitions of the main outcomes: crime type; risk of attest; formal labor market earnings; and dependence on public benefits (see the supplementary online appendix for more detail). *Finally*, we estimate results by length of restrictive housing. Specifically, we run models breaking both the longest consecutive bout of restrictive housing and the total duration of restrictive housing into three categories: 1-2 days; 3-7 days; and 7+ days.

RESULTS

Effects on Convictions

[10] Previous research had access to in-prison mental healthcare use during pretrial detention and found an increase in the risk of using it once subjected to restrictive housing (Sestoft et al., 1998). For prisoners who spend four weeks in restrictive housing, for example, the risk was about 20 times as high as for prisoners who did not experience it. Although some of that difference may well be attributable to something other than the treatment, in a population as homogenous as the penal population, a relative risk of 20 for severe mental health problems suggests some effect.

Table 4 presents results from the DID models considering the long-term consequences of having ever been placed in restrictive housing on the relative change in the risk of being convicted of a crime. The first set of two models considers these consequences using all inmates who did not experience restrictive housing as the reference cell; the second set uses inmates who were sanctioned during their incarceration but not placed in restrictive housing as the reference cell.

The results from Model 1, which does not include any controls, correspond directly with the results shown in Figure 1. The pre-admission means show that there are marked differences in conviction rates two-to-three years before admission—with those who experience restrictive housing having much higher (72%) conviction risk that those who did not (56%)—which is exactly the type of pre-existing differences that the model is designed to account for. From before to after incarceration, the conviction risk decreases substantially for both groups, yet the relative change is much larger for those never placed in restrictive housing. The estimate of interest—the difference in the relative changes—shows that those who experienced restrictive housing suffered a 16.5 percent lower decrease in conviction risk than they would have, had they not been held in restrictive housing; a difference that is significant at the .001 level. After adjusting for covariates in Model 2, the association is altered little, which is consistent with the difference-in-difference method adjusting for both observed and unobserved heterogeneity.

[Insert Table 4 about here.]

Results from the second set of models, which uses inmates who were sanctioned for their behavior at some point but never placed in restrictive housing as the reference cell (and who had a pre-admission conviction rate of 64%), follow in Models 3 and 4. Model 3 shows the difference between these groups in the risk of being convicted without controlling for background characteristics. Results imply 11.5 percent less relative decline in conviction risk for inmates who were ever placed in restrictive housing relative to the change for inmates who experienced other disciplinary actions. Again, this difference is significant at the .001 level. Thus, results from the second set of models provide further evidence that restrictive housing increases the risk of new convictions using a different, and likely stronger, control group.

Effects on Employment

Table 5 presents estimates from models that proceed in a parallel fashion to those shown in Table 4 but for a different outcome: cumulative share ever employed. Consistent with what was shown in Figure 1, there are substantial differences in mean employment across the comparison groups already before incarceration: around 68 percent for the two groups that did not experience restrictive housing and 61 percent for the group that did experience restrictive housing.

Results from Model 1, which does not control for background characteristics, show that from before to after incarceration, employment decreases substantially for both groups, yet the relative decline is much larger for the treatment group (a one-third decline in employment from a

baseline of 61 percent ever employed before incarceration). The difference in the relative changes shows that those who experienced restrictive housing experienced a 14.4 percent larger decrease in employment than they would have, had they not been placed. The difference is, again, significant at the .001 level. After adjusting for covariates in Model 2, the difference in the relative changes diminished to 9.6 percent, yet is still substantial and significant at the .001 level.

[Insert Table 5 about here.]

The final set of models shown in Table 5, which use inmates who were sanctioned for their behavior but not placed in restrictive housing as a referent, finds similar, if somewhat smaller, effects on employment. This is unsurprising since limiting the sample to inmates who experienced sanctions likely significantly diminishes unobserved heterogeneity. In Model 3, which does not control for background characteristics, inmates who were placed in restrictive housing experienced an 11.6 percent stronger relative decline in employment than those who were sanctioned but not placed in restrictive housing. Adjusting for controls cuts this estimate to 6.9 percent, a difference that is also statistically significant at the .001 level and substantial. Thus, all results for employment provide support for the existence of substantial and relatively long-term consequences of being placed in restrictive housing for the formerly incarcerated.

Supplementary Analyses

Pretrial Detention (Tables A1-A2). Results from robustness checks considering pretrial detention status show two things. First, having experienced pretrial detention but no restrictive housing leads to worse outcomes than among those where neither detained pretrial nor placed in restrictive housing. Second, results for inmates who experienced the treatment are stronger than for those who only experienced pretrial detention, yet do not differ by pretrial detention status.

Punishment Cell (Table A3). When we limit our analyses only to those placed in restrictive housing for infractions which get them sent to punishment cell, the results hold. In fact, all results for placement in punishment cell are quite similar to the main results, which is unsurprising since most cases of restrictive housing in Denmark are in a punishment cell.

Mental Health (Table A4). In support of the claim that the effects of restrictive housing could run through strained mental health, results from supplementary analyses document an increase in contact with the mental healthcare system outside the prisons among inmates who experienced restrictive housing. During the years before incarceration, around 11 percent of all comparison groups experienced contact with the mental healthcare system. Yet through to three years after release from prison, this share increased more for those who had been detained in restrictive housing than among those not. When comparing to all inmates who did not experience the treatment, the difference in the relative increases was 7-9 percent. When comparing to inmates who experienced other disciplinary actions, it was smaller, around 3 percent. The latter result is less precisely estimated and only reaches significance at the .05 level when controlling

for background traits. Yet because all estimates point in the direction of an increase in the use of the mental healthcare system, and because data limitations render these estimates lower bounds, strained mental health is likely to be a mechanism driving the effects of having been placed in restrictive housing. Future studies should, however, develop this line of research more.

Other Crime Outcomes (Table S8 in the online supplementary appendix). Inmates placed in restrictive housing are more likely to be convicted of a violent crime, more likely to be convicted of possession of drugs, and more likely to experience re-arrest. For DUI, the pattern of results is similar, yet estimates for this crime type are very imprecisely measured.

Other Labor Market Outcomes (Table S9 in the online supplementary appendix). High base levels (>65 percent) of dependence on social assistance before incarceration signal that prisoners are not well-integrated into the labor market in Denmark. And although the base levels differ little among those who experienced the treatment and those who did not, results still document that restrictive housing affects how likely inmates are of being dependent on social benefits following release. For cumulative earnings, however, we observe strong evidence *against* a damaging effect of restrictive housing placement. Here the difference in the relative changes is small, changes sign across the model space, and never reaches statistical significance.

Duration (Tables S10-S13 in the online supplementary appendix). In our supplementary analyses of effect heterogeneity across duration of placement in restrictive housing, we distinguish between maximum consecutive days in (Tables S10-S11) and total days in (Tables

S12-S13). Two conclusions arise from these analyses. First, those who spend longer in restrictive housing had worse outcomes before incarceration, implying selection of more disadvantaged inmates into longer bouts of restrictive housing. Second, whereas effects for those who spend 1-2 and 3-7 days in are strong, effects are the strongest for those who spend 7+ days in.

DISCUSSION AND CONCLUSION

Discourse around restrictive housing tends to focus on three issues: (1) the mental health effects of being placed in restrictive housing; (2) the effects of restrictive housing on the safety of correctional staff and other inmates; and (3) the ethics of restrictive housing. Evidence on the first two issues, which are the issues we feel most comfortable weighing in on given our training, is inconclusive, especially for the second issue. Although research shows that being placed in restrictive housing is linked with many poor mental health outcomes (e.g., Andersen et al. 1996; Haney and Lynch 1997; Kaba et al. 2014; Kupers 1999; Rhodes 2004), the research designs used in these studies are rarely well-suited for testing causal effects (e.g., Kapoor and Trestman 2016; Labrecque 2016; Morgan et al. 2016), indicating that there is suggestive but not definitive evidence regarding the mental health effects of restrictive housing placement. When combined with research providing little evidence regarding the benefits of restrictive housing for the future violence of those placed in it (e.g., Morris 2016) and overall levels of violence in prison systems

(e.g., Briggs et al. 2003), research in this area could be seen as tentatively showing harmful effects on mental health, with less evidence regarding prison safety or inmate behavior.

While research on the costs and benefits of restrictive housing during the confinement period is limited, research on the long-term consequences of exposure to restrictive housing is virtually non-existent. And, indeed, with the exception of a number of studies that consider long-run effects on mental health, almost no research has considered how having been in restrictive housing affects post-release adjustment for the formerly incarcerated. In this manuscript, we provide a first step in that direction by assessing the long-term consequences of being placed in restrictive housing on two core indicators of successful reentry that are also of the utmost importance for families and communities: being convicted of a new crime and being employed. In order to do so, we used a dataset that includes not only the full suite of information available on the Danish population in their registry data, but also detailed data on the in-prison experiences, including both restrictive housing and other sanctions, of all Danish inmates.

Results from analyses of these data provide support for three conclusions. First, Danish inmates in closed prisons experience similar rates of placement in restrictive housing to those experienced in American prisons. Although this is not a completely novel finding since it is well-known that Danish prisons use restrictive housing at quite high rates (e.g., Reiter et al. 2016), it is worth noting since it shows that the Danish criminal justice system is not always so lenient.

Our second and third conclusions are driven by our regression analyses, which rigorously evaluated the long-term effects of being placed in restrictive housing on the probability of being convicted of a new crime and being employed using a difference-in-differences (DID) approach. Second, for new convictions—the core indicator of successful reentry for most criminologists—the results show that individuals placed in restrictive housing experience a 12.8% to 16.6% lower decrease in the risk of being convicted of a new crime than do those not placed in restrictive housing. Third, for being employed—again, a core indicator of successful reentry—the results suggest damaging effects of placement in restrictive housing. Individuals placed in restrictive housing experienced a 6.9% to 9.6% greater decrease in employment than individuals not placed in restrictive housing. Both of these effects are substantial and indicate that restrictive housing has broad damaging effects on the formerly incarcerated. And because these results also hold up to a host of robustness checks—to limiting the analyses to punishment cell detention and to limiting the analyses to those who did not experience pretrial detention, to name the two core robustness checks—these substantial effects also appear to be representing real causal effects.

Of course, the current study is not without limitations. We focus here on three, two of which have to do with internal validity and one of which has to do with external validity. Of course, the most basic threat to internal validity with a DID model is a violation of the common trends assumption. Although we see no visible evidence of this in our data, the reality is that the DID model is always vulnerable to time-varying changes in the treatment and control groups that

affect both the explanatory and dependent variables. In addition to this core threat to internal validity is the fact that we cannot fully test the degree to which declines in mental health caused by restrictive housing exposure drive poor post-release outcomes. Although both of these limitations are worthy of attention, the reality is that the internal validity of our analysis is still strong and moves in the direction suggested by recent reviews (e.g., Kapoor and Trestman 2016; Labrecque 2016). In addition to these concerns about internal validity, it is also unclear how, if at all, these results generalize to other contexts—especially the US context. This is a core limitation because it is left to the reader to guess if these effects can be translated to the literally hundreds of thousands of US inmates who are placed in restrictive housing annually. Although the Danish and American systems place inmates in restrictive housing at similar rates, the criminal justice systems—and societies, for that matter—differ so much in other ways that it remains unclear how meaningful these results are outside of Denmark (e.g., Pratt 2008; Walmsley 2016).

Limitations aside, we nonetheless see this study as making an important theoretical contribution beyond expanding our knowledge of how restrictive housing affects inmates. Much research on the consequences of mass imprisonment has focused on estimating the average effects, yet a new vein of research calls for identifying sources of heterogeneity in the effects of incarceration. Some of this research focuses on individual-level factors as important sources of effect heterogeneity (e.g., Turney 2017), yet other research calls for considering not how individual factors introduce effect heterogeneity but on how the conditions of confinement

instead do so (Wildeman and Muller 2012; Wildeman and Wang 2017). By providing strong evidence that one condition of confinement—being placed in restrictive housing—does moderate the effects of incarceration, we not only show that conditions of confinement do indeed exert powerful effects on former inmates but also highlight ways in which research on the consequences of incarceration could take a cue from policing research and focus as much on the character of criminal justice contact as the fact of criminal justice contact (e.g., Tyler 2006).

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Table 1. Restrictive Housing in the United States, 2011-2012, and Denmark, 2006-2013.

	Denmark			United States	
	Closed Prisons	Open Prisons	Jails	Prisons	Jails
In Restrictive Housing Last Night (%)					
Yes	0.6	0.6	0.6	1.9	2.2
No	99.4	99.4	99.4	95.6	97.3
Don't Know	0	0	0	2.5	0.5
In Restrictive Housing in Last 12 Months, by Time Since Admission (%)					
All	20.3	11.4	17.07	18.1	17.4
<1 Month	3.2	1.4	1.6	8.4	8
2-3 Months	9.4	6.5	7.0	11.6	14.3
4-5 Months	17.8	14.8	16.1	13.5	19.6
6-8 Months	25.1	21.6	24.3	19.8	27.3
9-11 Months	32.5	28.5	31.6	22	32.2
12+ Months	24.0	16.4	21.7	20.4	35.4
Average Exposure in the Last 12 Months (Months)	7.8	5.1	6.9	8.6	3.5
In Restrictive Housing in Last 12 Months, by Offense Type (%)					
Violent Sex Offense	3.1	4.5	6.2	15.5	20.5
Other Violent Offense	20.7	12.4	19.5	24.6	27.7
Property Offense	22.9	12.5	17.4	19.1	18
Drug Offense	13.8	11.1	16.6	14.4	15.6
Other Offense	24.8	10.5	15.6	15.2	13.5
Number of Days in Restrictive Housing in Last 12 Months (%)					
0	79.7	88.6	82.9	79.3	82.2
≤1	0.9	0.5	0.5	0.6	1.6
2-6	13.7	8.1	11.8	2.2	4
7-13	4.9	2.5	4.3	2.4	3.1
14-29	0.6	0.2	0.2	3.4	3.1
30+	0.2	0.2	0.2	2.6	5.4
Don't Know	0	0	0	2.6	0.5

NOTE: The Denmark data are a repeated daily cross-sectional dataset made from all incarceration spells in 2006-2013 (N = 7,684,550 person-days). All spells were followed until terminated or, for those extending beyond this date, to December 31st, 2013. The figures in this table thus report the shares of all person-days incarcerated during this time window that were served in restrictive housing. Three Danish prisons have both closed and open prison wings, yet because the capacity in these open wings exceeds the capacity in the closed wings in each of the three prisons, all incarceration spells from these prisons are recorded in the open prison category. The estimates from the United States are based on data from the National Inmate Survey (N = 91,177) from 2011-2012 and have been published in previous research (Beck 2015).

Table 2. Descriptive Statistics (Means and Standard Deviations) of Control Variables, by Restricting Housing Status. Danish Correctional Facilities, 2006-2013.

	Restrictive Housing		No Restrictive Housing		No Infractions		Other Sanctions	
Variable	M	SD	M	SD	M	SD	M	SD
Born Before 1971	.156	.363	.336***	.472	.395***	.489	.236***	.424
Sentence Length (Months)	15.545	16.573	6.511***	11.094	4.691***	9.058	9.639***	13.346
Time Served (Months)	11.311	13.273	3.576***	6.647	2.342***	4.474	5.696***	8.867
Assault	.365	.481	.350	.477	.325***	.468	.394***	.489
Sex Crime	.017	.127	.037***	.188	.045***	.208	.023*	.149
Other Violent Crime	.032	.176	.020***	.139	.018***	.133	.023***	.149
Arson	.009	.094	.006*	.077	.006*	.074	.006	.080
Robbery	.161	.368	.041***	.198	.023***	.151	.071***	.258
Theft	.163	.369	.126***	.331	.112***	.315	.149*	.356
Other Property Crime	.020	.141	.031***	.174	.033***	.180	.027*	.163
Drug Crime	.091	.287	.055***	.229	.041***	.197	.081*	.272
Traffic Offense	.031	.173	.163***	.369	.206***	.404	.090***	.287
Other Crime	.112	.315	.171***	.376	.192***	.394	.135***	.342
Total Times in Restrictive Housing	1.603	1.319	-	-	-	-	-	-
Total Days in Restrictive Housing	9.976	2.998	-	-	-	-	-	-
Jail	.336	.473	.190***	.392	.199***	.399	.175***	.380
Open Prison	.535	.499	.755***	.430	.753***	.431	.757***	.429
Closed Prison	.128	.335	.055***	.228	.048***	.213	.068***	.252
Age at Admission	28.251	8.797	33.551***	10.653	35.105***	10.828	30.881***	9.785
Female	.057	.232	.053	.223	.057	.233	.045**	.207
Has Children	.159	.366	.169	.375	.175*	.380	.158	.365
Ethnic Minority Background	.294	.456	.192***	.394	.167***	.373	.235***	.424
Years of Education	8.855	3.203	9.577***	3.325	9.737***	3.367	9.302***	3.233
Missing in Education Register	.086	.281	.070***	.254	.067***	.250	.074*	.262
Recent Mental Health Care Contact	.057	.231	.053	.224	.054	.226	.051	.219
Ever Mental Health Care Contact	.236	.424	.204***	.403	.203***	.402	.206***	.404
Previously Incarcerated	.556	.497	.527***	.499	.523***	.499	.534*	.499
No Siblings	.147	.355	.192***	.394	.205***	.404	.169**	.375
One Sibling	.313	.464	.312	.463	.311	.463	.314	.464
Two Siblings	.262	.440	.262	.439	.260	.438	.265	.441
Three or More Siblings	.278	.448	.234***	.423	.224***	.417	.252**	.434
Parents Married at Age 15	.282	.450	.234***	.424	.216***	.411	.266	.442
Father Ever Convicted	.449	.497	.346***	.476	.317***	.465	.397***	.489
Father Ever Incarcerated	.255	.436	.205***	.404	.191***	.393	.230**	.421
Father Missing in Register	.085	.278	.118***	.322	.130***	.336	.097*	.296

Mother's Age at Birth	21.255	10.129	19.210***	11.361	18.500***	11.685	20.431***	10.672
Mother Missing in Register	.050	.218	.086***	.280	.098***	.297	.066***	.247
Missing Mother's Age at Birth	.151	.358	.228***	.420	.255***	.436	.182***	.386
Ever Employed, 2-3 Years Before	.614	.487	.685***	.465	.691***	.462	.674***	.469
Ever Employed, 2-3 Years After	.393	.488	.541***	.498	.556***	.497	.515***	.500
Cumulative Earnings, 2-3 Years Before	8.962	16.968	17.223***	25.991	19.038***	27.147	14.102***	23.551
Cumulative Earnings, 2-3 Years After	8.104	18.275	15.772***	26.675	17.383***	28.042	13.002***	23.895
Dependence on Social Ass. 2-3 Years Before	.618	.434	.644***	.437	.650***	.438	.634*	.435
Dependence on Social Ass. 3 Years After	.775	.369	.716***	.407	.707***	.413	.733***	.395
Convicted, 2-3 Years Before	.720	.449	.565***	.496	.519***	.500	.644***	.479
Convicted, 3 Years After	.616	.486	.366***	.482	.314***	.464	.457***	.498
Violence, 2-3 Years Before	.305	.460	.226***	.418	.203***	.402	.264***	.441
Violence, 3 Years After	.243	.429	.131***	.337	.111***	.314	.165***	.372
Drug Offense, 2-3 Years Before	.333	.471	.194***	.396	.165***	.371	.246***	.431
Drug Offense, 3 Years After	.392	.488	.203***	.402	.159***	.366	.278***	.448
DUI Offense, 2-3 Years Before	.070	.254	.125***	.331	.143***	.350	.094***	.292
DUI Offense, 3 Years After	.092	.289	.103*	.303	.104*	.305	.100	.300
Arrested, 2-3 Years Before	.515	.500	.331***	.471	.292***	.455	.398***	.490
Arrested, 3 Years After	.468	.499	.272***	.445	.237***	.425	.332***	.471
Number of Observations	4218		32214		19679		12535	

NOTE: Dummy variables for admission year and release are removed from this table in the interest of conserving space but available in Table S1. Observations are weighted by 1/frequency. The Other Sanctions group is a subgroup of the No Restrictive Housing group. Significance stars refer to Student's T-tests (two-tailed tests) of the means and standard deviations among the cell in question and the corresponding cell in the Restrictive Housing column. Cumulative Earnings are measured in 1,000s of 2010 PPP-adjusted USD. Missing mothers are recorded with 0 on Mother's Age at Birth, and we measure the association between this group and the outcome using the dummy indicator Missing Mother's Age at Birth. The low mean for Mother's Age at Birth reflects this construction.
ABBREVIATIONS: M = Mean; SD = Standard Deviation; PPP = Purchasing Power Parity.
SOURCE: Own calculations based on data from Statistics Denmark
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table 3. Distribution of Infractions by Sanction Type, Percent, 2006-2013

Infraction	Restrictive Housing	Other Sanction	Total
Refused drug test	5.8	10.9	10.1
Refused employment	0.9	1.7	1.6
Possessing drugs/weapons	13.2	20.5	19.3
Opposed staff instructions	5.9	6.4	6.3
Attempted escape	1.3	0.5	0.6
Possessing illegal goods	41.6	34.2	35.4
Failure to return from leave	0.5	0.6	0.6
Other infractions during leave	2.4	4.7	4.3
Violating the penal code	13.2	2.7	4.4
Violating facility-specific rules	15.3	17.8	17.4
Percentage of sanction type	100.0	100.0	100.0
Percentage of all infractions	16.2	83.8	100.0

NOTE: From 2006 to 2013 the official total number of restrictive housing spells was N = 21,062 and the number of other sanctions was N = 109,326. These total numbers include infractions committed during pretrial detention.
SOURCE: Danish Prison and Probation Service (2007-2014).

Table 4. Percent Change in Cumulative Criminal Conviction Risk After Three Years. Denmark, 2006-2013.

Sample	Restrictive Housing vs. No Restrictive Housing			Restrictive Housing vs. Other Disciplinary Action		
Relative Change from Before to After	Pre–Admission Mean	Model 1	Model 2	Pre–Admission Mean	Model 3	Model 4
No Restrictive Housing	.565	–.265***	–.301***	.644	–.214***	–.264***
(Baseline Difference)		(.005)	(.023)		(.007)	(.019)
Experienced Restrictive Housing	.720	–.100***	–.136***	.720	–.100***	–.136***
(Treatment Group Difference)		(.010)	(.017)		(.010)	(.017)
Difference in Relative Changes		.165***	.166***		.115***	.128***
(Difference–in–differences)		(.011)	(.020)		(.013)	(.018)
Control Variables			X			X
Number of Observations	36432	36432	36432	16753	16753	16753

NOTE: Table shows the percent change in cumulative conviction risk (from 2-3 year before admission to 3 years after release) for those who experienced restrictive housing relative to those who did not (Models 1 and 2) and relative to those who experienced other disciplinary actions (Models 3 and 4). Estimates are from standard difference-in-differences models estimated using OLS. Standard errors, clustered at the individual level and reported in parentheses, of the percent changes reported in this table are obtained using the Delta Method (Oehlert 1992). Table S3 in the online supplementary appendix reports the full set of parameter estimates.

SOURCE: Own calculations based on data from Statistics Denmark

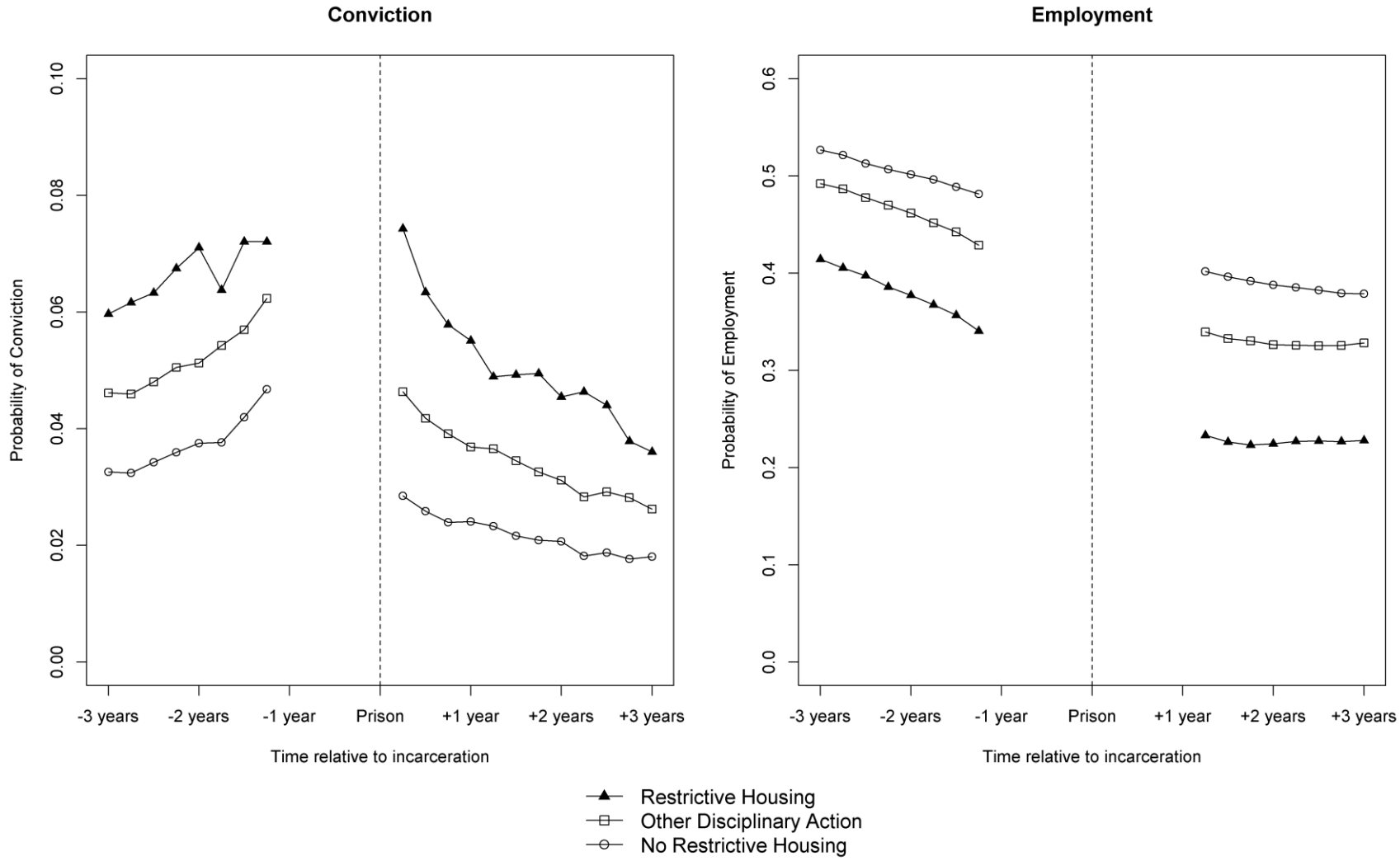
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table 5. Percent Change in Cumulative Employment After Three Years. Denmark, 2006-2013.

Sample	Restrictive Housing vs. No Restrictive Housing			Restrictive Housing vs. Other Disciplinary Action		
Relative Change from Before to After	Pre–Admission Mean	Model 1	Model 2	Pre–Admission Mean	Model 3	Model 4
No Restrictive Housing	.685	–.230***	–.197***	.674	–.258***	–.207***
(Baseline Difference)		(.004)	(.011)		(.007)	(.012)
Experienced Restrictive Housing	.614	–.375***	–.293***	.614	–.375***	–.276***
(Treatment Group Difference)		(.013)	(.020)		(.013)	(.018)
Difference in Relative Changes		–.144***	–.096***		–.116***	–.069***
(Difference–in–differences)		(.014)	(.013)		(.015)	(.012)
Control Variables			X			X
Number of Observations	36432	36432	36432	16753	16753	16753

NOTE: Table shows the percent change in cumulative proportion ever employed during the period (from 2-3 year before admission to 2-3 years after release), defined as having had any income from work, for those who experienced restrictive housing relative to those who did not (Models 1 and 2) and relative to those who experienced other disciplinary actions (Models 3 and 4). Estimates are from standard difference-in-differences models estimated using OLS. Standard errors, clustered at the individual level and reported in parentheses, of the percent changes reported in this table are obtained using the Delta Method (Oehlert 1992). Table S4 in the online supplementary appendix reports the full set of parameter estimates.
SOURCE: Own calculations based on data from Statistics Denmark
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Fig. 1
Probability of Conviction and Employment Before and After Incarceration. Denmark, 2006-2013 (*N* = 36,432).



SOURCE: Own calculations based on data from Statistics Denmark.

Table A1. Percent Change in Cumulative Criminal Conviction Risk After Three Years, by Pretrial Detention Status. Denmark, 2006-2013.

Sample	Restrictive Housing vs. No Restrictive Housing			Restrictive Housing vs. Other Disciplinary Action		
Relative Change from Before to After by Pretrial Detention Status	Pre-Admission Mean	Model 1	Model 2	Pre-Admission Mean	Model 3	Model 4
No Restrictive and No Pretrial Detention	.555	–.313***	–.337***	.645	–.248***	–.292***
(Baseline Difference)		(.008)	(.027)		(.013)	(.025)
No Restrictive but Pretrial Detention	.645	–.234***	–.274***	.645	–.198***	–.244***
		(.006)	(.021)		(.008)	(.019)
Difference in Relative Changes		.079***	.063***		.050**	.048*
		(.011)	(.013)		(.016)	(.019)
Restrictive but No Pretrial Detention	.743	–.118***	–.157***	.743	–.118***	–.156***
		(.022)	(.030)		(.022)	(.030)
Difference in Relative Changes		.194***	.181***		.130***	.136***
		(.023)	(.033)		(.025)	(.033)
Restrictive and Pretrial Detention	.783	–.093***	–.126***	.783	–.093***	–.126***
		(.012)	(.018)		(.012)	(.018)
Difference in Relative Changes		.220***	.211***		.155***	.166***
		(.015)	(.025)		(.018)	(.025)
Control Variables			X			X
Number of Observations	36432	36432	36432	16753	16753	16753

NOTE: Table shows the percent change in cumulative conviction risk (from 2-3 year before admission to 3 years after release) for those who experienced restrictive housing relative to those who did not (Models 1 and 2) and relative to those who experienced other disciplinary actions (Models 3 and 4), by pretrial detention status. Estimates are from standard difference-in-differences models estimated using OLS and standard errors, clustered at the individual level and reported in parentheses, of the percent changes reported in this table are obtained using the Delta Method (Oehlert 1992).
SOURCE: Own calculations based on data from Statistics Denmark
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table A2. Percent Change in Cumulative Employment After Three Years, by Pretrial Detention Status. Denmark, 2006-2013.

Sample	Restrictive Housing vs. No Restrictive Housing			Restrictive Housing vs. Other Disciplinary Action		
Relative Change from Before to After by Pretrial Detention Status	Pre-Admission Mean	Model 1	Model 2	Pre-Admission Mean	Model 3	Model 4
No Restrictive and No Pretrial Detention	.745	–.199***	–.188***	.735	–.228***	–.207***
(Baseline Difference)		(.006)	(.011)		(.010)	(.014)
No Restrictive but Pretrial Detention	.582	–.260***	–.205***	.582	–.278***	–.211***
		(.007)	(.013)		(.009)	(.013)
Difference in Relative Changes		–.061***	–.018*		–.050***	–.004
		(.009)	(.008)		(.014)	(.012)
Restrictive but No Pretrial Detention	.631	–.353***	–.303***	.631	–.353***	–.288***
		(.023)	(.027)		(.023)	(.024)
Difference in Relative Changes		–.154***	–.115***		–.124***	–.080***
		(.023)	(.022)		(.025)	(.022)
Restrictive and Pretrial Detention	.531	–.384***	–.293***	.531	–.384***	–.277***
		(.016)	(.022)		(.016)	(.020)
Difference in Relative Changes		–.186***	–.106***		–.156***	–.070***
		(.017)	(.016)		(.019)	(.016)
Control Variables			X			X
Number of Observations	36432	36432	36432	16753	16753	16753

NOTE: Table shows the percent change in cumulative proportion ever employed (from 2-3 year before admission to 2-3 years after release), defined as having had any income from work, for those who experienced restrictive housing relative to those who did not (Models 1 and 2) and relative to those who experienced other disciplinary actions (Models 3 and 4), by pretrial detention status. Estimates are from standard difference-in-differences models estimated using OLS and standard errors, clustered at the individual level and reported in parentheses, of the percent changes reported in this table are obtained using the Delta Method (Oehlert 1992).

SOURCE: Own calculations based on data from Statistics Denmark

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table A3. Percent Change in Cumulative Criminal Conviction Risk and Employment After Three Years. Denmark, 2006-2013. Restrictive Housing in Punishment Cell.

Sample	Restrictive Housing vs. No Restrictive Housing			Restrictive Housing vs. Other Disciplinary Action		
Relative Change from Before to After	Pre-Admission Mean	Model 1	Model 2	Pre-Admission Mean	Model 3	Model 4
Criminal Conviction						
No Punishment Cell	.615	–.255***	–.293***	.700	–.200***	–.252***
(Baseline Difference)		(.005)	(.022)		(.007)	(.019)
Experienced Punishment Cell	.769	–.090***	–.129***	.769	–.090***	–.128***
(Treatment Group Difference)		(.014)	(.021)		(.014)	(.021)
Difference in Relative Changes		.164***	.165***		.110***	.125***
(Difference-in-differences)		(.014)	(.023)		(.015)	(.022)
Employment						
No Punishment Cell	.643	–.232***	–.197***	.619	–.260***	–.204***
(Baseline Difference)		(.004)	(.011)		(.007)	(.012)
Experienced Punishment Cell	.592	–.435***	–.336***	.592	–.435***	–.320***
(Treatment Group Difference)		(.016)	(.022)		(.016)	(.020)
Difference in Relative Changes		–.203***	–.139***		–.175***	–.115***
(Difference-in-differences)		(.016)	(.015)		(.017)	(.014)
Control Variables			X			X
Number of Observations	36432	36432	36432	16753	16753	16753

NOTE: Table shows the percent change in cumulative conviction risk (from 2-3 year before admission to 3 years after release) and cumulative employment (from 2-3 years before admission to 2-3 years after release), defined as having had any income from work, for those who experienced restrictive housing in punishment cell relative to those who did not (Models 1 and 2) and relative to those who experienced other disciplinary actions (Models 3 and 4). Estimates are from standard difference-in-differences models estimated using OLS. Standard errors, clustered at the individual level and reported in parentheses, of the percent changes reported in this table are obtained using the Delta Method (Oehlert 1992).

SOURCE: Own calculations based on data from Statistics Denmark

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).

Table A4. Percent Change in Cumulative Risk of Contact with the Mental Health Care System After Three Years. Denmark, 2006-2013.

Sample	Restrictive Housing vs. No Restrictive Housing			Restrictive Housing vs. Other Disciplinary Action		
Relative Change from Before to After	Pre-Admission Mean	Model 1	Model 2	Pre-Admission Mean	Model 3	Model 4
No Restrictive Housing	.108	.018**	.011*	.112	.053***	.020***
(Baseline Difference)		(.006)	(.004)		(.012)	(.005)
Experienced Restrictive Housing	.121	.110***	.080**	.121	.110***	.047***
(Treatment Group Difference)		(.027)	(.027)		(.027)	(.013)
Difference in Relative Changes		.092***	.069**		.057	.027*
(Difference-in-differences)		(.027)	(.025)		(.029)	(.013)
Control Variables			X			X
Number of Observations	36432	36432	36432	16753	16753	16753

NOTE: Table shows the percent change in risk of contact with the mental health care system (from 2-3 year before admission to 3 years after release) for those who experienced restrictive housing relative to those who did not (Models 1 and 2) and relative to those who experienced other disciplinary actions (Models 3 and 4). Estimates are from standard difference-in-differences models estimated using OLS. Standard errors, clustered at the individual level and reported in parentheses, of the percent changes reported in this table are obtained using the Delta Method (Oehlert 1992).
SOURCE: Own calculations based on data from Statistics Denmark
* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests).