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HOW DOES VISITATION AFFECT INCARCERATED PERSONS AND THEIR FAMILIES?

Estimates Using Exogenous Variation in Visits Driven by Distance Between Home and Prison

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How Does Visitation Affect Incarcerated Persons and Their Families? Estimates Using Exogenous Variation in Visits Driven by Distance Between Home and Prison

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Abstract

Tens of millions of people in the world are incarcerated, which may have negative consequences for both them and their families. Visitation may mitigate the negative consequences, but there is little causally identified evidence on its efficacy. To generate plausibly causal estimates, we utilize the fact that inmates in Denmark are assigned to the prison with open capacity that is closest to their home. The distance-driven variation in visitation, coupled with Danish registry data, allows us to measure the effects of visitation on inmates and their families. We find little evidence that visitation has much effect on incarcerated individuals or on their family members.

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I. Introduction

Each year, tens of millions of families across the world are forcibly separated by penal systems. According to the World Prison Population List (Walmsley 2018), almost 11 million people were behind bars on any given day in 2018—2.1 million of whom are in the US alone. Considering these high numbers, the cycle of family separation through incarceration is a pressing problem worldwide. Most research on the consequences of incarceration has focused on consequences for the adults who experience incarceration and their children. In general, this research has found that incarceration has harmful consequences for inmates and their children (for relevant work in economics, see especially Aizer and Doyle 2015, Bhuller et al. 2020, and Dobbie et al. 2019; for broader reviews of these literatures, see Kirk and Wakefield 2018, and Wildeman and Wang 2017). However, to date little research has considered how conditions of confinement are associated with inmate or family outcomes; very little of this research uses a strong identification strategy (see the review of Wildeman, Fitzpatrick, and Goldman 2018).

In this article, we provide a rigorous test of how one widely-touted (and theoretically compelling) method for decreasing the detrimental effects of incarceration for inmates and their families, visitation, is causally linked with the outcomes of inmates and their families. For incarcerated people, visitation may have positive effects if it: (1) eases the pains of incarceration by maintaining social bonds to family members (e.g., Cochran and Mears 2013); (2) provides an incentive for good behavior during incarceration (e.g., Siennick, Mears, and Bales 2013); and (3) provides the means to obtain "luxury" goods in prisons (e.g., Comfort 2008). Each of these three mechanisms could lead to decreases in criminal recidivism (e.g., Bales and Mears 2008). Family members are also expected to benefit from visitation.

Yet there are also reasons to expect that visitation would have detrimental effects on incarcerated individuals and their families. Visitation can: (1) remind inmates of their

homesickness or of strained family relationships (e.g., Tasca, Mulvey, and Rodriguez 2016); (2) make them relive past disappointments (e.g., Swanson, Lee, Sansone, and Tatum 2013); (3) stress them out because of the punitive environment that surrounds visitation in prisons (e.g., Moran, Hutton, Dixon, and Disney 2017); and (4) be associated with an increase in serious disciplinary infractions (Casey-Acevedo, Bakken, and Karle 2004). For the visiting family, visitation has been shown to be stressful and costly in terms of time and money required for visitation, and the punitive environment around visitation can lead to "secondary prisonization" (Comfort 2008).

To date, research has been inconclusive about the relationship between visitation and inmate and family outcomes. The contradictory research findings regarding the effects of visitation could arise from at least four sources. First, effects of visitation could be heterogeneous across inmate and family profiles (see Poehlmann, Dallaire, Loper, and Shear 2010 for a review). Second, effects could depend on context, as research is from different areas and often arrives at differing conclusions.[1] Third, effects could vary by data type, as research relies on a wide range of types of data including data obtained from (1) unstructured or semi-structured qualitative interviews (e.g., Comfort 2008), (2) structured qualitative interviews (e.g., Turanovic and Tasco 2017), (3) administrative data (e.g., Bales & Mears 2008), (4) survey data (e.g., Brunton-Smith and McCarthy 2017), (5) mixed data sources (e.g., Brown and Bloom 2009), and (6) teacher or caregiver reports (e.g., Block and Potthast 2001; Dallaire, Ciccone, and Wilson 2010). Finally, and perhaps most importantly, the uncertainty about the effects of visitation on inmates and their families could also arise from a lack of research designs capable of isolating the effects of visitation (see Cochran 2019 for a discussion). Almost all existing quantitative studies are forced

[1] For example, studies have taken place in Arizona (Turanovic and Tasco 2017), California (Comfort 2008), and Florida (Bales and Mears 2008) in the US, and the United Kingdom (Moran, Hutton, Dixon, and Disney 2017).

to rely on statistical conditioning for identifying the effects of visitation. This strategy causes concern about unobserved differences between inmates who receive differing amounts of visitation. Even when one has access to highly detailed data, such as administrative data from Florida (Bales and Mears 2008; Cochran 2014), only being able to adjust for observed confounders may cause significant concern about unobserved endogeneity.

Only two existing studies move beyond statistical conditioning to identify the causal effect of visitation on inmate outcomes. Cochran et al. (2020) use a feature of the Florida corrections system—assignment to prisons based on open capacity—as a source of exogenous variation in how far from home newly admitted prisoners are sent. In the study, prisoners who are assigned to facilities further away from home receive fewer visits, yet this association fails to produce clear effects on the one outcome they consider, recidivism, even across different inmate types. Lee (2019) uses a similar identification strategy coupled with administrative data from Iowa correctional facilities. In this study, too, there is no effect of visitation on recidivism.

Impressive though they are, these studies are both limited because they are able to consider only a narrow range of effects, focusing on recidivism. We build on these studies by using a wider range of outcomes for inmates and families over a longer time horizon. This is particularly useful given that the effects of visitation may not be picked up in a long-run outcome like recidivism and that there are potential effects on both inmates and their families that have yet to be explored.

To do this, we use a novel combination of Danish registry data and information on all Danish inmates and their visits (and visitors). To overcome the selection bias between visits and outcomes, we exploit variation in how far from home inmates are sent to serve their sentence. This variation stems from the prison assignment rules that require inmates to be sent to a prison

as close to home as possible. Since prisons are often overcrowded, variation in the occupancy rate at prisons will generate variation in the distance between a prisoner's home and the prison where the sentence is served. We use this plausibly exogenous variation to isolate (1) the effects of distance from home on visits and (2) the effects of visits on incarcerated individuals and their families.

We find that distance from home has consistent and substantial effects on visitation, including both whether an inmate is visited at all, how soon after incarceration an inmate is visited, and the average number of visits an inmate receives. Results are stable across inmate and visitor types, except that the effect of distance on visitation by children is smaller than for other types of visitors. Almost all of our instrumental variables estimates of the effects of visitation on inmate and family member outcomes are quite close to zero. And although some of these estimates have large 95% confidence intervals, in many instances we are still able to rule out the possibility of visitation driving positive change.

Our results strongly confirm the hypothesis that distance from home matters for visitation (Comfort 2008; Cochran et al. 2020). If society wants to secure contact between family members during imprisonment, it will require thinking carefully about prison assignment, as is already done in Denmark. However, our results also indicate that visitation is unlikely to have the largely beneficial impacts on outcomes that have previously been hypothesized. Therefore, although there are strong ethical and moral reasons to facilitate and promote visitation among families during incarceration, our results suggest that there may be at most few pragmatic benefits of doing so when it comes to the behavior of incarcerated persons or the well-being of their families. Of course, these results may apply only to the Danish context, an issue we return to in the Discussion and Conclusion given the potentially explosive nature of these results.

II. Prison Assignment and Inmate Visitation in Denmark

II.a. Overview

In criminal justice terms, Denmark is characterized by "Scandinavian Exceptionalism", which implies a low incarceration rate, comparatively short sentences, and a strong focus on resocialization (Pratt 2008). The incarceration rate is among the lowest of developed democracies, just one-tenth the US incarceration rate (63 per 100,000 in Denmark versus 655 per 100,000 in the US, Walmsley 2018). The low incarceration rate reflects generally short sentences (about 60% of the 8,000 prison sentences handed down in Denmark in 2018 were 4 months or shorter; about 10% exceeded 2 years) as well as the extensive use of noncustodial alternatives to incarceration, such as prison sentences served at home under electronic monitoring (2,200 in 2018) or in community service (3,800 sentences in 2018) in addition to more standard probation (4,300 sentences in 2018; which is also common in the US).[2] In terms of crime types, the main difference between Denmark and the US concerns violent crimes; approximately 35% of prison sentences in Denmark in 2018 concerned violent crimes (including robbery); the corresponding number from state and federal prisons in the US was 56% at year-end 2017. Drug crimes (15% Denmark, 14% US) and theft, burglary, and the like (just above 20% Denmark, 17% US) are similarly represented. Fewer than 10% of newly admitted inmates in Danish prisons in 2018 were women; this approximates the estimated gender distribution in US state and federal prisons.

The incarceration experience also differs between Denmark and the US. Inmates in Denmark do not lose any civil rights, and everyday life in a Danish prison is structured to resemble life on the outside. A normal weekday in prison therefore consists of: 8 hours of sleep;

[2] Danish Prison and Probation Service (2020) for Denmark and Carson (2020) for the US provide these estimates.

8 hours of employment, training, or education; and 8 hours of leisure time (e.g., exercise). Inmates are paid for their employment in prison, but the wage rate is much lower than it would be outside the prison. They use this money to buy groceries in an in-prison store, as they are required to plan, shop for, and cook their own meals in common kitchens. The inmates in our data are either held in low security prisons (called open prisons) or in high security prisons (called closed prisons). Open prisons have unlocked doors and are surrounded by a fence, not a wall. There is strong focus on rehabilitation in open prisons and some inmates can be allowed (if found eligible for it by the prison board) to pursue work or education outside the prison (with or without supervision by an officer). Closed prisons have locked doors and are surrounded by a concrete wall; these facilities are more consistent with what those primarily exposed to US facilities would consider a prison. The availability of resocialization initiatives is restricted to those manageable within the strict security regime of the prison; leave from closed prisons can only be granted if specific circumstances speak for it and the inmate is escorted by an officer.

II.b. Locations of Prisons in Denmark

Figure 1 maps the geographical locations of prisons in Denmark. Prisons are, not unlike in other countries, scattered throughout the country and are often located in the countryside (although there is some variation). Denmark is a small country—area-wise somewhere in between Maryland and West Virginia, and population-wise about the same size as Wisconsin—implying that distances are manageable. Still, distances and travelling times differ considerably across prisons. While someone in the capitol city of Copenhagen can travel to one prison within approximately an hour ("Horserød Fængsel"), possibly relying on public transportation to do so,

he or she should plan to spend in excess of five hours each way by car to get to the prison furthest from Copenhagen ("Kragskovhede Fængsel", the northern most prison in Figure 1).

II.c. Denmark Prison Assignment Algorithm

The Danish Prison and Probation Service assigns inmates to the prison that is closest to their home conditional on the following six criteria, which are formalized in chapter 7 of the Law on the Enforcement of Sentences ("Straffuldbyrdelsesloven").

Facility type: Sentences longer than five years are typically served in closed prisons (marked by diamonds in Figure 1); shorter sentences are typically served in open prisons (marked by circles in Figure 1; some prisons have both open and closed wings, marked by squares in Figure 1).[3] In addition to sentence length, safety precautions, gang affiliation, and high risk of escape also shape which facility type an inmate is assigned to (with higher safety needs, gang members, and those with higher risk of escape being placed in closed prisons).

Age: In an effort to shield young offenders from potential malicious influence of older and "more hardened" criminals, the Danish prison system sentences inmates of different ages differently. Those younger than 18 years of age are placed in separate institutions and are not part of our data. Inmates in the age range 18-19 years are assigned to the closed prison as close to their parents as possible. Age range 20-22 years can be assigned to any open prison yet only to one specific closed prison. And, last, inmates older than 22 years can be assigned to any prison.

Family needs: If an inmate has small children or elderly parents in need of care, such inmates may be assigned to a prison closer to home.

[3] Very short sentences, typically not exceeding one or a few months, can be served in local jails (which also hold pretrial detainees), yet we do not include data from local jails in our analyses. We do so because visitation facilities are generally poor in local jails and the local nature of jails produces very little variation in distance from home

Medical treatment needs: Drug addiction that requires treatment or other medical needs can send an inmate to specific institutions that offer suitable medical treatment.

Employment prospects: Some inmates are expected to keep and return to their employment outside the prison. Such inmates may be placed closer to their employer to ease collaboration during the imprisonment.

Prison capacity/occupancy: The last criteria of how to assign inmates to prisons in Denmark concerns prison capacity. Specifically, if a prison is full (or has high occupancy), the prison and probation service may assign an inmate to another prison. Conversations with personnel at the Danish Prison and Probation Service confirmed that the occupancy rate is indeed very important for the specific assignment of inmates to prisons.

Inmates in Denmark are not sent directly from the sentencing court to prison to serve their sentence. Instead, they are released into the community and are then informed by letter when they must meet at a designated prison to serve the sentence. A series of laws, beginning with Law no. 432 of 31 May 2000, determines when citizens of Denmark who are sentenced to imprisonment are required to be informed about their admission date. The standard is to receive notice no later than 30 days prior to admission date. Since 2000, amendments have been made to the law, all reducing the notice period from 30 to 10 days for specific crimes, including violent crimes and those primarily related to the possession of weapons.[4] As we describe in more detail in Section III, we make use of this time horizon in constructing our prison capacity instrument.

II.d. Prisoner Visitation in Denmark

[4] Table C1 in Section C (which provides additional details on the estimated occupancy rates in Danish prisons) of the online appendix summarizes each of these amendments, which affected comparatively few cases.

Visitation in prisons in Denmark is governed under chapter 9 of the Law of the Enforcement of Sentences (the following description builds on Langsted, Garde, and Greve 2011, p. 233), which states that inmates are allowed to receive visits, except if there is suspicion that a specific visit could be part of planning an attempted escape or of smuggling contraband into the prison. Inmates are entitled to at least one hour-long visit per week, but visitation is often more frequent and longer, which is also the intention of the Danish Prison and Probation Service. Visitation is required to take place in a natural and friendly atmosphere—i.e., not a punitive environment, which is why visits are seldom under surveillance in Danish prisons—and may take place either on the open grounds of the prison (mainly possible in open prisons) or in a designated visitation space. The inmate may wear his or her own clothes, and sexual relations, such as between an inmate and his or her significant other, are accepted (except when the visit is under surveillance). Closed Danish prisons have apartments designed for overnight/weekend visitation, including for families with children, and recently there has been a push towards the establishment of even more family/child-friendly visitation facilities. Given that this type of visitation setting is different than in many other countries, including the U.S., our results may not generalize as it is likely that these visitation circumstances will lead visitation to have a more positive effect on inmates and families than in more restrictive visitation settings.

Before each visit, the visitor must be approved by the prison warden, which includes a background check in the criminal registers. When arriving to the prison the visitor should expect to go through security precautions, including the screening of any items the visitor may have brought to the visit. The screening and security precautions may be downsized if the security risk is considered very low, something that is only likely to be the case in open prisons.

Studies have shown that in other contexts, the likelihood of visitation depends on socioeconomic status, race, and whether visitors come from disadvantaged neighborhoods (e.g., Cochran, Mears, Bales, and Stewart 2016). In Denmark, this heterogeneity in visitation is likely less important, as the government subsidizes transportation costs related to visitation in prison. Of course, subsidizing costs still requires formal application, which can be time consuming, and low socioeconomic status groups could also face barriers to visitation other than those related to the direct costs.

III. Administrative Data on Danish Prisons, Inmates, and their Families

III.a. Overview of the Danish Registry and Prison Data

We combine two unique data sources for this study: standard register data from Statistics Denmark and never-before used data from the Danish Prison and Probation Service on all visits in Danish prisons. In Denmark, a wide range of information on the full population is recorded using unique personal identification numbers, and the resulting "standard register data" (which are de-identified yet still linkable at the individual level) are made available for research by Statistics Denmark, the national statistical agency. A detailed description of standard register data from Denmark can be found in Andersen (2018). Data from the Danish Prison and Probation Service, which represent information from the service's administrative registration system, are recorded using the same unique identification numbers and are hence directly linkable to the standard registers.

Our combined data have many unique features. First, our data hold the full population of inmates in Danish prisons during 2004-2014. Second, we have information on the prison placement of each of these inmates. Third, because inmates are recorded using unique personal

identification numbers, we can merge data from several administrative registers into the data, enabling us to add pre-admission covariates that are key to the allocation of inmates to prisons. We also merge information on outcomes during imprisonment (disciplinary infractions, for example) and for up to two years following release. Fourth, via GPS coordinates of all Danish prisons and of the (de-identified) home addresses of all mentioned inmates, we have obtained information on road-travelling distances between the inmates' address just prior to admission and each of the prisons in Denmark (a feature that is pivotal to our research design).[5] Fifth, our data hold the full population of visits (and visitors) to the inmates in Danish prisons during the same period, including information on the relationship between the visitor and the inmate (spouse, child, parent, friend, lawyer). Sixth, because the data contain unique personal identification numbers on the visitors, we can merge the visitors to the general administrative registers to obtain outcomes of the visitors, too. Relative to existing data sources within the research field, having data with just one or a few of the features we have just described could likely be the base for a contribution to research; we have data with all six features.

Table 1 presents summary statistics of the Danish data and compares it to the data from Florida used in Bales and Mears (2008), which contain comparable information on types of visitors and therefore represent a useful comparison. The Danish data are larger (which is no surprise as they contain the full population and cover a longer time frame). But other important differences and similarities emerge. Visitation seems to be more common in Denmark, which may be caused by inmates' lawful right to visitation in Denmark, a right not afforded to inmates in the U.S. Within the 12 months prior to release, almost 60 percent of inmates in Denmark

[5] The geographical data unit at Statistics Denmark calculated these road-travelling distances based on our coding of the de-identified home addresses just prior to admission and the location of each prison. We have also conducted all of our analyses using road-traveling times instead of distances. Results are qualitatively similar to those we present here and are available from the authors upon request.

received at least one visit, the same number in the Florida data is 40 percent. Intensive margin differences also exist. Inmates in Denmark who have visits are visited, on average, as much as 25 times per sentence. In Florida, the number is less than half of that, 10. Examining visitation patterns by visitor type shows, however, that the differences in visitation are driven by "significant others", other relatives, and by friends rather than immediate family, though these patterns are not consistent across all measures. Turning to background characteristics, a higher proportion of the Florida sample is non-white, sentences are much longer, and more are convicted of drug-related crimes. These differences make sense given differences in the Danish and US criminal justice systems, as well as differences in the demographics of each area.

III.b. Variables

In this section, we describe the key variables that we use in our analyses. We provide detailed information on our measures of distance, prison occupancy rates, and prison assignment, as well as on our measures of visitation. In the interest of brevity, we only summarize the outcome variables, both outcomes of the inmates and of the visiting family members. We refer to the online technical appendix, section B, for details on the measurement of and data sources from which we obtain these variables.

Distance to Nearest Relevant Prison with Space. Because our data contain information on some of the criteria that the Danish Prison and Probation Service uses to assign inmates to prisons in Denmark, we can estimate which prisons are relevant for each inmate. We use information on facility type, sentence length, age, and the occupancy rate at the prisons. We do not have information on family needs, medical treatment needs, or employment prospects. As long as our prison capacity instrument is uncorrelated with these factors and is a strong predictor

of distance (and, hence, of visits) our instrumental variables strategy will produce unbiased estimates of the effects of visitation on inmate and family outcomes.

Two similar offenders (same crime type, sentence length, age, etc.) from the same geographic area should both be assigned to the closest relevant prison if there is space in that prison. However, if one happens to be sentenced when the prison does not have capacity for new inmates, that inmate will be sent further away from home.

Unfortunately, official records of daily capacity for prisons are not available. Instead, we make use of the available information on the number of inmates in each prison on a given day to create a proxy for the official capacity of each prison during the period over which a sentencing decision was most likely made. Important context for our creation of this proxy is the fact that although there are very few large expansions or contractions or openings or closings of Danish prisons, there are changes to the capacity of prisons driven by the addition and withdrawal of beds to cells and by the opening and closing of wings within prisons. These are often hard to track because we do not have formal reports of these changes and because they can occur on a small scale (one or two beds at a time).

We therefore define the occupancy rate in prison *j* on day *t* as $Z_{j,t}^{95} = \frac{n_{j,t}}{N_{j,t^*}^{95}}$, where $n_{j,t}$ is the number of inmates in prison *j* on day *t* and N_{j,t^*}^{95} is the 95th percentile of the number of inmates in prison *j* over the preceding time period during which sentencing most likely took place. $t^* = [t-30: t-10]$ for inmates convicted of violent crimes or crimes involving weapons and $t^* = [t-60: t-30]$ for other crimes.[6] We used the 95th percentile rather than the maximum

[6] Although we do not know the exact timing of when the Prison and Probation Service assigns inmates to prisons, it is likely to observe the occupancy rates of the prisons for a period before assigning an inmate. The inmate must be informed no later than 10 days before admission if convicted of violent and specific other crime types, such a weapons possession, and 30 days before admission if convicted of other crimes. These time limits thus serve as the

occupancy over this period to avoid concerns about outliers. If the closest relevant prison is at or above capacity, as defined from the 95th percentile of inmates within the relevant pre-admission period, this should increase the risk that an inmate will be assigned to the next-closest prison that is relevant in terms of the other criteria. This is the variation we make use of when instrumenting for visitation.

Figure 2 plots the estimated daily occupancy rates in Danish prisons over our data period. The horizontal lines at 1.00 mark our definition of when a given prison becomes less likely to receive new inmates because the prison is considered at capacity. Across the data window, most prisons operate below capacity much of the time. Yet in every prison, the occupancy rate occasionally increases to or above capacity, which is the type of fluctuation that we rely on for identifying the causal effect of distance from home.

In practice, $Z_{j,t}^{95}$ enters our definition of which relevant prison has space as a binary indicator: $space_{Q95,t,j} = \mathbb{1}(Z_{j,t}^{95} < 1)$. Multiplying this indicator by the road-travelling distances between an inmate's home address just prior to incarceration and each of the prisons produces a new vector of distances between an inmate's home address and all relevant prisons *that have space* at the time of incarceration. (Prisons at or above capacity are not viewed as relevant in this specification.) Our measure of "distance to nearest relevant prison with space" is the lowest value of the resulting vector.[7]

Assigned Prison and Distance from Home. From the incarceration register, we obtain information on where each inmate was sent to serve his or her sentence. Again, because we have road-travelling distances between each inmates' home address just prior to incarceration and

latest day the prison assignment could have taken place, which is why we use a period leading up to these time limits as the relevant occupancy window in the prison assignment process.

[7] For additional details on occupancy rates and the definition of distance to nearest relevant prison that has space, see the online technical appendix, sections C and D.

each of the prisons, we can define distance from home to the assigned prison. We focus only on the first assignment of the inmate. For inmates who were detained pretrial in a local jail, we focus on the first prison that the inmate was assigned to post conviction.

Visitation. Each visit in the original data is recorded using a unique visit ID. For each visit ID, the data hold information on the personal identification numbers on everyone who attended the visit. The visitation data also contain the date of the visit.

To measure visitation, we define *number of visits per month.* We here count the number of unique visit IDs an inmate occurs on and divide by the length of incarceration. We thus focus on number of visits that have taken place, not the number of visitors (since the latter would be almost directly tied to family visits). Given that there may be different relevant ways to define visitation, we also conducted the analysis using a measure of whether an inmate ever received a single visit and whether the inmate received a visit within the first month of incarceration. These measures produced similar, but noisier, results.[8]

Visitor Type. The visitation data from the Danish Prison and Probation Service also contain information on the relationship between the visitor and the inmate: wife, girlfriend/significant other, child, parent, other visitors (e.g., a friend), and lawyers. We exclude lawyers from the data to avoid contaminating results by judicial processes; lawyers also seem less likely to exert the types of influences on inmates that we aim to analyze. By also calculating our visitation variables for each of the visitor types, we can provide a more comprehensive picture of visitation. We use separate measures of the following types of visits: all visitor types (includes everyone), family (wives, girlfriends/significant others, children, parents), children, parents, partners (wives/significant others), and other visitors.

[8] These results are available from the authors upon request. See Hickert, Tahamont, and Bushway 2018 for a discussion of measurement issues in relation to visitation.

Inmate Outcomes. We include outcomes measured during confinement and for two years following release. During confinement, we measure disciplinary actions per month of incarceration. This includes, in descending order of severity, the number of solitary disciplinary sanctions (i.e., punishment cell placements); number of official warnings about solitary disciplinary sanctions; and number of in-prison fines; number of general warnings. We also examine whether the inmate was granted early release on parole. Limited access to visitation could simply be counteracted by increases in access to leave, which could exert the same influence on inmates as visitation. So, we measure chances of being assigned to leave from prison (total number of leaves granted per month; total length of leaves relative to sentence length; total number of weekend leaves per month; and total length of weekend leaves relative to sentence length).

Following release, we focus on singlehood (defined as being unmarried and not living with a partner during the first year after release), re-partnering (only for those with a partner before admission), total number of residential moves (within two years from release), and several labor market outcomes (labor earnings; employment as defined from having labor earnings; officially employed on the last day of November; and ever being employed during the first year following release). We include whether the inmate received psychiatric treatment within one or two years. As measures of criminal recidivism, we focus on criminal reconviction, re-arrest, and receiving a new prison sentence on the basis of a new criminal conviction, not a violation of parole (all measured over a two-year period following the release date).

Family Outcomes. Partner outcomes cover the number of residential moves during two years from the visit, labor market outcomes (labor earnings and employment during the first year), whether the partner received psychiatric treatment within one or two years, and use of sick

leave (both whether the partner went on sick leave and for how long, measured during the first year from the visit). Child outcomes cover both authorized and unauthorized (i.e. truancy) absence from school during the first year, juvenile charges within two years, and performance on standardized test scores in Danish and mathematics (we use the first available test score after the visit). We measure the outcomes of visitors from the day the inmate was admitted to prison (because it is unclear which of the visits during an incarceration spell that we should focus on).

Control Variables. For each inmate in the data, we merge background characteristics from the standard registers, all measured prior to admission and covering demographic information, labor market information and criminal history. Demographic control variables are age at admission, sex, marital status (married, cohabiting, or single), ethnic minority background, years of education, and number of children (dummy coded). Labor market variables measure whether an inmate was employed, unemployed or not in the labor force (e.g., on social pensions) prior to admission, and labor earnings during the last year before admission. Criminal history variables are sentence length, crime type (violent crime, property crime, drug crimes, and a residual "other crimes" category), whether the inmate had previously been incarcerated, and number of previous convictions (dummy coded: 1, 2, 3, 4, 5+ prior convictions).

Section B of the online technical appendix provides details on the control variables and descriptive statistics across inmates who receive visitation during imprisonment and inmates who do not. As one would expect, there are marked differences between these two groups, and inmates who receive visitation are generally better off in terms of socioeconomic backgrounds. These distributions of background characteristics correspond with what we would expect given those reported in existing research (e.g., Cochran, Mears, and Bales 2017).

IV. Estimation Strategy

In what follows, we outline our estimation process. Before we turn to our estimation of the effects of visitation on inmate and family outcomes, we first show two sets of evidence supporting our instrumental variables strategy. First, we show that, as predicted by the rules for prison assignment, prison capacity is a key driver of the distance between an inmate's home and the prison in which he serves his sentence. Second, we show that distance is related to visitation.

IV.a. Prison Capacity Drives Distance between Prison and Home

First, we estimate the impact of prison occupancy rates on how far from home an inmate *i* on case *c* is sent to serve the sentence (*distance*). Here, we use our instrument, distance to the nearest relevant prison that has space: $min\left[\boldsymbol{PrisonDist} \times space_{Q95,t,j}\right]$. The model we estimate is:

$$distance_{ic} = \pi \min\left[\boldsymbol{PrisonDist} \times space_{Q95,t,j}\right] + \rho \min[PrisonDist] + \beta \mathbf{X}_{ic} + \delta_p + \theta_t + \mu_{ic}. \quad (1)$$

X contains control variables and a column of ones. δ_p are police jurisdiction fixed effects. θ_t are month-by-year fixed effects. The set of control variables includes the measures of local unemployment and criminal activity in an area to capture any changes in local conditions that could be driving assignment to prisons and inmate and family outcomes. We also control for the distance from the inmate's home to the nearest prison to control for within-precinct differences in inmates that live closer or further from prisons. μ is the error term assumed to be independent and identically distributed (iid). The parameter of interest is π, and we use the F-test statistic of excluding it from the model to evaluate the predictive power of the instrument.

In Figure 3, we present descriptive information on the relationship between prison occupancy and distance between an inmate's home and the prison where he serves his sentence. Panel A shows how far from home the inmates in our data are sent to serve their sentence relative to what would be the nearest relevant prison if we did not take occupancy rates into account. Inmates are on average assigned to prisons further away from home than the nearest relevant one. Most observations had 25-100 kilometers to the nearest relevant prison, yet were, on average, sent somewhere between 50 and 150 kilometers away from home to serve their sentence.

Panel B of Figure 3 shows the association between the occupancy rate at the nearest relevant prison and distance from home. The sizes of the dots represent size of the data, and the figure thus shows that by far most of the data fall within 10 percentage points below full capacity (marked by the dashed vertical line at 1.00). Above capacity, however, there is also a sizable amount of data. Above capacity, average distance from home is consistently higher than among inmates who are assigned at a point when occupancy is below capacity and the distance from home increases with increases above full capacity. Occupancy rates thus correlate with distance from home, which is exactly what we aim to leverage in our instrumental variable approach.

Table 2 reports results of the impact of distance to nearest relevant prison that has space on how far from home inmates are sent to serve their sentence. The relationship is positive and statistically significant, indicating that when the distance to nearest relevant prison that has space increases by 25 kilometers the distance to where an inmate is assigned increases by on average 13 kilometers (.520*25). According to the F-test of excluded instrument, the instrument is strong (F = 893), and we conclude that we can use our setup to measure the effect of distance from home on visitation.

Of course, our instrumental variables strategy rests on an assumption that the prison assignment mechanism generates exogenous variation in distance between home and the prison in which one is incarcerated. In support of this assumption, we looked to see whether the characteristics of inmates vary systematically with the occupancy rate at the closest prison to their home at the time the inmate is sentenced. In Table A1, we present results of separate regressions of each of 21 characteristics on the occupancy rate of the closest prison at the time of sentencing. Only one of these characteristics is statistically significantly related to the occupancy rate at even the 1 percent level and only a few are at the 5 percent level, regardless of whether we include police jurisdiction fixed effects and month by year fixed effects in the model. However, we control for them in our analysis.

IV.b. Distance from Home Drives Visitation

We now exploit the variation in distance that comes from the instrument to measure the unbiased effect of distance from home on *visitation* for inmates who had their distance increased by the instrument (a local average treatment effect). In practice, we do so by plugging individual predicted distances from home obtained from equation 1 ($\widehat{distance}$) into the second stage model:

$$visitation_{ic} = \gamma\, \widehat{distance}_{ic} + \beta \mathbf{X}_{ic} + p_{ic} + t_{ic} + \varepsilon_{ic} \qquad (2)$$

where $\boldsymbol{X}$, p, t, i, and c are like in equation 1 and ε is an error term that is iid.

Table 3 reports results of the effect of distance from home on visitation. Overall, the explanatory power of these outcome models is quite high. Results document a consistently negative effect of distance from home on the number of visits per month. For every additional 25 kilometers an inmate is sent from his or her home to serve a prison sentence, the average

frequency of visitation from any visitor type drops by one third visit per month. This decrease is mainly driven by decreased family visitation (except for children, for whom the point estimate is again numerically smaller), although distance also causes a marked reduction in "other visitation", which includes, for example, friends.

We present results for various inmate types in Table 4. There is some variation in the precision of the estimates by subgroups—due to variability in number of observations. Also, there are some signs of effect heterogeneity. The average number of visits for inmates older than 30 years, for example, seems to be impacted as little as half as much by distance from home than inmates who are convicted of drug crimes; and the effect for females, which is imprecisely estimated due to the low number of females in our data, seems to be positive. But, generally, the results are quite stable across inmate types, especially taking the sizes of the estimated standard errors into account.

IV.c. Instrumental Variables Estimates: Effects of Visits on Inmate and Family Outcomes

Our main estimates focus on the effect of visitation on the outcomes of inmates and their visiting family members. Because we theorize that any causal effect of distance from home on outcomes should be driven by the impact of distance on visitation (which our second analytical step measured), we estimate the following set of equations:

$$visitation_{ic} =$$

$$\pi \min\left[\boldsymbol{PrisonDist} \times space_{Q95,t,j}\right] + \rho\min[PrisonDist] + \beta\mathbf{X}_{ic} + \delta_p + \theta_t + \mu_{ic} \qquad (3)$$

$$outcome_{ic} = \gamma\, \widehat{visitation}_{ic} + \beta\mathbf{X}_{ic} + \delta_p + \theta_t + \varepsilon_{ic}.^{9} \qquad (4)$$

[9] When examining family outcomes, we cluster standard errors at the individual level to correct for autocorrelation that may arise from the same individual visiting more than one inmate.

In equation 4, γ measures the causal effect of visitation on inmate and family outcomes for inmates whose number of visits per month varies because of the distance between where they serve their sentence and their home. In other words, this is a local average treatment effect specific to the population whose visits vary with this distance.

The assumptions underlying this interpretation are the relevance assumption and exclusion restriction. We provided support above that the relationship between prison capacity and visitation is strong. The exclusion restriction implies that prison capacity is only related to the outcomes of inmates and their families because it drives visitation, rather than through any other mechanism. Given our time and police jurisdiction fixed effects, and controls for the distance the nearest prison, the variation in prison capacity is driven by within-month of the same year variation in prison capacity among inmates from the same jurisdictions. In turn, this is likely driven by things related to the prison operation (like release dates for existing prisoners) rather than by things related to the inmate being sentenced. Furthermore, we have included time-varying controls for the local unemployment rate and the local crime rates near the inmates' homes. This allows us to capture any trends in crime or economic conditions that might lead to more prisoners sent far from home and worse outcomes for families for reasons unrelated to visitation.

V. Results: The Effects of Visitation on Inmate and Visitor Outcomes

Having established that distance from home has a stable effect on visitation across definitions of visitation, across types of visitors, and across types of inmates, we now turn to results of the effect of visitation on inmate and visitor outcomes. As mentioned, we exploit the finding that distance from home matters for visitation patterns to estimate the derived effect of

visitation on outcomes. Figures 4 and 5 show point estimates and 95 percent confidence intervals from second stage regressions of the change in average number of visits per month on a range of inmate outcomes (Figure 4), as well as on partner and child outcomes (Figure 5). The associated point estimates are available in Table A2.[10]

Overall, our results paint a mixed portrait of the effects of visitation on inmate and family outcomes. Often, our instrumental variables estimates of the effects of visitation are quite close to zero. And, except in measures of low-probability events, we can rule out that visitation leads to changes in outcomes of more than a few (or at most 15) percent. We highlight some of the patterns in the estimates in this section.

First, more visitation leads to an increase in disciplinary infractions. An extra visit per month leads to a 2 percentage point increase in the number of warnings per month that is statistically significant at the five percent level.

Second, the evidence suggests that visits and leave may be complements of each other. Estimates of the effect of additional visits per month on the number of leaves is positive, though the amount of time spent on-leave does not change with additional visits and none of the estimates are statistically significant at conventional levels. This suggests that any estimates of the relationship between visits and outcomes should be considered in a context where there may be both more visits and more leave-taking.

Third, very few of the other outcomes for either families or inmates are affected by visitation. Almost all of the point estimates, either OLS or instrumental variables, are close to zero. Also, despite the fact that our instrumental variables strategy leads to noisier estimates, we

[10] Figures 4 and 5 also show results of the association between visitation and the outcomes as measured using a standard OLS model and controlling for time effects, police jurisdiction fixed effects, and the control variables. Even in this descriptive model, the association between visitation and all outcomes is zero or close to zero, with very few estimates showing a significant result.

can rule out changes larger than 15 percent of the mean of our measures of inmate criminal justice outcomes (early release and recidivism), migration (number of residential moves), and the likelihood of being single. Also, we can rule out changes in labor supply of over 20 percent of the mean across our earnings and employment measures. The point estimates near zero and limited scope of the confidence intervals suggest that visitation has limited effect on inmate outcomes across the board.

Fourth, we find visitation has a limited impact on the outcomes of the family members of inmates. As with inmates, the point estimates of the effect of visitation on partner labor supply measures are quite close to zero and the confidence intervals allow us to rule out increases of more than 5 percent and decreases of more than 8 percent. Our estimates of the effects of visitation on partner health and children's school outcomes and juvenile delinquency are also quite close to zero. However, since these outcomes are generally very low probability events, our confidence intervals are wider.

We also examined the results for various subgroups of our inmate population who might experience different conditions of confinement or whose behavior may be more or less elastic relative to visitation. We looked at inmates based on whether their sentences send them to closed prisons (those with longer sentences) or open prisons (those with shorter sentences). Closed prisons are ones where conditions are harsher than the open prisons in Denmark, and much closer to the conditions of confinement in the U.S. We also estimated the effects of visitation separately by whether an inmate had committed a drug related crime. The estimates across these various groups were not appreciably different, though the standard errors are large enough that we cannot make conclusive comparisons (Appendix Figure 1).

VI. Conclusion

In this study, we have shown that an increase in visitation, as measured by the number of visits an inmate receives per month, has little effect on his outcomes and on his family's outcomes in Denmark. To isolate this causal estimate, we used exogenous variation in visitation driven by exogenous variation in the distance between an inmate's home and the prison in which he serves time, which is in turn driven by the prison assignment mechanism.

These results confirm earlier findings that visitation has no effect on recidivism using administrative data in Florida and Iowa. We extend the previous research by considering not just recidivism, but also a wide range of outcomes for inmates and their families. Although our results are noisy for outcomes that are low-probability events, most of our point estimates are close to zero and suggest little effect of visitation.

There are a few limitations of our analysis that should be considered when generalizing the estimates. First, our estimates are relevant for the population who receive additional visits because they are assigned to a prison closer to their home. The effects of visitation may be different for inmates whose visits are not sensitive to distance. Second, the estimates are from Denmark. Although there are some similarities between Denmark and the U.S., there are also differences. Visitation in Denmark prisons occurs in a much less restrictive setting than most visitation in U.S. prisons. Similarly, incarceration itself in Denmark is less restrictive than incarceration in the U.S. We think these differences would both lead the estimates here to be more positive than estimates for the U.S. would be. However, it is worth pointing out that the estimates from Florida and Iowa on recidivism are similar to ours here, which may imply that our results for other outcomes results generalize as well. Third, it may be the case that the effects of visitation are not well captured by our quantitative outcome measures in the administrative

data. That said, if there are effects of visitation on softer or shorter-run outcomes, it is valuable to know they do not play out in the important measures of life success that we are able to observe in the administrative data.

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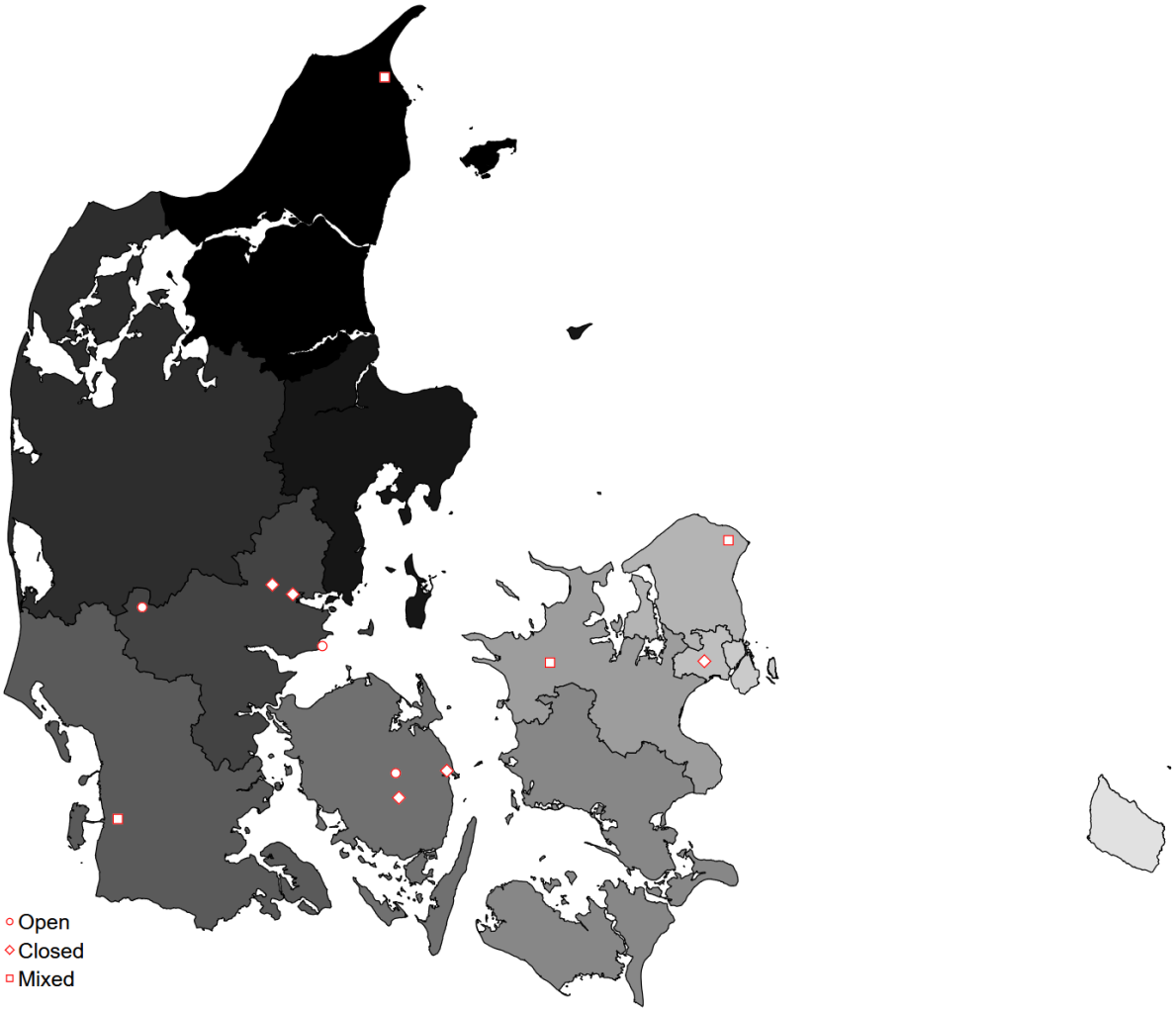
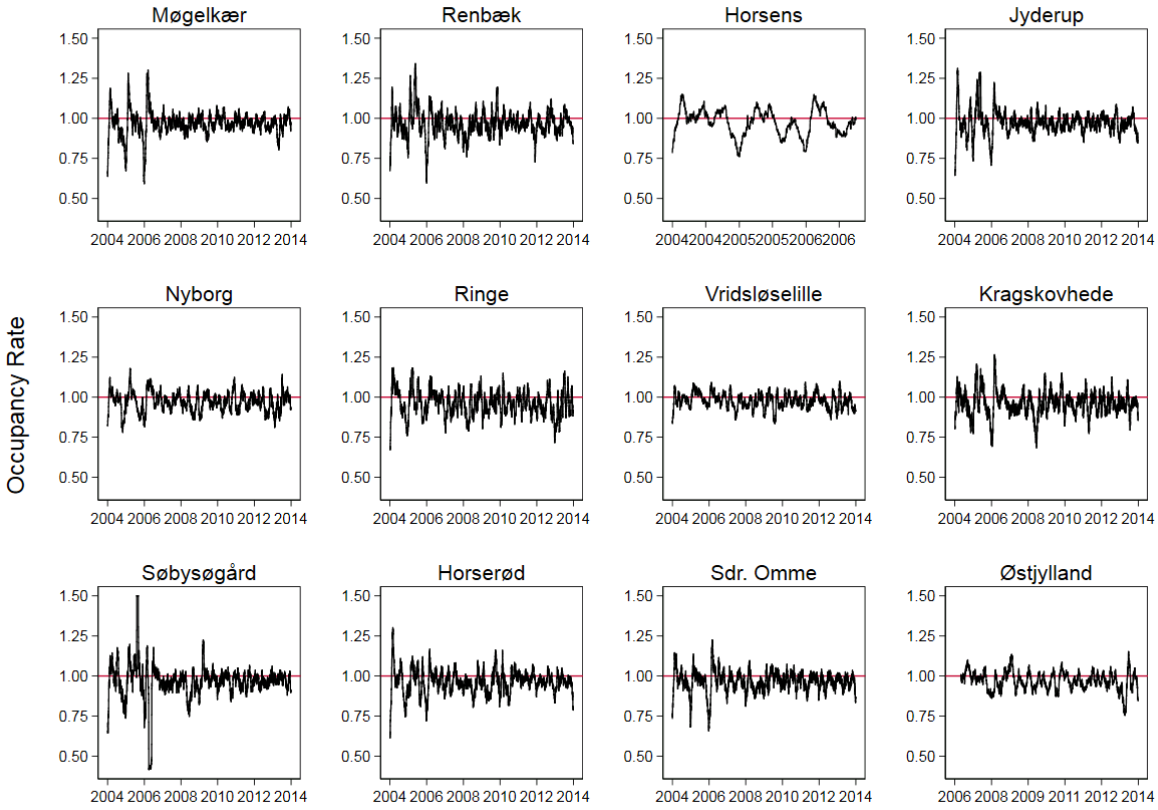
Figure 1. Location of Prisons and Police Jurisdictions in Denmark



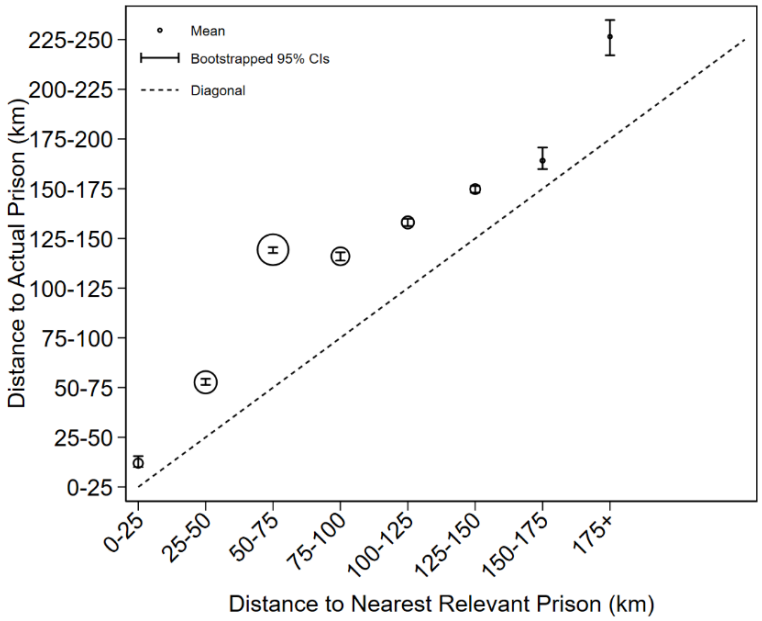
Notes: The map shows police jurisdictions (marked by grayscale) in Denmark, and the location of open (low security) and closed (high security) prisons. "Mixed" refers to prisons that have both open and closed prison wings. We only show prisons which existed during our data window.

Figure 2. Estimated Occupancy Rates of Prisons in Denmark, 2004-2014.

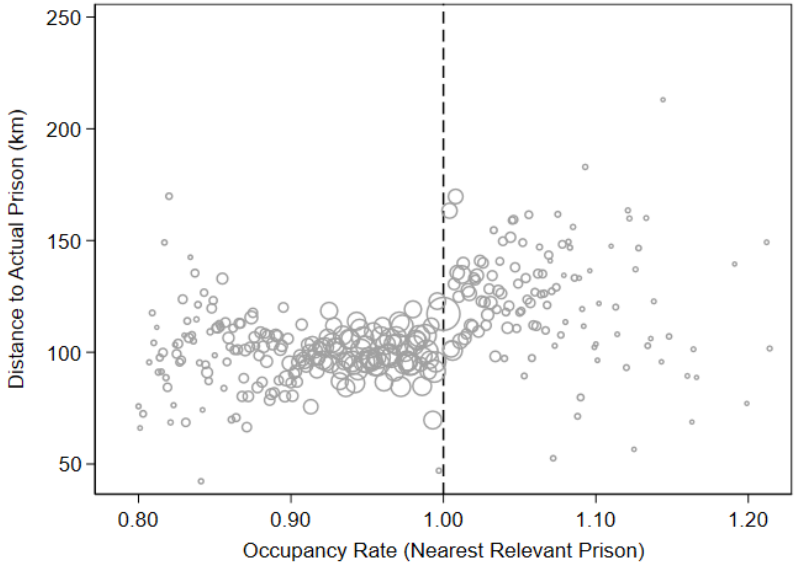


Notes: Figure shows the daily estimated occupancy rate of each prison in our data, as defined in our Method section: For each day in each prison we calculate the occupancy rate as the number of prisoners in the prison on that day divided by the 95th percentile of the number of prisoners in that prison during the preceding period when it is most likely that prison assignment occurred. Note that the outlying spikes in Søbysøgård Prison in 2005 have been trimmed at 1.50 (from app. 2.0) to maintain comparability of the y-axes across the subfigures. The steep increase in occupancy in Søbysøgård Prison at that point (and, as a consequence of how we estimate the occupancy rate, the steep decrease in 2006) arose from the temporary opening of a local prison, Institution Holmegård, which hosted prisoners serving short sentences and who were administratively registered to Søbysøgård Prison. Horsens Prison was closed in late 2006 and Østjylland Prison was opened in October 2006, which is the reason why we do not have information for the full period for these two prisons.

Figure 3. Association Between Distance to Nearest Relevant Prison and Actual Distance from Home (Panel A) and Association Between Occupancy Rate at the Nearest Relevant Prison and the Distance to Where the Prisoner is Actually Sent to Serve the Sentence (Panel B).



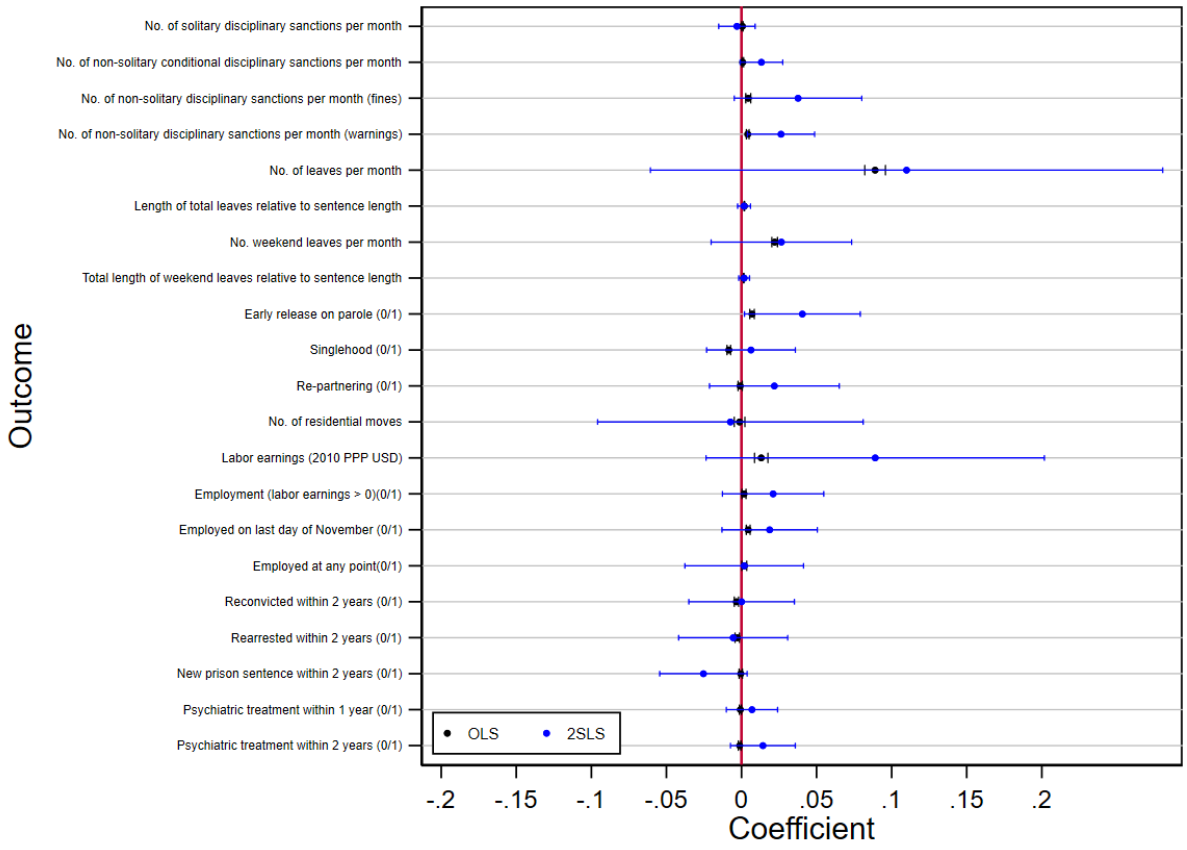
Panel A



Panel B

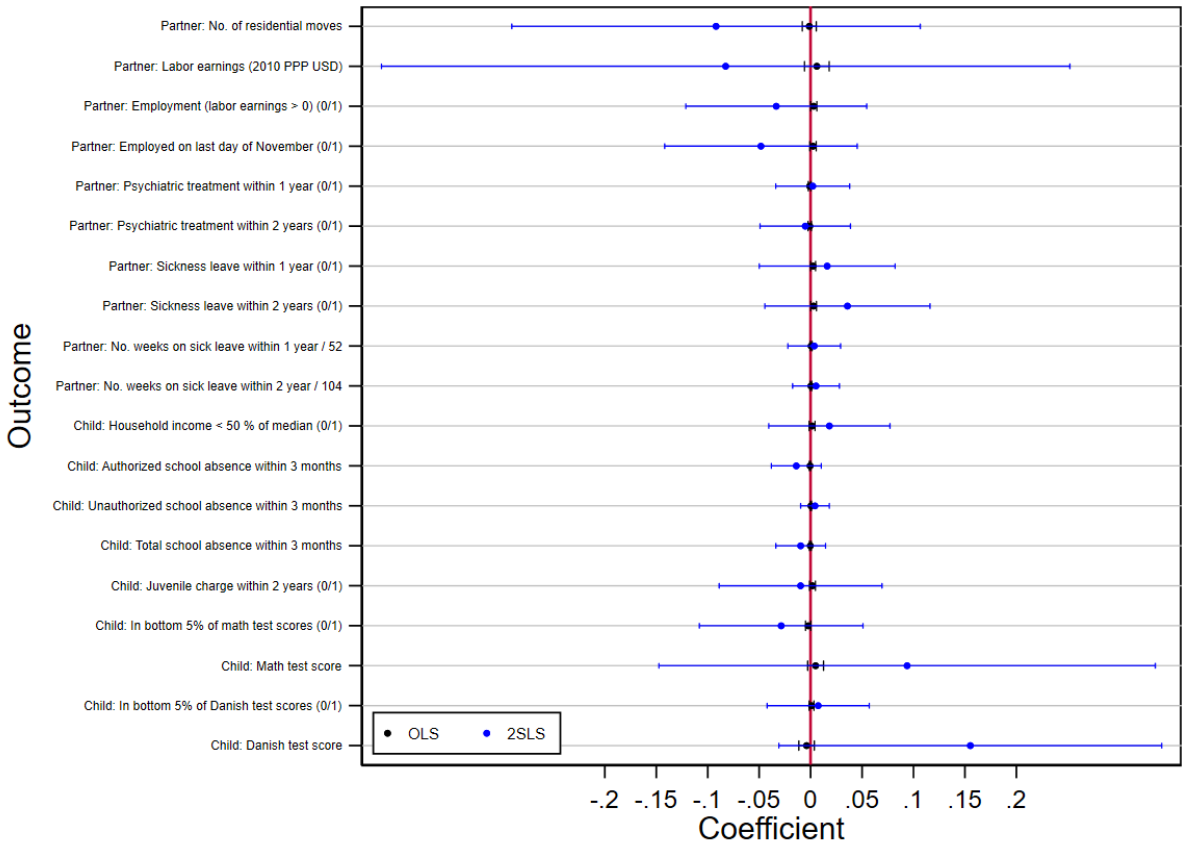
Notes: Panel A shows that prisoners on average serve their sentence farther away from home than the distance to the nearest relevant prison. The following percentages had the same distance to nearest relevant prison and distance to actual prison (and hence fall onto the diagonal of the figure): 0-25 km: 85%; 25-50 km: 72%; 50-75 km: 48%; 75-100 km: 59%; 100-125 km: 39%; 125-150 km: 49%; 150-175 km: 70%; 175-200 km: 5%. Panel B shows that occupancy rate, specifically whether occupancy is below or above what we define as the nearest relevant prison being "full" (1.00), at the nearest relevant prisons matters for how far from home prisoners are sent to serve the sentence. In both panels, the size of each dot represents the sample size within each bin on the x-axis.

Figure 4. Results Summary of Effect of Average Number of Visits Per Month on Offender Outcomes, by Estimator.



Notes: Figure shows parameter estimates and 95% confidence intervals from OLS and 2SLS models as defined in the Method section. All estimates are available in Table A2. The F-test statistics for the excluded instrument when regressing distance from home on average number of visits per month (and controlling for background characteristics, month-by-year fixed effects, and jurisdiction fixed effects) is 41.06. Please note that this figure does not report point estimates for "Total hours of leave per month" and "Total hours of weekend leave" as the scale of these estimates and their standard errors exceed what can be meaningfully reported in the figure. These estimates, which are statistically insignificant, are available in Table A2.

Figure 5. Results Summary of Effect of Average Number of Visits Per Month on Partner and Child Outcomes, by Estimator.



Notes: Figure shows parameter estimates and 95% confidence intervals from OLS and 2SLS models as defined in the Method section. All estimates are available in Table A2. The F-test statistics for the excluded instrument when regressing distance from home on average number of visits per month (and controlling for background characteristics, time effects, and jurisdiction fixed effects) is 41.06.

Table 1. Descriptive Statistics (Data Source information and Means and Standard Deviations of Variables) of Datasets on Inmate Visitation in the United States and Denmark.

Country	United States		Denmark	
Region	Florida only		Full population	
Data source	Florida Department of Corrections Offender-Based Information System (OBIS)[a)]		Danish Register Data	
Data period	Nov 2001 – Mar 2002		Jan 2004 – Dec 2014	
Statistic	M	SD	M	SD
Visitation within 12 months of release from prison				
Whether visited	0.42	0.49	0.59	0.49
Number of times visited (among all inmates)	4.28	9.57	14.76	31.00
Number of times visited (among inmates with visits)	10.21	12.58	25.10	37.07
Visited by…				
Parent	0.27	0.44	0.29	0.45
Spouse	0.05	0.21	0.09	0.28
Significant other	0.08	0.28	0.33	0.47
Child	0.06	0.23	0.06	0.24
Relative	0.22	0.41	0.53	0.50
Friend	0.12	0.32	0.37	0.48
Other person / undefined	0.01	0.1	0.00	0.07
Number of times visited by…				
Parent	2.36	7.22	2.68	7.54
Spouse	0.66	4.48	1.15	5.18
Significant other	0.97	5.38	4.82	12.68
Child	0.26	1.74	0.46	2.50
Relative	1.14	3.72	10.76	23.75
Friend	0.76	3.73	3.51	8.94
Other person / undefined	0.39	0.73	0.00	0.00

Control variables				
Male	0.92	0.27	0.92	0.27
Non-White / Ethnic minority background[b)]	0.62	0.49	0.18	0.38
Age at release	34.5	9.7	32.85	10.77
Months in prison	39.9	31.2	16.48	30.23
Number of disciplinary infractions in prison	3.90	7.10	2.54	4.99
Current offense is…				
Violent	0.38	0.48	0.51	0.50
Property	0.28	0.45	0.17	0.37
Drug	0.26	0.44	0.15	0.35
Other	0.08	0.28	0.18	0.39
Number of prior felony convictions	4.80	2.90	3.12	1.93
Number of prior prison commitments	1.80	0.87	0.63	0.48
Number of observations	7,000		25,573	

Notes: [a)] Table column reprints the results from Table 1 in Bales and Mears (2008: p.300). [b)] In the Florida data this variable measures the proportion of non-Whites (i.e., Black or Hispanic). In the Danish data, ethnic minority background refers to immigrants from non-Western countries and descendants hereof.

Table 2. Results Summary of First Stage Estimation of Distance from Home on Distance to Nearest Relevant Prison with an Opening.

Outcome	Actual Distance from Home
Distance to nearest relevant prison with an opening (in 25 kilometers)	0.520***
	(0.017)
F-test	893***
Observations	25,573
Police Jurisdiction Fixed Effects	Yes
Month-by-Year Fixed Effects	Yes
Covariates	Yes

Notes: Table reports the coefficient for how far from home (Actual Distance from Home, in 25 kilometers) a prisoner is sent to serve his or her sentence regressed on the distance between the prisoner's home and the nearest relevant prison that has a vacant cell, with both relevance and vacancy defined as described in the Method section. We control for background characteristics, month-by-year fixed effects, and for police jurisdiction fixed effects. The point estimate shows that as the distance to nearest relevant prison with an opening increases by 25 kilometers, the average actual distance from home increases by .520 x 25 kilometers = 13 kilometers. The F-test statistic indicates that distance to nearest relevant prison with an opening is a statistically strong instrument for how far from home prisoners are sent. Standard error in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two-tailed tests).

Table 3. Effect of Distance from Home on Number of Visits per Month, by Type of Visitor. Results from 2SLS Models.

Visitor	Any Visitors	Family Members	Children	Partners	Parents	Other Visitors
Distance	–0.327***	–0.249***	–0.016*	–0.135***	–0.065***	–0.073***
	(0.048)	(0.039)	(0.007)	(0.023)	(0.016)	(0.020)
N	25,573	25,573	25,573	25,573	25,573	25,573
R^2	0.174	0.156	0.068	0.111	0.125	0.078
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All models controlled for police jurisdiction fixed effects and month-by-year fixed effects. Distance measured in 25 kilometers. Standard errors in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two-tailed tests).

Table 4. Effect of Distance from Home on Visits per Month (Any Visitor), by Subsample.

Subsample	*N*	
Female	2,083	0.207
		(0.386)
Male	23,490	–0.336***
		(0.056)
Younger than 20 years of age	2,721	–0.303*
		(0.121)
Age 20–29 years	9,829	–0.447***
		(0.103)
Age 30+ years	13,023	–0.240***
		(0.059)
Convicted of Violence	12,946	–0.302***
		(0.072)
Convicted of Drugs	3,715	–0.485***
		(0.115)
Convicted of Property Crimes	4,240	–0.327**
		(0.118)
Convicted of Other Crimes	4,672	–0.398***
		(0.118)
First Incarceration Spell Only	9,505	–0.285***
		(0.084)

Notes: All models controlled for police jurisdiction fixed effects, month-by-year fixed effects, and covariates. Distance measured in 25 kilometers. Standard errors in parentheses.
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two-tailed tests).

Online Technical Appendix
to
"How Does Visitation Affect Incarcerated Persons and Their Families? Estimates Using Exogenous Variation in Visits Driven by Distance between Home and Prison"

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A. Appendix Tables and Figures

Table A1. Estimation Results of Separate Regressions of each of 21 Characteristics on the Occupancy Rate of the Closest Relevant Prison. Results from OLS models.

Model	(1)		(2)	
Variable	Estimate	(SE)	Estimate	(SE)
Distance to closest relevant prison (25 km)	0.54	(0.36)	0.35	(0.26)
Sentence length (years)	0.56*	(0.21)	0.44*	(0.15)
Previous convictions	0.26	(0.13)	0.37***	(0.11)
Previously incarcerated	0.02	(0.04)	0.00	(0.04)
Crime type = violence	–0.01	(0.05)	0.06	(0.06)
Crime type = drugs	0.05	(0.05)	0.03	(0.04)
Crime type = property	–0.05	(0.03)	–0.06*	(0.03)
Married	–0.05*	(0.02)	–0.04	(0.02)
Cohabiting	–0.04	(0.02)	–0.02	(0.02)
Age at admission	0.71	(1.05)	0.13	(0.99)
Female	0.05	(0.05)	0.07	(0.06)
No. of children	–0.13*	(0.06)	–0.11*	(0.04)
Non-western minority	0.04	(0.06)	0.01	(0.02)
Education (years)	–0.43	(0.32)	–0.58	(0.28)
Missing educ.	0.00	(0.03)	0.02	(0.03)
Wage income	–1,849	(1,157)	–371	(1,310)
Employed	–0.09	(0.05)	–0.02	(0.05)
Unemployed	–0.02	(0.05)	–0.01	(0.04)
Local admission rate	0.09	(0.23)	–0.17	(0.18)
Local crime rate	18.11	(12.64)	7.09	(6.01)
Local unemployment rate	–75.01	(78.4)	–102.76*	(34.41)
N	25,573		25,573	
Police jurisdiction fixed effects	No		Yes	
Month-by-year fixed effects	No		Yes	

Notes: Estimates reported in the table refer to $\hat{\beta}$(*occ_rate*), where *occ_rate* is the occupancy rate at the nearest relevant prison, from one regression model per table row, $variable = intercept + \beta(occ\_rate) + \delta_p + \theta_t + e$. δ_p are police jurisdiction fixed effects and θ_t are month-by-year fixed effects. Standard errors in parentheses.

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two–tailed tests).

Table A2. Effect of Number of Visits per Month on Offender, Partner, and Child Outcomes. Results from 2SLS models.

Outcome	95% CI of Effect of Number of Visits	Sample Mean (SD)	Mean for Prisoners with No Visitation (SD)	*N*
Offender				
No. of solitary disciplinary sanctions per month	–0.02:0.01	0.04 (0.15)	0.03 (0.17)	25,573[a]
No. of non–solitary conditional disciplinary sanctions per month	–0.00:0.03	0.05 (0.17)	0.04 (0.19)	25,573[a]
No. of non–solitary disciplinary sanctions per month (fines)	–0.00:0.08	0.26 (0.54)	0.23 (0.64)	25,573[a]
No. of non–solitary disciplinary sanctions per month (warnings)	0.00:0.05*	0.09 (0.27)	0.07 (0.32)	25,573[a]
No. of leaves per month	–0.06:0.28	1.85 (2.25)	1.26 (1.77)	25,573[a]
Total hours of leave per month	–1.86:4.25	38.55 (40.47)	29.24 (39.03)	25,573[a]
Length of total leaves relative to sentence length	–0.00:0.01	0.05 (0.06)	0.04 (0.06)	25,573[a]
No. weekend leaves per month	–0.02:0.07	0.58 (0.62)	0.43 (0.60)	25,573[a]
Total hours weekend leave	–1.32:3.81	30.96 (34.09)	23.34 (32.87)	25,573[a]
Total length of weekend leaves relative to sentence length	–0.00:0.01	0.04 (0.05)	0.03 (0.05)	25,573[a]
Early release on parole	0.00:0.08*	0.59 (0.49)	0.57 (0.50)	25,573[a]
Singlehood	–0.02:0.04	0.79 (0.40)	0.83 (0.38)	25,573[a]
Re–partnering	–0.02:0.07	0.05 (0.22)	0.04 (0.20)	4,799[b]
No. of residential moves	–0.10:0.08	2.02 (1.19)	1.92 (1.15)	25,573[a]
Labor earnings (2010 PPP USD)	–0.02:0.20	0.98 (1.75)	0.96 (1.75)	25,573[a]
Employment (labor earnings > 0)	–0.01:0.05	0.43 (0.49)	0.42 (0.49)	25,573[a]
Employed on last day of November	–0.01:0.05	0.31 (0.46)	0.30 (0.46)	25,573[a]
Employed at any point	–0.04:0.04	0.32 (0.47)	0.26 (0.44)	15,605[c]
Reconvicted within 2 years	–0.04:0.04	0.38 (0.48)	0.37 (0.48)	25,573[a]
Rearrested within 2 years	–0.04:0.03	0.46 (0.50)	0.43 (0.50)	25,573[a]
New prison sentence within 2 years	–0.05:0.00	0.16 (0.36)	0.14 (0.35)	25,573[a]
Psychiatric treatment within 1 year	–0.01:0.02	0.05 (0.21)	0.05 (0.21)	25,573[a]
Psychiatric treatment within 2 years	–0.01:0.04	0.08 (0.27)	0.07 (0.26)	25,573[a]
Partner				
No. of residential moves	–0.29:0.11	1.89 (1.13)	1.74 (1.05)	4,799[d]
Labor earnings (2010 PPP USD)	–0.42:0.25	1.61 (1.95)	1.63 (1.98)	4,799[d]

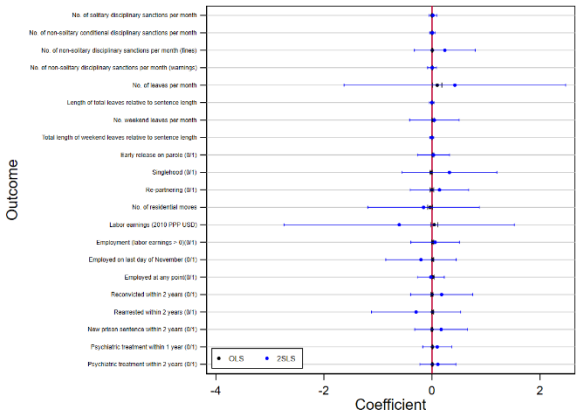
Employment (labor earnings > 0)	–0.12:0.05	0.58 (0.49)	0.56 (0.50)	4,799[d]
Employed on last day of November	–0.14:0.05	0.49 (0.50)	0.50 (0.50)	4,799[d]
Psychiatric treatment within 1 year	–0.03:0.04	0.04 (0.20)	0.04 (0.20)	4,799[d]
Psychiatric treatment within 2 years	–0.05:0.04	0.07 (0.25)	0.06 (0.24)	4,799[d]
Sickness leave within 1 year	–0.05:0.08	0.16 (0.37)	0.15 (0.36)	4,799[d]
Sickness leave within 2 years	–0.04:0.12	0.25 (0.43)	0.23 (0.42)	4,799[d]
No. weeks on sick leave within 1 year / 52	–0.02:0.03	0.04 (0.14)	0.04 (0.15)	4,799[d]
No. weeks on sick leave within 2 years / 104	–0.02:0.03	0.04 (0.13)	0.04 (0.13)	4,799[d]
Children				
Household income < 50% of median	–0.04:0.08	0.24 (0.43)	0.22 (0.41)	5,451[e]
Authorized school absence within 3 months	–0.04:0.01	0.04 (0.04)	0.04 (0.04)	1,113[f]
Unauthorized school absence within 3 months	–0.01:0.02	0.01 (0.03)	0.01 (0.03)	1,113[f]
Total school absence within 3 months	–0.03:0.01	0.05 (0.06)	0.05 (0.05)	1,113[f]
Juvenile charge within 2 years	–0.09:0.07	0.09 (0.29)	0.11 (0.31)	2,553[g]
Math test score	–0.15:0.34	–0.60 (1.03)	–0.64 (1.05)	4,326[h]
In bottom 5% of math test scores	–0.11:0.05	0.13 (0.34)	0.14 (0.35)	4,326[h]
Danish test score	–0.03:0.34	–0.53 (1.11)	–0.54 (1.13)	5,400[i]
In bottom 5% of Danish test scores	–0.04:0.06	0.13 (0.34)	0.13 (0.34)	5,400[i]

Notes: Sample sizes refer to [a] full sample, [b] prisoners with partners, [c] prisoners with formal employment, 2008–2014, [d] cohabiting or married partners (at admission) of the prisoners in the full sample, [e] all children of the prisoners in the full sample, [f] children aged 6–15 at admission who appear in the school absence register, 2011–2014, [g] Children aged 11–15 at prisoner's admission [h] Children who completed at least one national test in mathematics, [i] Children who completed at least one national test in Danish. Standard errors in parentheses.

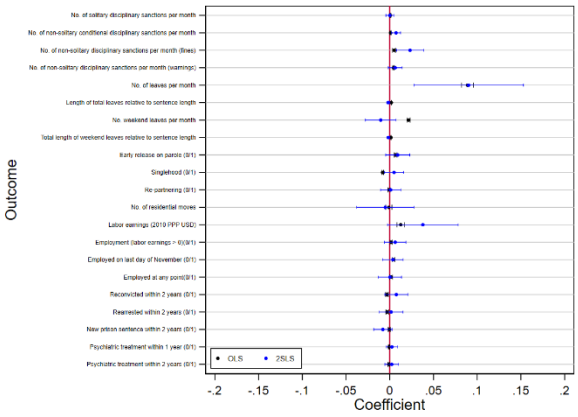
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two–tailed tests).

Appendix Figure 1. Effect of visitation on prisoner outcomes during and following release, by case characteristics

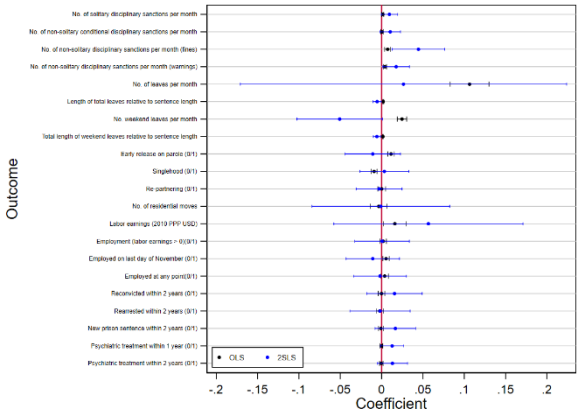
Panel A. Sentence length > 5 years



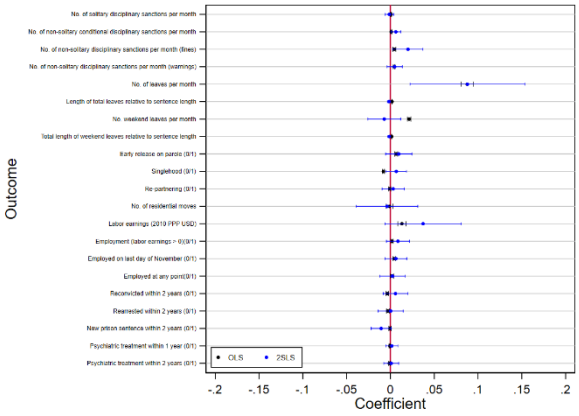
Panel B. Sentence length <= 5 years



Panel C. Drug crimes



Panel D. Non-drug crimes



B. Additional Details on Data and Variables

In this section, we provide additional details on our data, control variables, and outcomes, including the data sources from which they were obtained.

B.I From Raw Data to Analytical Sample

The raw data which we received from the Danish Prison and Probation Service (DPPS) contained detailed information on each individual-by-facility spell of incarceration, obtained from the DPPS's national case management system. To arrive at our analytical sample – which for each sentence includes one observation for the inmate's first assignment to an open or closed prison (admission and release during 2004-2014) – we took a number of data steps which reduced the raw data. Table B1 summarizes these data steps and reports the progression of our data size (total number of observations; number of unique inmates; number of unique incarceration spells). Our final analytical sample included 25,571 observations (spells) on 20,603 individual inmates. In addition to these data, which we used to define our analytical sample of inmates, we also received data on all visits (including the identification numbers of visitors) that they experienced during confinement, as explained in detail in the main text.

Table B1. Data Steps and Observations Counts from Raw Data to Analytical Sample.

Data Step	N	Unique inmates	Unique spells
Raw data from DPPS	334,637	83,827	130,346
Drop full duplicates	334,605	83,827	130,346
Keep spells with admission and release during 2000-2015[1]	326,775	81,461	127,442
Drop invalid identification numbers	311,543	73,133	118,308
Drop cases with reversed dates	310,137	72,896	117,761
Keep only relevant variables and drop duplicates on these	216,470	72,896	117,671
Drop spells served in local jails	181,558	57,371	87,778
Drop all spells for people placed in long-term court-ordered treatment ("Anstalten ved Herstedvester")[2]	172,748	56,034	85,893
Drop if first assignment is transit prison ("Midtjyllands Fængsel")	123,357	37,006	52,939
Keep only first observation per individual per case	52,936	37,004	52,936
Keep if admission and release during 2004-2014[3]	36,803	26,885	36,803
Drop if spells share admission date for the same individual	36,794	26,885	36,794
Drop observations with extreme values on instrument and arrive at analytical sample[4]	25,573	20,603	25,573

Notes: [1] Some cases in the raw data file fell outside the requested data window; we ignore these [2] We drop all cases for individuals who at some point enter the institution for long-term court-ordered treatment. Inmates placed here have severe mental issues and are often held in indefinite captivity, and they are not representative of inmates in general in Denmark. [3] The distribution of cases across time reveals anomalies prior to 2004 and we therefore focus only on admissions from January 1st, 2005. And although we have admissions in 2015, only few inmates admitted during 2015 have been release by December 31 that year, and we therefore drop cases from 2015. [4] See section D of this appendix.

B.II Control Variables

By merging standard Danish register data onto our data from the DPPS, we obtain our control variables. We measure all control variables prior to admission into prison to avoid them being affected by the imprisonment. We here list the control variables by the type of registers they are based on, and we summarize their distribution by visitation status.

B.II.a Population registers. We obtain date of birth, which is recorded from birth certificates. Combining date of birth with admission dates gives us age at admission (measured in years and entering our analyses as continuous variable). We include a dummy variable for females. For marital status, we use information on legal civil status and dates of any changes herein; we distinguish between individuals who were married, cohabiting but unmarried, or

unmarried and not living with a partner just before admission into prison. A set of dummy variables define number of children (0, 1, 2, 3, 4, or 5+ children) that the individual is recorded as being the parent of (again based on birth certificates). As a last demographic information we mark non-western ethnic minority backgrounds (1 = "Yes", 0 = "No"), defined as whether the individual or both of his/her parents migrated to Denmark from a country not on the list of "western" countries (the EU28 countries, Andorra, Iceland, Liechtenstein, Monaco, Norway, San Marino, Switzerland, the Vatican, Canada, the United States of America, Australia and New Zealand).

B.II.b Education registers. The Ministry of Education records the educational credentials of the population and reports these to Statistics Denmark. We rely on the ministry's definition of length (in years) of highest education attained. Tuition fees are tax covered in Denmark and from age 18, students get a public stipend for being enrolled in education. Denmark has 10 years of mandatory schooling but final exams in elementary schools are not mandatory, which causes some people to be recorded with fewer or no years of education. In our data, we mark such individuals with missing length of education with a dummy variable.

B.II.c Labor market registers. We rely on two sources of information to define control variables related to the labor market. First, the Ministry of Employment reports each resident's labor market status on the last day of November each year to Statistics Denmark, following the International Labour Organisation's (ILO) standards to secure comparability across countries. Labor market status consists of three mutually exclusive categories: employed (holds a job), unemployed (does not hold a job but is required to look for one), and not in the workforce (does not hold a job and is not required to look for one because of, for example, social pension). We use the latest recorded labor market status prior to admission to prison. Second, the Danish

Customs and Tax Administration reports all legal income to Statistics Denmark, and to gauge labor market performance (in addition to the November measures of labor market status), we control for labor wages earned during the year before admission to prison (in 2010 Purchasing Power Parity [PPP] adjusted Dollars, using OECD's individual consumption PPP-adjustment). In Denmark, all income is full third-party reported to the Danish Customs and Tax Administration, and 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. The earnings measure thus includes any income from legal labor work.

B.II.d Criminal justice registers. The DPPS and the Danish National Police report several criminal justice variables to Statistics Denmark. Cases have unique identification numbers, and individuals are matched to cases using their personal identification number, allowing us to merge a range of case and individual information (charges, convictions, sentences, incarcerations). We distinguish between information related to the case for which a person serves the prison sentence in our data, and prior criminal history. Concerning the current case, we include sentence length (in years) and a set of dummy variables for crime type (violent crime, property crime, drug crimes, and a residual "other crimes" category). Concerning criminal history, we include a dummy for whether the person had previously been incarcerated and the person's number of previous convictions (dummy coded: 1, 2, 3, 4, 5+ prior convictions).

B.II.e Local measures (at admission). We include three measures of the local constraints which the person and his or her family members are exposed to, all measured as rates per 10,000 in the population of 15-65-year-old persons in the police jurisdiction during the month when the focal person was admitted to prison. Local admission rate relies on the incarcerations register and measures the number of people in that jurisdiction who were admitted

to prison. Local crime rate relies on the crime reports register and measures the number of reported crimes during the month of admission. Local unemployment rate relies on the DREAM register, which consists of official records of weekly social benefit receipt, as compiled from data from the Danish Ministry of Employment, The Danish Ministry of Education, and the Danish Customs and Tax Administration. The DREAM register records the primary type of public benefit a person receives each week, which allows us to single out benefits related to unemployment and active labor market program participation. We count as unemployed everyone who received these types of benefits for at least one week during the month the focal person was admitted to prison.

*B.II.f Control variable distribution*s. Table B2 shows descriptive statistics of the control variables across inmates who receive visitation during imprisonment and inmates who do not. Disregarding visitation status, few inmates are females and about one in five have ethnic minority backgrounds. A large proportion is single and around 80 percent do not have children, which reflects the relatively low average age at admission (just above 30 years). And as one might expect, the inmates have low education (corresponding to just above mandatory schooling on average) and only one in three had a job prior to admission – and the proportion outside the labor market (e.g., on social pensions) is high. Around half the inmates serve a prison sentence for violent crimes, more than half were previously incarcerated, and a low proportion had not prior convictions.

Comparing the control variable distributions across visitation status shows that the group of never visited inmates consist of a higher share of females, a higher share of singles, and a higher share with minority backgrounds. They have fewer average years of education and lower employment (which is counterbalanced by even more outside the labor force). Also, their

average labor earnings are lower. In terms of criminal justice characteristics, the main difference between the groups is that those without any visitation tend to serve shorter sentences on average. In conclusion, although the inmates in general fare poor across the control variables, those who are never visited in prison constitutes an even more negatively selected group.

Table B2. Descriptive Statistics of Control Variables, by Visitation Status.

	Visited		Never Visited		
	Mean (SD)		Mean (SD)		Difference
Demographic registers					
Age at admission (years)	31.04	(10.17)	34.13	(11.38)	3.10***
Female	0.06	(0.25)	0.11	(0.31)	0.04***
Married	0.14	(0.35)	0.13	(0.33)	–0.02***
Cohabiting	0.18	(0.38)	0.13	(0.33)	–0.05***
Single	0.68	(0.47)	0.74	(0.44)	0.06***
No. of children = 0	0.80	(0.40)	0.82	(0.38)	0.02***
No. of children = 1	0.11	(0.31)	0.08	(0.28)	–0.02***
No. of children = 2	0.06	(0.23)	0.06	(0.23)	–0.00*
No. of children = 3	0.02	(0.14)	0.02	(0.15)	0.00*
No. of children = 4	0.01	(0.09)	0.01	(0.10)	0.00*
No. of children = 5+	0.00	(0.06)	0.01	(0.07)	0.00*
Non–western minority backgrounds	0.16	(0.37)	0.20	(0.40)	0.04***
Education registers					
Education (years)	9.39	(3.17)	9.20	(3.42)	–0.19***
Missing education	0.07	(0.26)	0.09	(0.28)	0.01***
Labor market registers					
Employed	0.31	(0.46)	0.28	(0.45)	–0.03***
Unemployed	0.10	(0.30)	0.10	(0.29)	–0.01*
Not in workforce	0.59	(0.49)	0.63	(0.48)	0.03***
Wage income (2010 PPP USD)	9,322	(17,201)	8,306	(15,768)	–1,015***
Criminal justice registers					
Sentence length (years)	1.11	(1.48)	0.49	(0.69)	–0.61***
Crime type = violence	0.53	(0.50)	0.47	(0.50)	–0.06***
Crime type = drugs	0.16	(0.37)	0.12	(0.32)	–0.04***
Crime type = property	0.16	(0.36)	0.18	(0.38)	0.02***
Crime type = other	0.15	(0.36)	0.23	(0.42)	0.08***
Previously incarcerated	0.64	(0.48)	0.61	(0.49)	–0.02***
No. of prev. convictions = 0	0.13	(0.34)	0.17	(0.38)	0.04***
No. of prev. convictions = 1	0.13	(0.34)	0.13	(0.33)	–0.01*
No. of prev. convictions = 2	0.12	(0.32)	0.11	(0.32)	–0.00*
No. of prev. convictions = 3	0.10	(0.30)	0.09	(0.28)	–0.01**
No. of prev. convictions = 4	0.09	(0.28)	0.07	(0.26)	–0.01***
No. of prev. convictions = 5+	0.43	(0.50)	0.43	(0.50)	–0.00*
Local measures (at admission)					
Local admission rate	2.42	(0.60)	2.53	(0.65)	0.11***
Local crime rate	100.46	(25.50)	104.55	(29.79)	4.09***
Local unemployment rate	634.57	(173.64)	664.26	(165.44)	29.70***
Observations	15,053		10,520		

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two–tailed tests).

B.III. Outcome Variables

We describe the outcome variables by whether they refer to the incarcerated individual or his or her visiting partner or children. For incarcerated individuals, we split the description by whether the outcomes concern the period a person is confined (based on the dataset from the DPPS), which included visitation information), or whether they concern the post release period (based on standard register data available from Statistics Denmark). Following the variable descriptions, we summarize their distribution by visitation status.

B.III.a Offender outcomes during confinement. The dataset from the DPPS contains measures related to conditions of confinement and in-prison conduct during each confinement spell in our data window. We use these to measure the potential effect of visitation on in-prison outcomes; for many of these, we measure the average of the outcome per month of incarceration to take differences in incarceration length into account. We include *disciplinary actions*. In Denmark, there are no supermax prisons in which solitary confinement are part of the punishment, and solitary confinement in punishment cell is instead considered the harshest disciplinary sanction an inmate can endure for in-prison misconduct. Other than punishment cell placement, inmates may be recorded as having experienced three types of disciplinary actions, namely conditional punishment cell, fines, and official warnings. We construct four variables measuring the average number of each of these disciplinary sanctions that an inmate has experienced per month of incarceration. We also include access to *leave from prison*. In Denmark, prisoners can apply for leave from prison. In closed prisons the leave is under strict surveillance by an officer and is relatively rare. In open prisons, leave is unsupervised if circumstances permit it and weekend leave is common. Leave is typically granted to visit family, to maintain ties with an employer, or to pursue education that is unavailable in the prison. We

include average number of leaves per month, total hours of leave per month, and the total length of granted leave relative to sentence length. We also include information specifically on weekend leave; average number of weekend leaves per month, total hours of weekend leave per month, and total length of weekend leave relative to sentence length. Last, we include a dummy variable for whether the offender achieved *early release on parole*, an indication of good in-prison behavior.

B.III.b Offender outcomes after release. The post release outcomes are all based on standard register data. Most of the registers which we rely on here were described above, under "control variables". We therefore simply mention these sources here, we do not explain them in detail; we do provide detail on sources that were not described above. From the population registers we obtain *relationship status* using two variables. Singlehood is a dummy variable indicating whether the offender, post release, is unmarried and not cohabiting with a partner. Re-partnering is a dummy variable indicating whether offenders, who had a romantic partner prior to admission (i.e. was married or cohabiting), has found a new partner. We construct this outcome by combining legal civil status and dates of any changes herein with the identification number of the recorded partner. We also include each offender's number of *residential moves* during the first two years following release, a gauge of residential instability. We obtain this information from the housing register, recorded by the Ministry of Housing, which contains unique identification numbers of all dwellings in Denmark, down to the apartment level, as well as the dates of any changes in who officially occupies these dwellings. From the labor market registers, we include four *labor market outcomes*. As defined under "Control Variables", labor earnings cover all legal income from work during the first full year following release and employment in late November is a dummy variable indicating that the offender holds a job as of late November

following release. We add two supplements to the November measure of employment. The first of these relies on the labor earnings outcome and is simply a dummy variable indicating whether the offender had any legal income from work during the first full year following release. The second of these relies on a monthly panel of reported salaries, administered by the Ministry of Employment and the Tax Authorities, and indicates whether the offender has employment at any point following release. The latter register is more detailed than the annual information we rely on for our other outcome measures of labor market outcomes, especially since it has better information on the timing of earnings relative to release. But it is not available for our full data window, only from mid-2008, which makes the annual measures our preferred ones. From the criminal justice registers (described above), we include three measures of *criminal recidivism*. The first one indicates reconviction within two years from release; the second one indicates re-arrest within two years, and the last one indicates that the offender was sentenced to a new prison sentence within two years from release. Last, from the National Patient Registry, we obtain indicators of *psychiatric treatment* within one and two years. The National Patient Registry is recorded by the Danish Board of Health Data and includes information on all contacts (consultancies, referrals, treatments) in the Danish hospital sector, including the reason for the contact and the outcome. The contacts are recorded using unique identifiers that are matched to the individual identifiers and can thus be merged with our data.

B.III.c Partner outcomes. We measure the outcomes of partners from the day the inmate was admitted to prison; otherwise it would be unclear which visit we should measure from for partners who visit prison more than once, just as it would be hard to decide when to start the follow up period for partners who do not visit the inmate at all. Partner outcomes cover the number of *residential moves* within two years, *labor market outcomes*, and *psychiatric treatment*

within one and two years; all defined as above. Last, we use four measures of *sick leave from work*. Here, we rely on official records of weekly social benefit receipt from the DREAM register, which was already described, and we single out sick leave benefits. In Denmark, sick leave benefits can be paid out after 30 days of absence from work and contingent upon a medical report from a general practitioner, and one sick leave period cannot exceed 22 consecutive weeks. As our measures of sick leave we use two binary indicators of having received sick leave benefits at all during one and two years, and two variables that measure the share of weeks during one and two years that the partner received sick leave benefits (i.e. total number of weeks on sick leave during one and two years divided by 52 and 104 weeks, respectively).

B.III.d Child outcomes. As for the partners, we measure child outcomes from the day the inmate was admitted into prison. We merge household IDs from the population register to income data (total income, including public benefits) from the Danish Customs and Tax Administration to indicate whether the child lives in a family with *household income lower than 50 percent of the median household income* in the population during the first full year after the admission. We use data recorded by the Ministry of Education to measure authorized and unauthorized *absence from school*, as well as total absence (authorized plus unauthorized), within three months from the inmate's admission date. As a measure of *juvenile crime*, we rely on a register of criminal charges against minors from the Danish National Police (age of criminal responsibility in Denmark is 15 years) to indicate whether the child was charged with a juvenile crime within two years. Last, we use data from the Danish Ministry of Education on *standardized test scores* in Mathematics and Danish. From 2008, all children in Danish public elementary schools were required to take such tests in second, fourth, sixth, and eighth grade, and we use the test score results from the first tests that followed the inmate's admission into

prison. We use two different measures of test scores for both Danish and Mathematics. The first version is simply the standardized test score, and we here measure whether prison visitation matters for a child's position in the overall rank of children. The second version focuses on the risk of performing poorly on the tests, by indicating whether the child scored among the 5 percent lowest on the tests. Note that because the tests were not implemented until 2008, we only observe this outcome for a subset of the children in our data.

*B.III.e Outcome variable distribution*s. Table B3 shows descriptive statistics of the outcome variables, split by visitation status. In general, around 4 percent experienced solitary placement in punishment cell and more than one in four received non-solitary sanctions, such as fines or warnings, for in-prison behavior. The amount of leave from prison is fairly high (more than one leave per month and for more than 24 hours on average), likely reflecting that a large share serves their sentence in open prisons were access to leave is less restrictive. More than half are released early on parole, which resonates with inmates in Denmark being expected to be released after having served two-thirds of their sentence (half time if good behavior, full time if bad). After release, most are single, and the rate of re-partnering is low (among those with a partner upon admission). Released inmates move around a lot and have very poor labor market attachment, and, as in many other countries, a large proportion recidivates. The partners move around quite a bit too yet are more attached to the labor market. And although a relatively high share received sick leave benefits during one and two years, which indicates substantial health problems, a relatively low proportion received psychiatric treatment. Turning to the children, around one in four grow up in poor households and around every tenth are charged with juvenile crimes. The children also on average fall below the national average on test scores. Comparing the outcomes across visitation status shows those who do not receive any visitation fare even

worse, except for on outcomes related to in-prison disciplinary actions, residential instability post release, and criminal recidivism, which may all be caused by the never visited on average being older at admission (as was shown above). For partners and children, we observe very little difference in the outcomes by visitation status.

Table B3. Descriptive Statistics of Outcome Variables, by Visitation Status.

	Visited			Never visited			
	Mean	(SD)	*N*	Mean	(SD)	*N*	Difference
Offender							
No. of solitary disciplinary sanctions per month	0.04	(0.15)	15,053	0.03	(0.17)	10,520	–0.02***
No. of non–solitary conditional disciplinary sanctions per month	0.06	(0.16)	15,053	0.04	(0.19)	10,520	–0.01***
No. of non–solitary disciplinary sanctions per month (fines)	0.27	(0.46)	15,053	0.23	(0.64)	10,520	–0.04***
No. of non–solitary disciplinary sanctions per month (warnings)	0.10	(0.24)	15,053	0.07	(0.32)	10,520	–0.03***
No. of leaves per month	2.27	(2.44)	15,053	1.26	(1.77)	10,520	–1.01***
Total hours of leave per month	45.06	(40.19)	15,053	29.24	(39.03)	10,520	–15.82***
Length of total leaves relative to sentence length	0.06	(0.06)	15,053	0.04	(0.06)	10,520	–0.02***
No. weekend leaves per month	0.68	(0.61)	15,053	0.43	(0.60)	10,520	–0.26***
Total hours weekend leave	36.29	(33.92)	15,053	23.34	(32.87)	10,520	–12.95***
Total length of weekend leaves relative to sentence length	0.05	(0.05)	15,053	0.03	(0.05)	10,520	–0.02***
Early release on parole	0.60	(0.49)	15,053	0.57	(0.50)	10,520	–0.03***
Singlehood	0.77	(0.42)	15,053	0.83	(0.38)	10,520	0.06***
Re–partnering	0.06	(0.24)	3,096	0.04	(0.20)	1,703	–0.02**
No. of residential moves	2.10	(1.22)	15,053	1.92	(1.15)	10,520	–0.18***
Labor earnings (2010 PPP USD)	9,984	(17,451)	15,053	9,556	(17,498)	10,520	–428*
Employment (labor earnings > 0)	0.43	(0.50)	15,053	0.42	(0.49)	10,520	–0.01*
Employed on last day of November	0.31	(0.46)	15,053	0.30	(0.46)	10,520	–0.01*
Employed at any point	0.35	(0.48)	10,900	0.26	(0.44)	4,705	–0.09***
Reconvicted within 2 years	0.38	(0.49)	15,053	0.37	(0.48)	10,520	–0.01*
Rearrested within 2 years	0.48	(0.50)	15,053	0.43	(0.50)	10,520	–0.05***
New prison sentence within 2 years	0.17	(0.37)	15,053	0.14	(0.35)	10,520	–0.02***
Psychiatric treatment within 1 year	0.05	(0.22)	15,053	0.05	(0.21)	10,520	–0.00*
Psychiatric treatment within 2 years	0.08	(0.27)	15,053	0.07	(0.26)	10,520	–0.01**
Partner							
No. of residential moves	1.97	(1.16)	3,096	1.74	(1.05)	1,703	–0.22***
Labor earnings (2010 PPP USD)	15,998	(19,377)	3,096	16,304	(19,795)	1,703	306

Employment (labor earnings > 0)	0.59	(0.49)	3,096	0.56	(0.50)	1,703	–0.03*
Employed on last day of November	0.49	(0.50)	3,096	0.50	(0.50)	1,703	0.01
Psychiatric treatment within 1 year	0.04	(0.20)	3,096	0.04	(0.20)	1,703	–0.00
Psychiatric treatment within 2 years	0.07	(0.25)	3,096	0.06	(0.24)	1,703	–0.01*
Sickness leave within 1 year	0.17	(0.37)	3,096	0.15	(0.36)	1,703	–0.02*
Sickness leave within 2 years	0.25	(0.43)	3,096	0.23	(0.42)	1,703	–0.02*
No. weeks on sick leave within 1 year / 52	0.04	(0.14)	3,096	0.04	(0.15)	1,703	0.00
No. weeks on sick leave within 2 years / 104	0.04	(0.12)	3,096	0.04	(0.13)	1,703	0.00*
Children							
Household income < 50% of median	0.26	(0.44)	3,180	0.22	(0.41)	2,271	–0.04**
Authorized school absence within 3 months	0.04	(0.04)	738	0.04	(0.04)	375	0.00
Unauthorized school absence within 3 months	0.01	(0.04)	738	n.a.[1]	(n.a.[1])	375	–0.00*
Total school absence within 3 months	0.05	(0.06)	738	0.05	(0.05)	375	–0.00
Juvenile charge within 2 years	0.08	(0.27)	1,479	0.11	(0.31)	1,074	0.03*
Math test score	–0.58	(1.02)	2,604	–0.64	(1.05)	1,722	–0.06*
In bottom 5% of math test scores	0.13	(0.34)	2,604	0.14	(0.35)	1,722	0.01
Danish test score	–0.52	(1.10)	3,280	–0.54	(1.13)	2,120	–0.02
In bottom 5% of Danish test scores	0.13	(0.34)	3,280	0.13	(0.34)	2,120	–0.00

Notes: [1] The Law of Statistics Denmark prevents us from publishing statistics in these cells as they are based on too few observations (fewer than 5 observations with outcome = 1).
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$ (two–tailed tests).

C. Additional Details on Occupancy Rates

We define the occupancy rate Z of each prison j in Denmark on day t as $Z_{j,t} = \frac{n_{j,t}}{N_{j,t}}$, which simply relates the number of inmates in a prison on a given day ($n_{j,t}$) to the capacity in that prison on that day ($N_{j,t}$). We here explain how we obtain these two numbers.

C.I.a Daily inmate counts. We rely on the incarcerations register, which the DPPS compiles from their management system and submit to Statistics Denmark each year. The incarcerations register contains admissions and releases from correctional facilities in Denmark from January 1st, 1991 and up to the latest updated version, which at the time of writing includes December 31st, 2018. Admission and release is recorded by individual (using the unique individual identifier that can be merged with other registers) and by facility (using unique facility identifiers), meaning that we can track which facility an inmate is held in at each day of his or her sentence (transfers across facilities are recorded as separate spells). This data structure makes it straightforward to count the number of inmates held in each facility on each day, ($n_{j,t}$).

C.I.b Daily prison capacity and occupancy. The DPPS only reports prison capacities on one day each year (towards year-end) and as an annual average. We thus do not directly observe the daily capacity, $N_{j,t}$. We do observe from the DPPS's annual statistics that there are substantial differences between the reported year-end capacity and the average capacity across the year within prisons, just as we observe variation in these numbers across years. As our identification strategy focuses on the daily fluctuation in the occupancy rate of the nearest relevant prison, we opt not to rely on the aggregate official capacity numbers from the DPPS (which would produce limited variation in $N_{j,t}$) and instead estimate the daily capacity from the data. To do so, we use a percentile measure based on the number of inmates in each prison

during a period prior to any given day *t*: $Z_{j,t}^{\tau} = \frac{n_{j,t}}{N_{j,t^*}^{\tau}}$. Here, τ denotes the τ-th percentile of the number of inmates in prison *j* during the period *t** (which precedes *t*). The idea is that the DPPS are likely to observe the number of inmates in a prison over a period just prior to assigning a new inmate to a prison, and to compare the number of inmates on the assignment day to that preceding period. If there are many inmates in a prison on day *t* relative to the number of inmates in that prison over the preceding period (*t**), the DPPS is less likely to send the new inmate to that prison. If, however, there are relatively few inmates in the prison on day *t* compared to the what was observed in that prison over the preceding period, the inmate is more likely to be assigned there. As explained in the main text body, our measure of the occupancy rate then becomes $Z_{j,t}^{95} = \frac{n_{j,t}}{N_{j,t^*}^{95}}$, where $n_{j,t}$ is the number of inmates in prison *j* on day *t* and N_{j,t^*}^{95} is the 95th percentile of the number of inmates in prison *j* over the preceding time period *t**. We have checked the robustness of our results by also running the analyses using different percentiles $(50, 75, 90, 95, 99)$, something which did not change our overall conclusions to any substantial degree but did affect the strength of the instrument for the lower percentiles.

Our definition of *t** rests on the case type and when the assignment took place. In Denmark, where inmates are randomly transferred directly from the sentencing court to prison, inmates must be informed about their admission date and the assigned prison no later than 30 days before admission for inmates serving a sentence for most crime types. For specific crime types (mainly violent crimes and weapon's possession), however, the notice is only 10 days. Since 2000, the rules that govern the length of the period of notice have changed a few times, which we summarize in Table C1, and we define *t** accordingly.

Table C1. Required period of notice about assigned prison and admission date, Denmark.

	Required notice period	
Date	At least 30 days	At least 10 days
May 31, 2000	All	
May 17, 2001	All other	Violent crimes; Technical violation of probation
June 17, 2005	All other	+ Rape; Technical violation of electronic monitoring
January 22, 2009	All other	+ Possession of firearms or explosives
December 5, 2016	All other	+ Illegal possession of knife
April 3, 2017	All other	+ Illegal immigration

Notes: The specific legal acts (in Danish) which the dates in the table refer to may be obtained from the corresponding author.

D. Measurement Error in the Relationship Between Distance to Nearest Relevant Prison with Space and Actual Distance from Home

In the main text, Section III.b, we describe how our data contain information on some of the criteria that the DPPS uses to assign inmates to prisons in Denmark, which we use to estimate which prisons are relevant for each inmate. We use information on facility type, sentence length, age, and the occupancy rate at the prisons, as mentioned. Yet two sources of error may impact our estimate of the nearest relevant prison with space. First, we do not have perfect information about which prison is the nearest relevant one (we cannot take employment prospects into account, for example). Second, during our data window, some prisons had both open and closed wings (see Table D1), and because we in our analyses define a prison's security level from whether it is primarily open or closed, some prisons are not counted as relevant to inmates assigned to closed prisons although in fact they were (and vice versa). We chose to focus on the primary security level of the prisons because the vast majority of cells would typically fall into this security level.

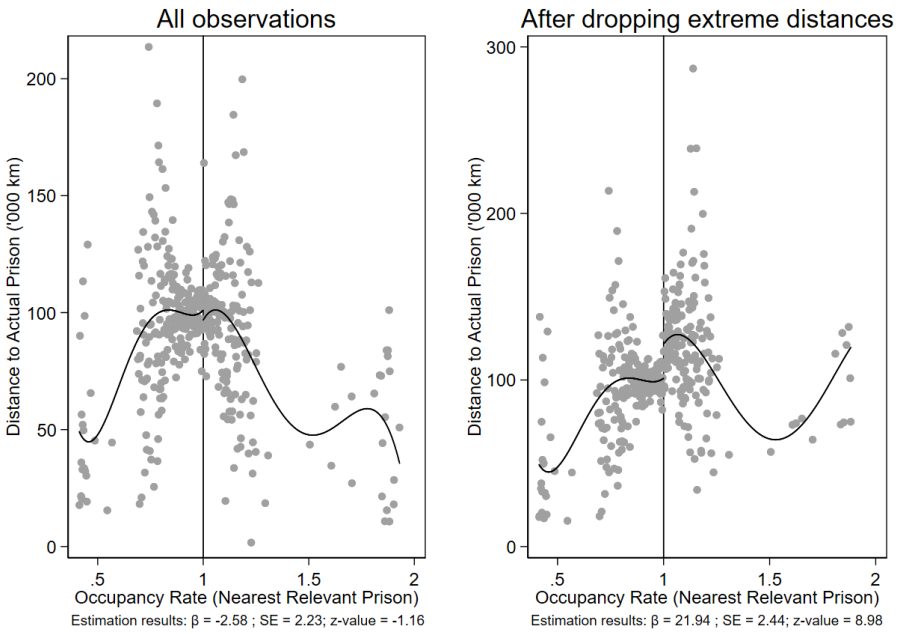
Table D1. Overview of the Prisons in our Analytical Sample

Prison	Observation Period	Security Level
Møgelkær	2004-2014	Open
Renbæk	2004-2014	Open until December 1, 2011 then both open and closed
Horsens	2004-2006	Closed, shut down on October 1, 2006
Østjylland	2006-2014	Closed, starts on October 1, 2006
Jyderup	2004-2014	Mainly open but has closed wing
Nyborg	2004-2014	Closed
Ringe	2004-2014	Closed
Vridsløselille	2004-2014	Closed
Kragskovhede	2004-2014	Open but has closed wing from May 2005
Søbysøgård	2004-2014	Open
Horserød	2004-2014	Both open and closed
Sdr. Omme	2004-2014	Open

Notes: We exclude "Midtjylland (incl. Nr. Snede)" from the table. This prison was used both to house inmates and as a transit prison, meaning that many inmates would be placed there only to be transferred to their intended prison. As we cannot distinguish these inmates clearly in the data, we opted to exclude the prison altogether.

Because of these measurement errors in prison relevance, a minority of inmates (16.4 percent) end up with negative estimates of distance between the nearest relevant prison that has space and the prison to which they are eventually assigned. Some of the negative distance are extreme (exceeding –250 kilometers), which impacts the strength of our instrument significantly. To minimize the impact of this measurement issue, we deleted observations with negative distances (numerically) exceeding the median of all negative distances in the data (median is 56.2 kilometers), which corresponds to 8.2 percent of the data (see Table B1). Figure D1 shows the importance of deleting these extreme observations by plotting average distance from inmates' home to the prison they were eventually assigned by the occupancy rate at the estimated nearest relevant prison.

Figure D1. Distribution of Distance to Assigned Prison Including and Excluding Extreme Observations.



Notes: Beta estimates, standard errors and z-values noted below the x-axes refer to the potential level shift in distance from home around occupancy rate 1 at the nearest relevant prison. Lines are fitted using local polynomial smoothing.