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PRETRIAL DETENTION AND THE COSTS OF SYSTEM OVERREACH FOR EMPLOYMENT AND FAMILY LIFE

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for Employment and Family Life

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ABSTRACT

Using unique Danish register data that allows for comparisons across both conviction and incarceration status, this paper analyzes the association between pretrial detention and work, family attachment, and recidivism. We find that pretrial detention may impose unique social costs, apart from conviction or additional punishments. Most notably, men who are detained pretrial experience poorer labor market trajectories than men who are convicted of a crime (but not incarcerated). Importantly, this result holds even for men who are detained pretrial but who are *not* convicted of the crime. Consistent with prior research, we also find that pretrial detention is unrelated to later family formation but might disrupt pre-existing household arrangements. Finally, the association between pretrial detention and work and family life are not counterbalanced by reductions in recidivism. Men who should not have been incarcerated, and hence should not have to suffer the damaging effects of this experience, nonetheless experience the denial of liberty associated with incarceration as well as longer term social harms.

INTRODUCTION

Pretrial detention, or incarceration prior to a legal finding of criminal responsibility, is common the world over. In most countries, between 10 and 40 percent of all prisoners are pretrial or remand detainees, with the highest rates in Africa and parts of Asia (Walmsley 2016a). Pretrial detention is thus common but represents an understudied experience in the broader literature on the social effects of criminal justice expansion.

We analyze the effects of pretrial detention, with and without conviction, to more clearly distinguish effects that flow from the imposition of a criminal label from those that flow from incarceration and additional punishment experiences. Specifically, we exploit the detailed nature of register data from Denmark to compare work, family, and recidivism outcomes for four groups that are difficult to isolate from one another in other available data sources: 1) Those who are detained pretrial yet not convicted; 2) those who are convicted and serve their full sentence pretrial; 3) those who are detained pretrial and then sentenced to an additional term of imprisonment; and 4) those who are pretrial detained and then sentenced to other sanctions (primarily probation). We compare these four groups to a group of people who are convicted but never incarcerated.

We find that pretrial detention may impose unique social costs, apart from conviction or additional punishments, and reduces labor market and family attachments with no corresponding reduction in recidivism for detainees, to the degree that our results can be given causal interpretation. Our results highlight an important consequence of system overreach in the mass incarceration era; men who should not have been incarcerated pretrial, and hence should not have to suffer any damaging effects of this experience, nonetheless experience both the denial of liberty associated with incarceration as well as longer term harms once released.

In the sections to follow, we first describe pretrial detention and the Danish context for readers who may be less familiar with it. We next describe our analytic strategy and the difficulties of establishing salient comparisons in the absence of random assignment to differential criminal justice outcomes. We then offer evidence on the relationship between pretrial detention and labor market outcomes, family attachment, and recidivism. In so doing, we aim to broaden the lens of collateral consequences research to include forms of carceral contact, like pretrial detention without conviction, that are often hidden from view in contemporary datasets.

BROADENING THE LANDSCAPE OF INCARCERATION

Increases in incarceration across Western democracies is well known. In the most extreme example, the imprisonment rate in the United States grew from a relatively stable 105 per 100,000 to a peak of 504 per 100,000 in 2008. By 2012, roughly one in every 36 adults was in a prison, jail, or serving a probation or parole sentence (Kaebel et al. 2016). The widespread experience of arrest (Brame et al. 2011) and entanglements related to misdemeanors (Kohler-Hausmann 2013) further detail the long reach of the criminal justice system into American life. Criminal justice expansion, albeit smaller in magnitude, is evident in the incarceration rates across other Western democracies, including England and Wales (148 per 100,000), the Netherlands (69 per 100,000), and, our research site, Denmark (61 per 100,000) (Walmsley 2016b).

The rapidly expanding literature on the 'collateral consequences' of incarceration suggests that over-incarceration results in widespread social harms (Comfort 2007, National Research Council 2014, Ramakers et al. 2014, Turney 2015, Turney and Connor 2019,

AUTHOR DATE, Wildeman and Muller 2012). Scholarship on high incarceration rates is focused on relatively few countries however, with the lion's share commenting on the United States, reflecting the extreme punishment rates there. Such a parochial focus makes good sense given how extreme the United States is within the carceral landscape but also comes with significant limitations. Most notably, the data infrastructure in the United States is sorely lacking with respect to variation in incarceration experiences. Relatively few data sources distinguish cleanly between various forms of incarceration or provide information on sentence length and a variety of outcomes (AUTHOR DATE).

A salient example of data limitations is the focus of our paper: jail incarceration and pretrial detention. The most common form of incarceration in the United States – short stints served in local jails, often prior to conviction – is among the least studied experience (Turney and Connor 2019). Pretrial detention is thus an important but poorly understood feature of the criminal justice system. It is thought to protect the public from potentially dangerous people during the adjudication process and ensure that defendants appear for trial. Yet it also represents a fundamental and relatively unexamined denial of liberty, with no corresponding finding of criminal responsibility to justify its use. In an era where millions are exposed to the pains of incarceration for increasingly lengthy periods without a finding of criminal responsibility, the social consequences of pretrial detention are of substantial importance.

Research on the consequences of pretrial detention, relative to other forms of incarceration, for social outcomes is scant, though a robust literature has focused on its constitutionality (Heaton, Mayson, and Stevenson 2017, Mitchell 1969), influence on guilty pleas, conviction, and incarceration (Lowenkamp, VanNostrand, and Holsinger 2013, Rankin 1964, Stevenson 2018, Williams 2003), the extra-legal predictors of detainment (Demuth 2003,

Spohn 2009, Ulmer 2012), and the role bail systems play in punishing poverty (Gupta, Hansman, and Frenchman 2016, Scott-Hayward and Ottone 2018, Stevenson 2018). Most relevant for the analysis to follow, critics of pretrial detention link it to worse case outcomes, often especially among low risk defendants (e.g., Dobbie, Goldin, and Yang 2018, Goldkamp 1983, Lowenkamp, VanNostrand, and Holsinger 2013, Stevenson 2018, Ulmer 2012). Such findings can be interpreted in a multitude of ways, of course. Those who are detained may be more likely to recidivate. It remains the case, however, that research on pretrial detention has tended to focus on case outcomes and the management of risk, rather than harms to social life.

The lack of research on pretrial detention contrasts sharply with a robust literature on the social consequences of post-conviction incarceration. Incarceration and criminal conviction worsen labor market outcomes for the formerly incarcerated (Andersen 2015, Apel and Sweeten 2010, Harding et al. 2018, Pager 2003, Western 2006; but see Loeffler 2013). The same is true for family outcomes; a number of studies document the strains associated with incarceration for romantic relationships (Comfort 2007, AUTHOR YEAR, Massoglia, Remster, and King 2011) but few of them focus on pretrial detention specifically and it is often poorly measured (but see Apel 2016, Harding et al. 2018, Sugie and Turney 2017).

A more recent study (Dobbie, Goldin, and Yang 2018) focused squarely on pretrial detention offers evidence on two of the three outcomes of interest to this paper. Leveraging variation among bail judges in the likelihood of imposing pretrial detention in two US jurisdictions, Dobbie et al. (2018) found those who were detained pretrial were more likely to plead guilty, less likely to be employed, and earned less if employed following release.[1] Whereas

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[1] Dobbie et al. (2018) find that "the leave-out leniency measure [of bail judges] is highly predictive of detention decisions, but uncorrelated with case and defendant characteristics" (2018:203), suggesting critics of pretrial detention are correct when they note little relationship between detention decisions and risks to the public.

pretrial detention was linked to worse outcomes, it failed to reduce recidivism. Such findings highlight the mixed nature of pretrial detention; while it does hold defendants in place prior to case disposition, it appears to be relatively unconnected to either prior criminal history or risk of future crime.

A critical distinction between the Dobbie et al. study and ours, however, pertains to which marginal defendant is analyzed. Dobbie et al. analyze the effects of pretrial detention for the subgroup of defendants and cases where pretrial detention is based on a qualitative evaluation by the judge and where the judge's pretrial detention decision could – depending on the judge's leniency – go either way. They do so because their identification strategy is very strong for this subgroup. Most pretrial detention decisions do not fall on this margin, however, as the characteristics of some defendants and/or cases (almost) always lead to pretrial detention.

In our study, as we explain in detail below, the focus is on defendants who are all detained pretrial but who differ in their conviction and sentencing outcomes. As such, although our analytical strategy relies on common trends assumptions (which are fundamentally untestable) and does not benefit from semi-random assignment of pretrial detention, results from our analyses provide new knowledge on the potential consequences of pretrial detention in cases where pretrial detention is either justified or not. From a policy perspective, it is imperative that we have knowledge on this margin, as such knowledge is instructive on the consequences of system failure in the form of overzealous detention decisions. In the analysis to follow, we add much needed detail on the relationship between pretrial detention and the social consequences of incarceration.

CONTEXT, DATA, AND METHODS

Context

Pretrial detention in jail settings is common but less widely appreciated is that it may serve as a more severe form of incarceration (May et al. 2014, Toman, Cochran, and Cochran 2018, Turney and Connor 2019). Pretrial detainees are often held in local jails or detention centers, rather than in prisons, and detention in facilities of this type may be substantially different. For example, the population of local jails in the United States tends to be more heterogenous and overturn rapidly, resulting in conditions that are far less stable relative to prisons. Partially as a result of the high rate of turnover, those held in local jails have access to fewer services and programs as well as fewer opportunities for connection to family and friends outside. Finally, jails are often characterized by high rates of solitary confinement, severe mental health problems among its residents, and unsafe conditions (Haney et al. 2015, Smith 2006, Teplin 1984, Turney and Connor 2019).

Lacking US-based data that combines pretrial detention data with social outcomes of interest like employment and family attachment, we utilize registry data from Denmark. The scale of contact with the criminal justice system is lower in Denmark than in the US, as mentioned, and the conditions of confinement differ markedly across these countries. During post-conviction incarceration in Denmark, for example, incarcerated people are offered a range of resocialization initiatives, such as education and employment training. Imprisonment in Denmark is structured so as to resemble life outside prison (deprivation of freedom is the punishment, rather than enduring harsh prison conditions as in the United States), and incarcerated people are paid a small salary for participating in resocialization initiatives, allowing them to buy groceries and cook their own meals in common prison wing kitchens. The

average sentence length in Denmark was 8 months in 2016 but more than half the sentences were shorter than four months that year (Danish Prison and Probation Service 2017).

Whereas incarceration rates and prison conditions differ greatly between the US and Denmark, pretrial detention in Denmark is arguably comparable to (or harsher than) detention in the United States. Pretrial detention in Denmark is marked by incarceration in separate pretrial detention centers and private cells. Lengthy solitary confinement is common, with inmates locked in their cells up to 23 hours a day and one hour of fresh air, alone, each day (Andersen et al. 2003, Sestoft et al. 1998, Smith 2006).[2] To avoid the risk of collusion (detainees aligning their testimonies), resocialization initiatives are offered only to the limited extent that these can be carried out in the private cell. Visitation is restricted and detention staff often monitors phone calls, visits, and communication. Pretrial detention periods are often lengthy; median time from facing criminal charges to adjudication in Denmark is 64 days for pretrial detainees, whereas it is 112 days for released defendants.[3] The corresponding median periods in the US are 45 and 127 days, respectively (Cohen and Reaves 2007). 21.1 percent of all incarcerated people are pretrial detainees in the United States. In Denmark, perhaps reflecting its smaller overall prison population, 31.1 percent of all prisoners are pretrial detainees (Walmsley 2016a). Although the scale of contact with the criminal justice system is lower in Denmark, one in three inmates on any given day is a pretrial detainee, and just like in the US, relatively few (about 16 percent) pretrial detainees are charged with violent crimes (Danish Prison and Probation Service 2017). Finally, around 20 percent of pretrial detainment cases in Denmark fail to lead to a prison sentence (Smith and Jakobsen 2017).

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[2] To break away from these solitary conditions of pretrial detention there has been a recent development in Denmark to increase pretrial detainees' possibilities of visiting each other in their private cells.
[3] Own calculation based on data from Statistics Denmark. Specifically, the median time from charge to adjudication in Denmark is found for cases with conviction of the Penal Code during 1995-2010.

Data

We exploit Danish register data. Using register data for research purposes has become increasingly common because of their accuracy, flexibility, and level of detail. In Denmark, tax records, contact with the criminal justice system, life events and so on are linked to a unique personal identification number by the relevant agencies and reported to Statistics Denmark, the national statistical agency. Statistics Denmark documents the content of each variable, notes potential discontinuities in variables over time, and makes individual-level, de-identified data available to researchers for specific research questions. Data are available for the entire population and across years (most registers are available since 1980). In the context of our study, for example, we were able to establish exactly the relevant comparison groups for analyzing the association between pretrial detention and work, family attachment, and recidivism while taking the impact of conviction into account.

Comparison Groups. We rely on three criminal justice registers to construct our comparison groups. From the incarcerations register we obtain all incarceration spells that were initiated and ended during 1995-2010 that included a period of pretrial detention. The incarcerations register holds information on all admissions and releases from correctional facilities in Denmark, including from pretrial detention centers, and exact dates differentiate between arrest, pretrial detention, and serving a sentence. From the charges register, we merge information on crime type (as per the charge) and offense date. From the convictions register, we obtain information on case outcomes. Here, a particularly important distinction is the one between whether the defendant was found guilty of the crime (by bargain or in court) or not (found not guilty or case dropped), as well as the sentencing outcome. These distinctions are important because they, in

combination with the criminal justice registers, allow us to distinguish between five groups: 1)

Pretrial detainees who were *not* convicted; 2) pretrial detainees who were convicted and served

the full sentence pretrial; 3) pretrial detainees who were convicted and then sentenced to

additional imprisonment; 4) pretrial detainees who were convicted and then sentenced to other

sanctions (primarily probation); and, last, 5) those who were convicted but never incarcerated;

this group serves as the referent category.[4]

Outcome Variables. We focus on three domains that could be affected by pretrial detention:

Labor market outcomes, family outcomes, and recidivism. We measure the outcome variables

during the three years preceding admission and the three years following release.

Two variables measure labor market outcomes. The first measures monthly labor

earnings, i.e., earnings obtained from employment. We report earnings in 2010 OECD individual

consumption Purchasing Power Parity (PPP) adjusted USD (e.g., the 2010 earnings in USD

while taking fluctuations in local prices into account). The second denotes monthly employment,

measured by a binary indicator of whether the person had any income from work during the

month. Information on earnings come from tax records and includes all sources of legal income

from work. The measure is annual and we split the annual numbers by 12 to achieve average

monthly earnings. Because we have precise incarceration and release dates, we can then note

average monthly earnings prior to admission and following release. For years which precede or

follow the year of the incarceration, we are certain that the earnings in fact either precede or

follow incarceration. The same is true for incarceration spells that include January 1st. Spells

initiated and terminated during the same year present a challenge regarding the timing of

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[4] Those who were convicted of a crime but who were not incarcerated are a random sample (drawn from the population of only convicted offenders) to reflect the distribution of offenses across time among those who experienced pretrial detention.

earnings relative to the timing of incarceration. Our solution is to count as pre-admission earnings the number of pre-admission months during that year and assign each of these months the average monthly earnings during the year. We do the same post-release. As a consequence, our earnings measure is imprecise during a period up to +/- 11 months from admission and release. In the graphical material we present, we mark these months that are subject to imprecision and exclude these months from our statistical models.

Two variables measure family outcomes. The first measures whether a person lives with a partner, the second measures whether a person lives together with his children. We construct these measures from the housing registers and demographic registers; living with a partner includes both marital and non-marital relationships and living with children measures whether one shares an address with one's own children. The housing register provides exact dates on when people move, allowing us to take move timing relative to incarceration timing into account.

Two variables measure recidivism. The first measures criminal conviction, the second measures arrest. Criminal conviction measures whether one is found guilty of violating the Penal Code during each of the three years preceding and following incarceration. Importantly, we use the charges register to obtain offense dates; we use timing of the offense rather than the timing of conviction (which may vary with court caseloads and the like). For arrests we exploit the details of the incarceration register to note whether a person was arrested during each of the three years preceding and following the incarceration in question. Importantly, we use case IDs to ensure that our outcome measures of criminal conviction and arrest exclude contact with the criminal justice system that is recorded on the same case ID as the case that selects them into the sample in the first place.

Background Characteristics. Our analytic strategy does not strictly require control variables because it focuses on individual change. There are two reasons why we include a range of background characteristics, however. First, controlling for background characteristics increases the precision of our estimates by reducing the error term variance of our statistical model. Second, background characteristics allow us to describe how the people in our comparison groups differ, thus providing a better understanding of the social mechanisms that select people into each of the comparison groups.

From the demographic registers we add age at the time of the criminal charge, parental status, and ethnic minority background (in Denmark, this indicates that either the focal person or their parents immigrated from a non-Western country). From the educational register we add years of schooling (and a dummy variable indicating missing educational information for those who do not show up in the register). From the criminal justice registers we add number of previous convictions, number of incarceration experiences, whether the current incarceration is a person's first, second, or third or more. We also add details of the current incarceration, namely length of incarceration (in months), how often the detainee was transferred to another facility, and facility type (local arrest, low or high security, or other facilities). Last, we include crime type from the criminal charge: violent, property, or other crime.

Sample. From the criminal justice registers we select cases that include a period of pretrial detention of seven days or more. We use this cutoff because the shortest possible prison sentences in Denmark are seven days, and our analyses rely on comparing pretrial detainees who are found not guilty to pretrial detainees who end up serving their full sentence pretrial. Serving a full sentence pretrial is simply not possible for less than seven days. For the 'convicted only' comparison group, we select cases resulting in conviction but with no incarceration (except arrest

for less than 24 hours). We include only cases initiated and terminated within the 1995-2010 window. We restrict our sample to include only men because only around 5 percent of prison sentences in Denmark are handed to women. Last, we focus only on those 18-59 years old at admission because pretrial detainees younger than 18 in Denmark are detained in secured youth custody – which differs substantially from the experience we are interested in analyzing (Bengtsson 2012).

Method

Our analyses proceed in several steps. First, we present descriptive results showing how our comparison groups differ prior to detention and following release, using labor market outcomes as a salient example. We then present descriptive statistics of the background variables by comparison group. The point is to show that substantial differences exist across the comparison groups even before incarceration. The preadmission differences come as little surprise as there are important mechanisms selecting specific men into each comparison group both in observed characteristics (which we show in the first analytical step) and in characteristics that are unobserved in the data.

Second, the descriptive differences across comparison groups highlight the importance of using an analytic approach that takes individual change in the outcomes into account rather than focusing only on post release differences: the focus on individual change allows us to factor in the differences (observed and unobserved time-invariant) between the comparison groups and provide unbiased average estimates (for as long as all selection into the comparison groups is random conditional on background characteristics and outcomes prior to detention, otherwise

known as the conditional independence assumption). To estimate the average effect of pretrial detention on labor market outcomes, we model the change in individual outcomes as follows:

$$y_{ict} = \alpha_t$$
$$+ \beta_t Pretrial_{ict}$$
$$+ \gamma_t Pretrial \times Notconvicted_{ict}$$
$$+ \delta_t Pretiral \times Prison_{ict}$$
$$+ \theta_t Pretrial \times Probation_{ict}$$
$$+ \pi X_{ic} + \rho YM_{ic} + \varepsilon_{ict}$$

Here, y_{ict} denotes the outcome under study for individual i prior to ($t = 0$) or following ($t = 1$) the case (c) in question. The model is set up such that the intercept (α) measures the preadmission average for the relevant outcomes for the convicted only group when $t = 0$, and measures the change in that outcome for that group from before to after conviction when $t = 1$. Because all groups except the convicted only group experience pretrial detention, $\beta_{t=0}$ measures the preadmission difference in outcomes between the convicted only group and the group who serve their full sentence pretrial. Correspondingly, $\beta_{t=1}$ measures any changes from before to after incarceration for the latter group in addition to the change that the convicted only group experience. Because of the additive structure of the model, the parameters associated with the remaining pretrial detention groups measure the level difference ($t = 0$) and change ($t = 1$) for these groups in addition to what was just described for the group that serve the full sentence pretrial. For the *Pretrial × Notconvicted* group, for example, $\gamma_{t=0}$ measures any level difference before incarceration between those who serve their full sentence pretrial and those who are pretrial detained but not convicted. And $\gamma_{t=1}$ measures any change to the outcome from before to

after detainment for the pretrial detained but not convicted group which is added to the change experienced by those who serve their full sentence pretrial (and which is already measured relative to the change for the convicted only group). In this way, the model measures any additional effects of pretrial detention with and without conviction while taking the general trend from $t = 0$ to $t = 1$ for the convicted only group into account. For those who are sentenced to imprisonment or other sanctions in addition to experiencing pretrial detainment, δ and θ do the same. These last two comparison groups and corresponding parameters are mainly included in the model to provide a full picture of how the focal outcomes respond to different types of contact with the criminal justice system. Last, X is a vector of control variables (which we have already described under Background Characteristics), YM denotes year and month fixed effects to take general time trends into account, and ε is the model's error term. Standard errors are clustered at the individual and case level because all individuals appear in the data twice ($t = 0$ and $t = 1$) for each of their cases (c) and that we estimate the models using OLS.

There are two fundamental identifying assumptions behind our first statistical model for measuring the effect of pretrial detention net of conviction. The first is one of common trends, and in our case, we rely on two common trend assumptions. First, we expect a general increase in labor market outcomes with age, and we therefore assume that the group of men who were convicted but not incarcerated can be said to express this general trend. Essentially, we assume that the other comparison groups would have experienced the same trend in outcomes had they not experienced any additional contact with the criminal justice system. Second, to tease out the consequences of pretrial detention with and without conviction, we assume that the trend in outcomes from before the criminal case to after pretrial sentence expresses what the trend in outcomes would have been for the pretrial incarcerated but not convicted group if they had been

convicted of the crime. We also compare the trends in outcomes for the last two comparison groups (sentenced to other sanctions and sentenced to additional post-conviction imprisonment) to the one for those who served their full sentence pretrial, effectively allowing these trends to express any additional consequence that either imprisonment or other sanctions have over and above pretrial detention.

The second identifying assumption behind our statistical model concerns the exogeneity of the conviction decision, conditional on background characteristics and the common trends that were just laid out. We cannot rule out that differences between the comparison groups that are unobserved in the data but correlate with both conviction decision and outcomes may drive some of our results. When we talk about effects of pretrial detention with or without conviction, this important caveat must be kept in mind: we compare changes in outcome trends among these two groups of detainees who are comparable in terms of observed characteristics and pre-custody trends, yet other an unobserved (e.g., time-variant) differences across the group may be driving some of the results. To gain a feeling for whether the results from the models we just presented could be driven by such unobserved differences between the people in our data (or, to be specific, differences across people in how the dependence between any unobserved individual component and the observed covariates in the model pan out)), we supplement our statistical model with an individual level fixed effects specification. Here, we assign an intercept to each person in the data and then observe how the outcomes change as each month passes. Whereas the statistical model defined above compares the average outcome of an individual after release to the average outcome of the same individual before admission (i.e. two observations per person), we here exploit the full panel to have several observations per person (i.e. one observation per month per person). The fixed effect model thereby measures whether we observe a change in

individual trajectories in the outcomes that corresponds with the timing of

incarceration/conviction, measured relative to each individual's starting point and development

in the outcome.[5] Importantly, the individual fixed effects strategy does not rule out the possibility

that unobserved and time-variant factors may still be driving our results for the "effect" of

pretrial detention on outcomes. As such, whereas large differences between estimates from the

individual fixed effects strategy and our main results would imply substantial risk that our main

results could be biased, a lack of differences between the estimates is no guarantee of the

opposite.

Third, we focus on people who had family attachment just prior to being taken into

custody (i.e., people who were living with a partner and/or living with their children). We

evaluate whether their risks of losing this family attachment differ following release depending

on the type of contact with the criminal justice system that they experienced. We estimate Cox

proportional hazard models to measure how the risks of losing family attachment depend on type

of criminal justice contact while controlling for background characteristics.[6]

Fourth, we summarize the results from a number of robustness checks. These checks

serve to show that our main results are general and hence not driven by any specific criminal

offense type, facility type, or outcome measurement window.

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[5] This specification also implies that we cannot control for time-invariant factors – which in this context cover background characteristics and other time constant variables such as dummies for group membership prior to admission – as the effects of these factors are soaked up by the individual fixed effects.

[6] The Cox proportional hazards model relies on the basic assumption of proportionality in the hazard rates between the groups over the follow up period. We test this assumption by regressing the scaled Schoenfeld residuals from the regressions on the time variable to observe whether the corresponding slope coefficient is zero. After allowing the impact of time from release to vary for age, being charged of property crime, and total number of incarcerations a person has experienced we cannot reject this null hypothesis.

RESULTS

Descriptive Statistics

Figure 1 presents monthly average earnings (panel a) and the monthly share with labor earnings (panel b) up to 36 months before and after the case. Focusing on the period after the case, the group that experienced both pretrial detention and a sentenced term of imprisonment fares worst across both the amount of earnings and employment. Their employment rate is around 30 percent when released from prison, half that of the convicted only group. The convicted group that served their entire sentence pretrial follows the same pattern. Their outcomes following the case are, however, slightly better, most likely because they are sentenced to shorter terms that do not exceed their period of pretrial detention. At the other extreme, those who are only convicted (but never incarcerated) fare the best across both labor market outcomes. In between these extremes fall two groups: Those who are detained pretrial and then sentenced to other sanctions fare the same as those who are pretrial detained but not convicted. The latter result most likely reflects the highly select nature of this group; probationers, for example, differ from prisoners on many parameters as well.

[Insert Figure 1 about here]

It is evident from Figure 1, however, that large differences exist between the groups before the case in question. The sorting of groups in terms of the outcomes is similar to the post release pattern just described, underscoring how important it is to analyze individual change in outcomes rather than simply post release differences in a setup such as ours. Over the observation period, the average labor earnings increase for the convicted only group, and the earnings response to

conviction is negligible (panel a), although there is a fairly strong downward slope to the share of these men who actually have income from work (panel b). For the other groups – the groups that experience pretrial detention – we observe a stronger labor market response. Pretrial detainees who are sentenced to imprisonment have the worst labor market outcomes, whether or not the full sentence is served pretrial or there is additional imprisonment following conviction. This finding makes sense as people who are sentenced to imprisonment are generally worse off. Note the stronger discontinuity in the share with labor earnings (panel b) for those who are sentenced to additional imprisonment than for those who serve the full sentence pretrial, likely reflecting their longer absence from society. The developments in labor market outcomes for the group that is detained pretrial but not convicted and the group that is detained and sentenced to community sanctions are, again, fairly comparable. Both their monthly average earnings and the share with labor earnings in these groups fall in-between the group that is only convicted and the two groups that are sentenced to imprisonment.

The differences just shown could simply reflect that the comparison groups are made up of vastly different men. We therefore compare means and standard deviations by comparison group across background characteristics in Table 1. Results support the claim that there are compositional differences between the groups. The groups of pretrial detainees who either are not convicted or who end up having served their full sentence pretrial, for example, are much smaller than the other groups. As was also reflected in the outcomes, those who serve their full sentence pretrial, on average, resemble those who are detained pretrial and then sentenced to an additional term of imprisonment, except in highly predictable ways; the additional imprisonment group is of course incarcerated for longer and they tend to experience more transfers between

facilities because almost all men in the pretrial sentence only group serve their time in a local arrest facility.

The composition of the group that experiences pretrial detention but not conviction generally falls in-between the composition of the convicted only group and the group that is detained pretrial and then sentenced to other sanctions. A few important exceptions stand out, however, perhaps indicating the pretrial detained but not convicted group is to some degree made up of people with more criminal justice experience and comprised of a larger share of ethnic minorities, echoing findings from a previous study showing ethnic disparities in groundless arrest and pretrial detention in Denmark (Holmberg and Kyvsgaard 2003).

The lower rows of Table 1 list the distributions of our outcome variables. Focusing on outcomes related to family life and criminal recidivism (labor market outcomes were described in detail in the figure above), it is evident that although there are differences between the comparison groups in these outcomes both prior to incarceration and following release, the "response" to the incarceration experience seems to be fairly similar across the groups. As such, we do not expect to find any notable consequences of pretrial detention on the change in these average outcomes from before incarceration to following release.

[Insert Table 1 about here]

Estimation Results

Labor Market Outcomes. Table 2 presents the results from our statistical model that measures the change in labor market outcomes for the comparison groups. For each of the two labor market outcomes, the first model ((1) and (4)) reports raw correlations, i.e. models without control variables. The second model ((2) and (5)) controls for background characteristics and time fixed

effects, whereas the last model ((3) and (6)) reports results from models which include individual level fixed effects (hence the larger number of observations). Focusing on the raw correlations before admission and only on labor market earnings, the groups that will become detained pretrial have lower average earnings, more than USD 450 lower per month relative to USD 900 for the convicted only group. Because of the structure of the model, this preadmission level difference pertains only to the group that serves their full sentence pretrial. Those who experience additional imprisonment have USD 17 less per month but this amount does not differ statistically from the USD –450 among those who serve the full sentence pretrial. Those who experience other sanctions have higher pre-detention earnings; almost USD 100 per month. The pretrial detainees who are not convicted have even higher average preadmission earnings than those who serve their full sentence pretrial, USD 160 more per month.

Following release, we observe an increase of USD 110 to average monthly earnings for the convicted only group. We also observe that this increase is not found for pretrial detainees: the additional change to their average labor earnings is around USD –120, effectively their earnings trajectories are flat across the case. The lack of neither statistically significant nor substantially important point estimates for any of the other pretrial detention groups indicate that pretrial detainees have uniformly lower average labor earnings – conviction and sentence status notwithstanding – at least in these uncontrolled models.

The differences between the comparison groups decrease, however, when we control for background characteristics and time fixed effects in model (2). Yet still, the overall conclusion remains the same, namely that pretrial detainment effectively cancels the earnings trajectory from before to after incarceration which the detainees would, as per the model and thus as expressed by the convicted only group, have experienced if they had not been detained pretrial.

We find this result no matter whether the pretrial detainees experience conviction or not, and no matter whether they experience additional sentencing, such as imprisonment or other sanctions.

When we exploit the full extent of the data panel structure to include individual level fixed effects in model (3), we again arrive at the same conclusion. In this model, which cannot include pre-admission average earnings of the comparison groups because they are differenced out by the fixed effects estimator (as mentioned and which is why several table entries are missing for this model), the intercept refers to the mean of the heterogeneity in labor earnings across all people in the data before admission. More importantly, the point estimates for post release labor earnings do not differ much from those reported in the first two columns of the table, suggesting that those results are not driven by individual heterogeneity.

[Insert Table 2 about here]

Turning to the share with labor earnings, our measure of labor market attachment, much the same conclusion arises (models (4) to (6) in Table 2). There are large differences in labor market attachment prior to admission, even though pretrial detainees who are not convicted or who are sentenced to other sanctions have better attachment than the ones who are sentenced to imprisonment. Following release, we observe a general decline in labor market attachment, and the share with labor earnings among the convicted only group drops by more than five percentage points. Pretrial detention doubles this loss, however, adding a six percentage points drop to labor market attachment. Again, it is pretrial detention per se and not conviction or sentence status that promotes this drop (the estimates related to the other pretrial detention groups are insignificant, both when we focus on raw correlations, when we control for background characteristics and time effects, and when we add individual fixed effects). Once again, adding individual fixed effects to the model does not alter the conclusions.

Family Outcomes. We find no effects of pretrial detention on family outcomes for the full sample

using the modeling strategy just presented (results presented in Table A.1).[7] When we observed

large post-release differences between the comparison groups on family outcomes (see Table 1;

A1), this finding was thus driven (almost) exclusively by preexisting differences between the

men in those groups. It may be that family members respond to detention of loved ones

differently than employers. The lack of an effect of pretrial detention (with or without

conviction) on family outcomes could also mask a great deal of instability in family relations. If,

for example, the men in our sample transition in and out of families, the steady average rates

could reflect that as some of the men in our sample move out of relationships, others move into

relationships, and so on.

Table 3 investigates this possibility and presents results from Cox proportional hazards

models. We estimate the risk of transitioning out of a household family attachment with a partner

or child, by comparison group. The advantage of using this model is that it allows us to control

for background characteristics and time fixed effects, thereby somewhat corresponding to our

main results. Results in Table 3 thus remove the differences between the groups that are driven

by the composition of the groups on observed characteristics and by general time trends. We

allow for the groups to differ in how their risk of transitioning out of families changes after

release, to observe any heterogenous effects. Results are presented as hazard ratios indicating

how much higher (hazard ratio larger than one) or lower (hazard ratio between zero and one) the

risk of losing family attachment is for each group relative to the convicted only group.

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[7] We also re-estimated these models with a truncated age distribution of men, ages 18-30. Results are
substantively similar, although the younger men have lower levels of family attachment following release
than was observed in the full sample (results available from the authors on request).

[Insert Table 3 about here]

Three years after release, around 70 percent of the convicted only group still live with the partner they did before conviction and 65 percent who lived with their children at conviction still did so three years afterward. Pretrial detainees have higher risks of losing a family attachment (with one exception: the risk of losing co-residence with a partner is higher, but not statistically significantly so, for men who serve their full sentence pretrial when compared to the convicted only group). The rank of the risks generally corresponds to what one would expect and our findings for labor market outcomes. Those who are detained pretrial but not convicted have higher risks of losing both types of family attachment (1.29 and 1.25 times higher risks). Their risks are, however, lower than the groups that in addition to pretrial detention experience conviction and an additional sentence. The risks are especially stark among those who are sentenced to additional prison terms, an almost doubling of the risks.

Recidivism. Table 4 presents results for recidivism. Except for a general decrease in conviction (2.5 percentage points) and arrest (almost 7 percentage points) from before to after incarceration – and uniformly lower rates for the imprisonment group following release (perhaps driven by age and deterrence; this group was incarcerated for longer than the other groups) – almost all parameter estimates following release are small and statistically insignificant. Importantly, these results suggest that the costs to labor market and family attachments for pretrial detainees are associated with no counter-balancing payoff in lower recidivism rates.

[Insert Table 4 about here]

Robustness Checks. We investigated whether our findings are driven by differences in other characteristics that affect criminal justice contact and/or social outcomes. In the online appendix A, we detail the robustness analyses and provide results using the same or similar modeling technique presented above. Specifically, we analyze whether our results are robust across length of detention and time period; whether effects are stronger or weaker for first-time detainees; whether results differ geographically; and whether the main results are driven by the longest detention spells. In all cases, although the magnitude of the estimated effects may change slightly, the pattern of effects remains unchanged. Taken together, our results are robust across a variety of specifications and sample restrictions and indicate a clear disadvantaging penalty of pretrial detention, no matter whether one is found guilty of the crime in question or not. In addition to these robustness checks we analyze whether our main results for labor market outcomes are sensitive to the choice of pre and post detention measurement points. The logic of this sensitivity analysis and the results concern the credibility of our identifying assumption of common trends and are described in section B of the online appendix. Results uniformly lend support to our main results which thus are unlikely to be caused by our choice of pre and post detention measurement points.

Discussion and Conclusion

Our paper makes three main contributions to the growing literature on the social consequences of criminal justice expansion. First, we show that pretrial detention, with or without conviction, imposes unique social costs for labor market and family attachments. This study suggests that pretrial detention should be added to the rapidly accumulating list of inefficiencies and hidden

harms associated with current criminal justice practice in many Western democracies (for reviews, see Comfort 2007, Pettit 2012, Turney and Connor 2019, Wildeman and Muller 2012; AUTHOR YEAR).

Second, prior work makes it difficult to differentiate harms that flow from separation (e.g., incarceration) and those that flow from the imposition of a stigmatizing credential (e.g., a felony conviction or criminal record). With regard to separation, incarceration terms need not be lengthy to cause significant instabilities in employment and family life (Apel 2016, Kling 2006, Ramakers et al. 2014). Similarly, while much research is focused on the imposition of a felony conviction (e.g., Manza and Uggen 2006, Pager 2003), stigmatizing labels conferred by the criminal justice system need not involve serious crimes (e.g., Kohler-Hausmann 2013, Lageson 2016). Given the nature of Danish register data, we are able to distinguish here between social costs that derive from detention and conviction, finding that pretrial detention imposes a unique cost for individuals.

Third, pretrial detention is at least partially motivated by an assessment that defendants present a danger to the public or a flight risk prior to adjudication. Our results show that detention results in consequential harms to labor market and family attachments that are not counterbalanced by reductions in recidivism. Such results, taken together, raise serious questions about the [over]-use of pretrial detention, where the pains of punishment are visited on those who may never be convicted of anything.

While research on mass incarceration in Western democracies has tended to focus on post-conviction imprisonment, this gap in research largely reflects the limited availability of data suited for analyzing pretrial and jail detention (Turney and Connor 2019; AUTHOR 2019). For this paper, we used administrative data from Denmark to overcome the data challenge; an

approach that offers both strengths and limitations. Strengths of registry data include full population information with precise and repeated measures of criminal justice contact and a wide array of background characteristics and outcomes; these attributes are difficult to find in other data sources. In US-based surveys, researchers must often infer pretrial detention when an incarcerated respondent does not report a conviction. In administrative datasets, pretrial detention is measured more cleanly but limited to a small set of jurisdictions or a limited set of outcomes.

There are, however, two main limitations of these data. The first concerns potential endogeneity among pretrial detainees who served their full sentence pretrial and those who were not convicted. Our statistical models assume that this decision is random, conditional on background characteristics and outcomes measured prior to detention. Although our results document common trends among these groups in the outcomes before detention, this assumption is fundamentally untestable. As such, whereas our results offer important knowledge on the consequences of system overreach in the form of pretrial detention, they are fundamentally descriptive.

The second limitation concerns generalizability: there are many differences between Denmark and other countries. We have noted elsewhere that the United States represents a criminal justice extreme and is thus the focus of much research attention; yet while average pretrial detention periods are similar between Denmark and the US, the comparatively short prison sentences in Denmark allowed an analysis distinguishing pretrial detention from conviction but prison terms in the US are much longer. It is difficult to imagine a similar analysis – across many jurisdictions and outcomes – being possible with US-based data. Conversely, pretrial conditions of confinement are arguably hasher in Danish pretrial detention centers. We

thus sacrifice generalizability in order to provide clean measures of incarceration and conviction status while offering results on a variety of outcomes not often available together in other data sources.

With these caveats in mind, our work underscores the importance of considering new forms of criminal justice contact. Pretrial detention constitutes a substantial portion of all incarcerations throughout the world and represents a unique criminal justice experience. Pretrial detention is especially important because it imposes the pains of incarceration and consequences for social life on those who may never receive a legally-imposed criminal label. From a systemic point of view, pretrial detention is thought to protect the public from potentially dangerous people during the adjudication process and ensures that defendants appear for trial. Yet whereas these aims may be noble, pretrial detention also represents a fundamental and – as results from Denmark in this paper have shown – consequential denial of liberty, even in cases with no corresponding finding of criminal responsibility to justify its use. In an era replete with examples of criminal justice system overreach and a web of entanglements between the criminal justice system and other institutions (e.g., Beckett 2018), it is nonetheless the individual and his family who must bear the consequences. As we have shown here, such consequences are clear well in to the future and across two domains – work and family – that are central to social life.

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Fig 1.
Labor market outcomes before and after incarceration or conviction. Men, Denmark, 1995-2010.
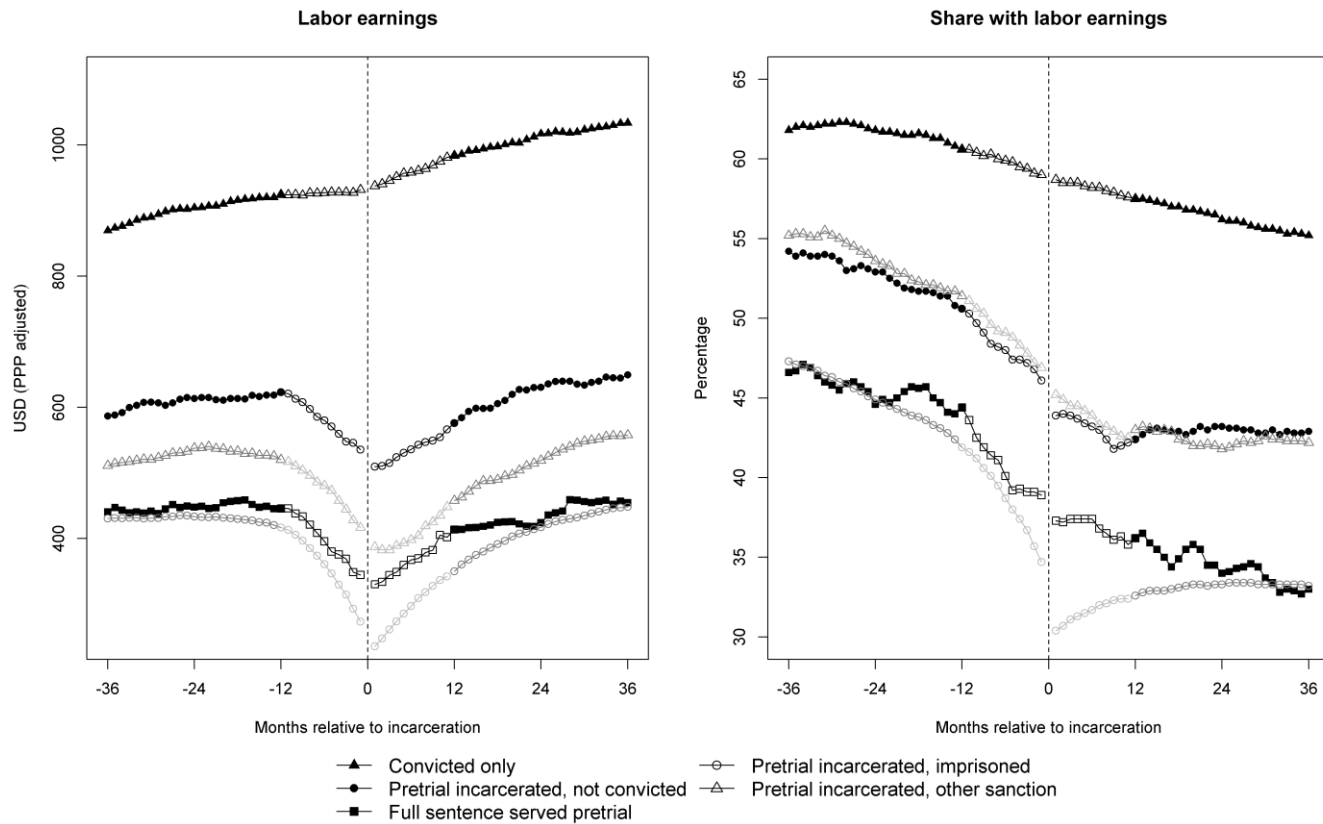
Note: *N* = 75,187. Labor earnings measured in 2010 OECD individual consumption Purchasing Power Parity (PPP) adjusted USD. Labor earnings are uncertain during the last 11 months before admission/conviction and during the first 11 months following release/conviction because they are derived from annual labor earnings. In our estimation results we exclude these uncertain data points.

Table 1. Descriptive statistics of analytic sample, by sanction status. Men, Denmark, 1995-2010.

Variable	Convicted only M/SD	Pretrial incarcerated, not convicted M/SD	Sentence served pretrial M/SD	Pretrial incarcerated then imprisoned M/SD	Pretrial incarcerated then other sanction M/SD
Age	34.500	29.626	30.335	29.606	29.330
	11.510	9.089	8.668	8.306	9.287
Parent	0.248	0.241	0.155	0.152	0.180
	0.432	0.428	0.362	0.359	0.384
Ethnic minority background	0.183	0.392	0.290	0.188	0.214
	0.387	0.488	0.454	0.391	0.410
9 years schooling	0.354	0.430	0.475	0.529	0.452
	0.478	0.495	0.500	0.499	0.498
10 years schooling	0.215	0.221	0.197	0.198	0.243
	0.411	0.415	0.398	0.399	0.429
12 years schooling	0.100	0.075	0.053	0.045	0.068
	0.300	0.264	0.224	0.207	0.251
13 years schooling	0.199	0.143	0.124	0.100	0.116
	0.399	0.350	0.330	0.301	0.320
14 years schooling	0.022	0.016	0.008	0.008	0.013
	0.146	0.124	0.088	0.090	0.113
15+ years schooling	0.045	0.027	0.021	0.009	0.019
	0.208	0.162	0.142	0.097	0.138
Missing education	0.065	0.087	0.120	0.109	0.087
	0.246	0.282	0.325	0.312	0.282
# previous convictions	0.219	0.154	0.088	0.058	0.155
	0.414	0.361	0.284	0.234	0.362

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Total number of incarcerations	1.358	2.398	3.831	4.157	2.353
	0.893	2.526	4.684	4.587	2.764
Current incarceration is first	0.877	0.645	0.554	0.520	0.763
	0.329	0.478	0.497	0.500	0.425
Current incarceration is second	0.090	0.206	0.178	0.186	0.119
	0.286	0.404	0.383	0.389	0.324
Current incarceration is third+	0.034	0.149	0.268	0.295	0.118
	0.180	0.356	0.443	0.456	0.322
Length of incarceration (months)	–	1.699	2.104	9.358	4.526
		2.949	2.526	10.522	11.932
# transfers during incarceration	–	0.464	0.349	1.260	0.486
		0.682	0.625	1.568	0.789
Share in local arrest	–	0.973	0.987	0.671	0.857
		0.134	0.101	0.369	0.284
Share in high security facility	–	0.012	0.005	0.066	0.008
		0.093	0.067	0.184	0.072
Share in low security facility	–	0.001	0.000	0.251	0.001
		0.021	0.000	0.358	0.036
Share in other facility	–	0.014	0.008	0.000	0.133
		0.096	0.076	0.000	0.275
Charged w/ violent crime	0.385	0.323	0.362	0.254	0.198
	0.487	0.468	0.481	0.435	0.398
Charged w/ property crime	0.604	0.387	0.459	0.550	0.631
	0.489	0.487	0.499	0.497	0.483
Charged w/ other crime	0.011	0.290	0.179	0.195	0.172
	0.106	0.454	0.384	0.396	0.377

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Av. earnings before	909.359	599.793	431.698	0.396	510.908
	1137.413	893.087	726.309	406.877	837.719
Av. earnings after	994.932	596.422	414.076	732.610	483.358
	1218.274	925.345	761.011	375.948	864.285
Share w. earnings before	0.611	0.513	0.440	742.183	0.522
	0.418	0.409	0.412	0.430	0.409
Share w. earnings after	0.569	0.430	0.352	0.402	0.428
	0.435	0.417	0.398	0.327	0.413
Lives w. partner before	0.176	0.125	0.088	0.385	0.080
	0.351	0.301	0.248	0.074	0.242
Lives w. partner after	0.176	0.128	0.087	0.230	0.075
	0.355	0.301	0.252	0.062	0.234
Lives w. children before	0.168	0.120	0.087	0.202	0.085
	0.344	0.292	0.251	0.075	0.251
Lives w. children after	0.163	0.126	0.083	0.231	0.075
	0.340	0.297	0.239	0.067	0.233
Conviction before	0.020	0.038	0.066	0.072	0.042
	0.038	0.049	0.078	0.070	0.056
Convictions after	0.017	0.034	0.054	0.056	0.034
	0.034	0.046	0.067	0.063	0.053
Arrest before	0.016	0.051	0.084	0.090	0.055
	0.035	0.069	0.101	0.092	0.079
Arrest after	0.012	0.039	0.060	0.061	0.040
	0.028	0.058	0.078	0.074	0.066
N	27,376	2,797	1,017	38,990	5,007

Note: *M* = Mean, *SD* = Standard Deviation.

Table 2. Results from estimations of the change in labor market outcomes by pretrial detention status.

Outcome	Labor earnings (2010 PPP USD)			Share with labor earnings		
Model	(1)	(2)	(3)	(4)	(5)	(6)
Before admission						
Convicted only (intercept)	901.739***	862.316***	736.921***	0.617***	0.573***	0.574***
	(7.619)	(131.607)	(68.360)	(0.003)	(0.062)	(0.039)
Pretrial incarcerated	–454.541***	–212.772***		–0.162***	–0.077***	
	(25.640)	(23.074)		(0.014)	(0.012)	
Pretrial inc. X Not convicted	161.928***	59.603*		0.072***	0.013	
	(29.993)	(26.991)		(0.016)	(0.014)	
Pretrial inc. X Imprisoned	–17.027	5.429		–0.006	–0.008	
	(24.558)	(21.964)		(0.014)	(0.012)	
Pretrial inc. X Other sanction	79.932**	–16.940		0.081***	0.008	
	(27.568)	(24.682)		(0.016)	(0.013)	
After release						
Convicted only (post dum.)	109.394***	109.394***	109.394***	–0.054***	–0.054***	–0.054***
	(5.986)	(5.990)	(5.986)	(0.003)	(0.003)	(0.003)
Pretrial incarcerated	–121.255***	–124.957***	–112.993***	–0.057***	–0.058***	–0.053***
	(25.381)	(25.637)	(25.369)	(0.014)	(0.014)	(0.014)
Pretrial inc. X Not convicted	26.130	30.014	23.193	0.013	0.015	0.013
	(29.958)	(30.189)	(29.931)	(0.016)	(0.016)	(0.016)
Pretrial inc. X Imprisoned	–6.362	–6.600	7.481	–0.007	–0.006	0.001
	(24.945)	(25.199)	(25.022)	(0.014)	(0.014)	(0.014)
Pretrial inc. X Other sanction	2.151	3.568	5.743	–0.002	–0.001	0.001
	(27.364)	(27.608)	(27.337)	(0.015)	(0.016)	(0.015)
Fixed effects						
Control variables		X	X		X	X
Month and year		X	X		X	X
Individual			X			X
R^2	0.062	0.207	0.008	0.057	0.249	0.028
N*T	150,374	150,374	3,759,350	150,374	150,374	3,759,350
N	75,187	75,187	75,187	75,187	75,187	75,187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table 3. Results from Cox proportional hazards models analyzing the risk of moving away from partner/children after release among those living with a partner/their children at admission. Men, Denmark, 1995-2010.

Variable	Moving away from partner Hazard ratio	Moving away from children Hazard ratio
Main effects		
Convicted only	(reference)	(reference)
Pretrial incarcerated, not convicted	1.292**	1.250*
	(0.120)	(0.122)
Sentence served pretrial	1.098	1.601*
	(0.211)	(0.303)
Pretrial incarcerated then imprisoned	2.146***	1.711***
	(0.101)	(0.085)
Pretrial incarcerated then other sanction	1.695***	1.473***
	(0.137)	(0.133)
Time-varying effects		
Convicted only	(reference)	(reference)
Pretrial incarcerated, not convicted	1.054	2.022***
	(0.278)	(0.388)
Sentence served pretrial	1.381	1.644
	(0.655)	(0.638)
Pretrial incarcerated then imprisoned	3.084***	4.692***
	(0.294)	(0.424)
Pretrial incarcerated then other sanction	1.140	2.615***
	(0.250)	(0.428)
Fixed effects		
Control variables	X	X
Month and year	X	X
Pseudo R^2	0.027	0.032
Total time-at-risk	212,499	196,208
N	8,631	8,280

Note: In addition to being standard control variables, age, we allow being charged of property crime, and total number of incarcerations a person has experienced to have time-varying effects. Standard errors in parentheses, clustered at the individual level. Data are right censored 36 months after release. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table 4. Results from estimations of the change in criminal justice outcomes by pretrial detention status.

Outcome	Conviction			Arrest		
Model	(1)	(2)	(3)	(4)	(5)	(6)
Before admission						
Convicted only (intercept)	0.352***	0.321***	0.037***	0.305***	0.357***	0.065***
	(0.003)	(0.050)	(0.007)	(0.003)	(0.058)	(0.009)
Pretrial incarcerated	0.330***	0.184***		0.393***	0.243***	
	(0.015)	(0.012)		(0.015)	(0.013)	
Pretrial inc. X Not convicted	−0.115***	−0.025		−0.097***	−0.020	
	(0.017)	(0.014)		(0.017)	(0.015)	
Pretrial inc. X Imprisoned	0.087***	0.048***		0.073***	0.042***	
	(0.015)	(0.012)		(0.015)	(0.012)	
Pretrial inc. X Other sanction	−0.100***	−0.041**		−0.109***	−0.053***	
	(0.016)	(0.013)		(0.016)	(0.014)	
After release						
Convicted only (post dum.)	−0.025***	−0.025***	−0.004***	−0.067***	−0.067***	−0.005***
	(0.003)	(0.003)	(0.000)	(0.003)	(0.003)	(0.000)
Pretrial incarcerated	−0.024	−0.022	−0.010***	−0.008	−0.006	−0.026***
	(0.016)	(0.016)	(0.003)	(0.016)	(0.016)	(0.003)
Pretrial inc. X Not convicted	0.018	0.017	0.008**	0.001	0.000	0.016***
	(0.019)	(0.019)	(0.003)	(0.019)	(0.019)	(0.003)
Pretrial inc. X Imprisoned	−0.051**	−0.047**	−0.006*	−0.041**	−0.039*	−0.002
	(0.016)	(0.016)	(0.003)	(0.016)	(0.016)	(0.003)
Pretrial inc. X Other sanction	−0.045*	−0.044*	0.007**	−0.032	−0.032	0.017***
	(0.017)	(0.017)	(0.003)	(0.018)	(0.018)	(0.003)
Fixed effects						
Control variables		X	X		X	X
Month and year		X	X		X	X
Individual			X			X
R^2	0.132	0.346	0.001	0.179	0.335	0.003
N*T	150,374	150,374	3,759,350	150,374	150,374	3,759,350
N	75,187	75,187	75,187	75,187	75,187	75,187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A.1. Results from estimations of the change in family outcomes by pretrial detention status.

Outcome	Live with partner			Live with children		
Model	(1)	(2)	(3)	(4)	(5)	(6)
Before admission						
Convicted only (intercept)	0.239***	0.091	0.253***	0.232***	0.119**	0.163***
	(0.003)	(0.051)	(0.023)	(0.003)	(0.042)	(0.025)
Pretrial incarcerated	–0.101***	–0.012		–0.098***	–0.013	
	(0.011)	(0.010)		(0.011)	(0.009)	
Pretrial inc. X Not convicted	0.043***	0.007		0.044***	–0.001	
	(0.013)	(0.011)		(0.013)	(0.011)	
Pretrial inc. X Imprisoned	–0.019	0.008		–0.007	0.009	
	(0.011)	(0.009)		(0.011)	(0.009)	
Pretrial inc. X Other sanction	–0.017	–0.016		–0.007	–0.011	
	(0.012)	(0.010)		(0.012)	(0.010)	
After release						
Convicted only (post dum.)	–0.003	–0.003	0.002	–0.002	–0.002	–0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Pretrial incarcerated	0.001	0.002	0.005	0.010	0.011	0.005
	(0.009)	(0.009)	(0.007)	(0.010)	(0.010)	(0.008)
Pretrial inc. X Not convicted	0.012	0.011	–0.001	0.011	0.010	0.012
	(0.010)	(0.010)	(0.009)	(0.012)	(0.012)	(0.010)
Pretrial inc. X Imprisoned	–0.002	0.002	–0.001	–0.011	–0.009	–0.002
	(0.008)	(0.009)	(0.007)	(0.010)	(0.010)	(0.008)
Pretrial inc. X Other sanction	0.000	0.001	–0.003	–0.009	–0.008	–0.007
	(0.009)	(0.009)	(0.008)	(0.010)	(0.010)	(0.009)
Fixed effects						
Control variables		X	X		X	X
Month and year		X	X		X	X
Individual			X			X
R^2	0.024	0.255	0.004	0.018	0.290	0.002
N*T	150,374	150,374	3,759,350	150,374	150,374	3,759,350
N	75,187	75,187	75,187	75,187	75,187	75,187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

APPENDIX A: ROBUSTNESS ANALYSES

To analyze whether our main results are driven by specific subgroups in our data, we run a number of robustness checks. All robustness checks are set up using the same logic of inquiry. The model we estimate in our robustness analyses expands our analytic model in the main text by a set of interaction terms, $R \times Pretrial \times$., where R is a dummy variable indicating that a person and case belongs to the specific subgroup which we are analyzing robustness for. Parameters marked by * in the model below thus measure whether results for the subgroup ($R = 1$) differ from results from those not in this subgroup ($R = 0$), effectively testing for subgroup differences in our results. Except from the R terms, the model we estimate here is similar to the one we estimate in the main analyses.

$$
\begin{aligned}
y_{ict} = \alpha_t \\
+ \beta_t Pretrial_{ict} \\
+ \gamma_t Pretrial \times Notconvicted_{ict} \\
+ \delta_t Pretrial \times Prison_{ict} \\
+ \theta_t Pretrial \times Probation_{ict} \\
+ \beta_t^* R \times Pretrial_{ict} \\
+ \gamma_t^* R \times Pretrial \times Notconvicted_{ict} \\
+ \delta_t^* R \times Pretiral \times Prison_{ict} \\
+ \theta_t^* R \times Pretrial \times Probation_{ict} \\
+ \pi X_{ic} \\
+ \rho Y M_{ic} \\
+ \varepsilon_{ict}
\end{aligned}
$$

Tables A.1–A.6 report estimates from our robustness analyses by outcome. In all tables, Model 1 reprints our main results. We check whether our main results are robust to length of incarceration (two checks, one for incarcerations periods exceeding three months and one for periods exceeding six months); to time period (whether case was in 2002 or later); whether effects are stronger or weaker for people who are experiencing their first incarceration; whether results differ between the Copenhagen detention center (which is the largest facility that houses pretrial detainees) and other facilities (local arrests); and whether the main results are driven by the longest detention spells (four checks: longest 5 percent of spells; longest 10 percent; longest 25 percent; and longest 50 percent).

The overarching conclusion that we draw from results presented in this appendix is that our main results are robust to differences across the subgroups, although we do acknowledge that the estimates for the subgroups (interaction terms) are imprecisely estimated due to low number of observations in some of those subgroups.

Table A.1. Summary table of robustness analyses for labor earnings.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Robustness description	Main results	> 3 months	> 6 months	After 2002	First spell	Copen-hagen	Longest 5 pct.	Longest 10 pct.	Longest 25 pct.	Longest 50 pct.
Before admission										
Convicted only (intercept)	862.316***	871.817***	861.469***	862.758***	869.699***	860.227***	859.762***	859.479***	863.711***	863.286***
	(83.919)	(84.349)	(84.676)	(83.960)	(83.146)	(83.822)	(83.812)	(83.780)	(84.676)	(83.481)
Pretrial incarcerated	-212.772***	-212.143***	-217.916***	-224.014***	-17.091	-156.129***	-214.934***	-214.654***	-216.008***	-228.241***
	(22.717)	(24.440)	(22.995)	(30.936)	(27.642)	(31.514)	(22.620)	(22.730)	(23.056)	(28.448)
Pretrial inc. X Not convicted	59.603*	68.470*	69.783*	48.211	20.447	41.647	63.728*	63.078*	68.450*	87.686**
	(26.913)	(29.089)	(27.423)	(38.191)	(32.222)	(37.148)	(26.852)	(26.979)	(27.425)	(33.416)
Pretrial inc. X Imprisoned	5.429	13.928	4.420	25.421	-46.906	-49.832	9.244	6.452	3.648	26.790
	(21.861)	(24.308)	(22.380)	(30.081)	(24.015)	(30.920)	(21.781)	(21.910)	(22.384)	(29.008)
Pretrial inc. X Other sanction	-16.940	57.267*	15.515	54.695	-61.102*	-101.602**	-7.803	-6.022	6.919	97.828**
	(24.434)	(27.510)	(25.254)	(33.353)	(27.525)	(33.244)	(24.465)	(24.610)	(25.171)	(32.151)
Robustness group effects										
R X Pretrial incarcerated		-6.113	112.573	19.035	-258.937***	-142.663***	572.676	197.303	78.885	31.102
		(59.810)	(121.632)	(44.311)	(43.781)	(41.067)	(664.398)	(290.138)	(120.002)	(43.371)
R X Pretrial inc. X Not convicted		-71.625	-207.349	15.532	70.938	39.966	-1188.463	-364.554	-208.344	-79.750
		(75.982)	(140.194)	(53.633)	(51.491)	(52.431)	(669.869)	(323.609)	(141.813)	(56.464)
R X Pretrial inc. X Imprisoned		-5.006	-100.457	-37.285	93.388*	107.368*	-586.827	-190.464	-67.930	-37.292
		(60.296)	(121.837)	(43.452)	(41.745)	(43.502)	(664.553)	(290.356)	(120.219)	(44.469)
R X Pretrial inc. X Other sanction		-231.180***	-305.529*	-151.937**	84.258	251.018***	-743.633	-380.142	-267.085*	-246.365***
		(64.029)	(124.936)	(48.645)	(45.802)	(49.677)	(666.676)	(293.971)	(124.065)	(48.881)

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After release

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Convicted only (post dummy)	109.394***	109.394***	109.394***	109.394***	109.394***	109.394***	109.394***	109.394***	109.394***	109.394***
	(5.797)	(5.797)	(5.797)	(5.797)	(5.797)	(5.797)	(5.797)	(5.797)	(5.797)	(5.797)
Pretrial incarcerated	-124.957***	-120.719***	-117.984***	-137.692***	-126.756***	-140.588***	-121.896***	-120.870***	-122.540***	-77.658*
	(25.482)	(26.966)	(25.539)	(32.192)	(28.450)	(35.941)	(25.399)	(25.523)	(25.539)	(31.615)
Pretrial inc. X Not convicted	30.014	28.524	20.284	67.816	9.462	9.814	26.395	23.770	23.689	-11.002
	(30.283)	(32.252)	(30.568)	(40.685)	(37.561)	(41.605)	(30.238)	(30.386)	(30.523)	(37.073)
Pretrial inc. X Imprisoned	-6.600	-12.147	-3.510	-7.010	25.888	8.780	-8.596	-5.066	-1.225	-55.621
	(25.157)	(27.286)	(25.409)	(32.107)	(28.203)	(35.717)	(25.084)	(25.222)	(25.351)	(32.642)
Pretrial inc. X Other sanction	3.568	54.464	20.717	41.546	-6.171	17.805	8.354	7.437	16.683	34.258
	(27.508)	(30.378)	(28.085)	(35.415)	(31.418)	(37.962)	(27.535)	(27.684)	(27.956)	(35.580)
Robustness group effects										
R X Pretrial incarcerated		-26.847	-165.202	23.723	3.363	39.410	-787.846	-421.664	-69.304	-99.954*
		(74.210)	(162.120)	(49.020)	(47.702)	(47.642)	(660.804)	(294.279)	(177.423)	(49.983)
R X Pretrial inc. X Not convicted		5.319	214.819	-65.575	31.193	54.893	945.716	623.196	155.062	76.423
		(90.290)	(181.407)	(60.018)	(58.655)	(59.656)	(695.679)	(325.683)	(198.592)	(63.670)
R X Pretrial inc. X Imprisoned		27.960	145.549	0.738	-62.040	-31.735	774.597	390.392	52.052	101.356*
		(74.695)	(162.311)	(49.710)	(48.401)	(50.402)	(661.043)	(294.493)	(177.609)	(51.164)
R X Pretrial inc. X Other sanction		-151.943	-9.898	-80.422	11.886	-33.771	585.235	252.330	-76.202	-71.982
		(77.821)	(164.948)	(54.533)	(52.059)	(55.302)	(663.495)	(298.986)	(180.578)	(55.207)
Fixed effects										
Control variables	X	X	X	X	X	X	X	X	X	X
Month and year	X	X	X	X	X	X	X	X	X	X
R2	0.207	0.209	0.208	0.208	0.209	0.208	0.208	0.208	0.208	0.209
N*T	150374	150374	150374	150374	150374	150374	150374	150374	150374	150374
N	75187	75187	75187	75187	75187	75187	75187	75187	75187	75187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A.2. Summary table of robustness analyses for share with labor earnings.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Robustness description	Main results	> 3 months	> 6 months	After 2002	First spell	Copen-hagen	Longest 5 pct.	Longest 10 pct.	Longest 25 pct.	Longest 50 pct.
Before admission										
Convicted only (intercept)	0.573***	0.574***	0.571***	0.582***	0.566***	0.578***	0.572***	0.571***	0.571***	0.573***
	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.044)
Pretrial incarcerated	-0.077***	-0.072***	-0.080***	-0.111***	-0.073***	-0.033*	-0.077***	-0.077***	-0.080***	-0.091***
	(0.012)	(0.013)	(0.012)	(0.017)	(0.017)	(0.015)	(0.012)	(0.012)	(0.012)	(0.016)
Pretrial inc. X Not convicted	0.013	0.015	0.021	0.022	-0.001	-0.009	0.014	0.014	0.020	0.035*
	(0.014)	(0.015)	(0.014)	(0.021)	(0.020)	(0.018)	(0.014)	(0.014)	(0.014)	(0.018)
Pretrial inc. X Imprisoned	-0.008	-0.013	-0.006	0.013	-0.021	-0.050**	-0.006	-0.005	-0.005	0.010
	(0.012)	(0.013)	(0.012)	(0.017)	(0.016)	(0.015)	(0.012)	(0.012)	(0.012)	(0.016)
Pretrial inc. X Other sanction	0.008	0.041**	0.026*	0.068***	0.004	-0.034*	0.011	0.013	0.022	0.077***
	(0.013)	(0.014)	(0.013)	(0.019)	(0.020)	(0.016)	(0.013)	(0.013)	(0.013)	(0.017)
Robustness group effects										
R X Pretrial incarcerated		-0.030	0.084	0.065**	-0.001	-0.109***	0.114	0.057	0.091	0.029
		(0.031)	(0.055)	(0.023)	(0.024)	(0.023)	(0.237)	(0.138)	(0.057)	(0.023)
R X Pretrial inc. X Not convicted		-0.026	-0.163*	-0.021	0.020	0.053	-0.328	-0.179	-0.186**	-0.058*
		(0.038)	(0.063)	(0.028)	(0.028)	(0.028)	(0.269)	(0.160)	(0.067)	(0.028)
R X Pretrial inc. X Imprisoned		0.031	-0.081	-0.039	0.026	0.070**	-0.138	-0.072	-0.090	-0.033
		(0.031)	(0.055)	(0.023)	(0.023)	(0.025)	(0.237)	(0.138)	(0.057)	(0.024)
R X Pretrial inc. X Other sanction		-0.088**	-0.184**	-0.120***	0.003	0.105***	-0.182	-0.140	-0.190**	-0.147***
		(0.033)	(0.057)	(0.026)	(0.026)	(0.026)	(0.239)	(0.141)	(0.060)	(0.026)

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After release

Convicted only (post dummy)	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***	-0.054***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Pretrial incarcerated	-0.058***	-0.057***	-0.056***	-0.035	-0.030	-0.083***	-0.058***	-0.056***	-0.056***	-0.049*
	(0.014)	(0.015)	(0.014)	(0.020)	(0.020)	(0.018)	(0.014)	(0.014)	(0.014)	(0.019)
Pretrial inc. X Not convicted	0.015	0.014	0.010	0.008	-0.009	0.017	0.014	0.012	0.011	0.005
	(0.016)	(0.018)	(0.016)	(0.024)	(0.024)	(0.021)	(0.016)	(0.016)	(0.016)	(0.021)
Pretrial inc. X Imprisoned	-0.006	0.010	0.007	-0.019	0.005	0.019	-0.004	-0.003	0.006	-0.002
	(0.014)	(0.016)	(0.014)	(0.020)	(0.020)	(0.018)	(0.014)	(0.014)	(0.014)	(0.020)
Pretrial inc. X Other sanction	-0.001	0.002	-0.004	-0.008	-0.003	0.022	-0.004	-0.006	-0.007	-0.003
	(0.015)	(0.017)	(0.016)	(0.022)	(0.023)	(0.019)	(0.015)	(0.015)	(0.015)	(0.021)
Robustness group effects										
R X Pretrial incarcerated		-0.005	-0.046	-0.043	-0.050	0.063*	-0.064	-0.155	-0.044	-0.019
		(0.036)	(0.063)	(0.027)	(0.027)	(0.028)	(0.170)	(0.124)	(0.068)	(0.028)
R X Pretrial inc. X Not convicted		0.006	0.093	0.017	0.043	-0.004	0.059	0.244	0.098	0.020
		(0.045)	(0.076)	(0.032)	(0.033)	(0.033)	(0.251)	(0.154)	(0.081)	(0.034)
R X Pretrial inc. X Imprisoned		-0.017	0.017	0.025	-0.024	-0.041	0.042	0.135	0.016	0.005
		(0.037)	(0.063)	(0.028)	(0.028)	(0.030)	(0.170)	(0.124)	(0.068)	(0.028)
R X Pretrial inc. X Other sanction		-0.006	0.055	0.008	0.016	-0.053	0.147	0.223	0.085	0.004
		(0.039)	(0.066)	(0.030)	(0.031)	(0.031)	(0.174)	(0.129)	(0.072)	(0.030)
Fixed effects										
Control variables	X	X	X	X	X	X	X	X	X	X
Month and year	X	X	X	X	X	X	X	X	X	X
R^2	0.249	0.250	0.249	0.250	0.250	0.249	0.249	0.249	0.249	0.250
$N*T$	150374	150374	150374	150374	150374	150374	150374	150374	150374	150374
N	75187	75187	75187	75187	75187	75187	75187	75187	75187	75187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A.3. Summary table of robustness analyses for share living with partner.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Robustness description	Main results	> 3 months	> 6 months	After 2002	First spell	Copen-hagen	Longest 5 pct.	Longest 10 pct.	Longest 25 pct.	Longest 50 pct.
Before admission										
Convicted only (intercept)	0.091	0.096	0.093	0.090	0.096	0.094	0.091	0.092	0.094	0.091
	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)
Pretrial incarcerated	-0.012	-0.015	-0.015	-0.007	0.017	-0.004	-0.014	-0.014	-0.013	-0.005
	(0.010)	(0.010)	(0.010)	(0.015)	(0.013)	(0.012)	(0.010)	(0.010)	(0.010)	(0.013)
Pretrial inc. X Not convicted	0.007	0.010	0.008	0.019	0.037*	-0.007	0.008	0.009	0.008	0.000
	(0.011)	(0.012)	(0.011)	(0.018)	(0.015)	(0.014)	(0.011)	(0.011)	(0.011)	(0.015)
Pretrial inc. X Imprisoned	0.008	0.000	0.002	0.004	0.030**	-0.000	0.005	0.004	-0.000	-0.002
	(0.009)	(0.010)	(0.010)	(0.015)	(0.012)	(0.012)	(0.009)	(0.009)	(0.010)	(0.014)
Pretrial inc. X Other sanction	-0.016	-0.003	-0.010	-0.020	0.014	-0.024	-0.013	-0.012	-0.011	-0.016
	(0.010)	(0.011)	(0.010)	(0.016)	(0.014)	(0.013)	(0.010)	(0.010)	(0.010)	(0.014)
Robustness group effects										
R X Pretrial incarcerated		0.017	0.042	-0.011	-0.021	-0.023	0.249	0.164	0.016	-0.015
		(0.024)	(0.047)	(0.019)	(0.019)	(0.019)	(0.182)	(0.114)	(0.049)	(0.018)
R X Pretrial inc. X Not convicted		-0.023	-0.033	-0.020	-0.053*	0.035	-0.318	-0.170	-0.016	0.012
		(0.031)	(0.055)	(0.023)	(0.022)	(0.023)	(0.199)	(0.127)	(0.059)	(0.023)
R X Pretrial inc. X Imprisoned		-0.003	-0.028	0.007	-0.042*	0.005	-0.212	-0.137	0.002	0.018
		(0.025)	(0.047)	(0.019)	(0.018)	(0.020)	(0.182)	(0.114)	(0.049)	(0.019)
R X Pretrial inc. X Other sanction		-0.048	-0.070	0.008	-0.048*	0.020	-0.278	-0.206	-0.052	0.000
		(0.026)	(0.048)	(0.020)	(0.020)	(0.021)	(0.183)	(0.115)	(0.051)	(0.020)

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After release

Convicted only (post dummy)	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Pretrial incarcerated	0.002	-0.004	0.003	-0.004	0.015	-0.004	0.003	0.003	0.002	-0.010
	(0.009)	(0.010)	(0.009)	(0.014)	(0.012)	(0.010)	(0.009)	(0.009)	(0.009)	(0.012)
Pretrial inc. X Not convicted	0.011	0.016	0.011	0.025	-0.012	0.018	0.010	0.010	0.011	0.024
	(0.010)	(0.011)	(0.010)	(0.016)	(0.015)	(0.012)	(0.010)	(0.010)	(0.010)	(0.014)
Pretrial inc. X Imprisoned	0.002	0.009	0.003	0.008	-0.008	0.008	0.005	0.005	0.005	0.019
	(0.009)	(0.010)	(0.009)	(0.014)	(0.012)	(0.010)	(0.008)	(0.009)	(0.009)	(0.013)
Pretrial inc. X Other sanction	0.001	0.015	0.004	0.010	-0.008	0.006	0.002	0.002	0.005	0.027*
	(0.009)	(0.010)	(0.009)	(0.014)	(0.014)	(0.011)	(0.009)	(0.009)	(0.009)	(0.013)
Robustness group effects										
R X Pretrial incarcerated		0.035	-0.019	0.011	-0.023	0.015	-0.238	-0.099	0.004	0.025
		(0.021)	(0.051)	(0.017)	(0.017)	(0.018)	(0.218)	(0.095)	(0.055)	(0.017)
R X Pretrial inc. X Not convicted		-0.029	0.017	-0.024	0.039	-0.016	0.250	0.100	-0.004	-0.029
		(0.026)	(0.057)	(0.020)	(0.020)	(0.021)	(0.218)	(0.117)	(0.062)	(0.021)
R X Pretrial inc. X Imprisoned		-0.038	0.014	-0.013	0.017	-0.017	0.185	0.074	-0.011	-0.032
		(0.021)	(0.051)	(0.017)	(0.017)	(0.019)	(0.218)	(0.095)	(0.055)	(0.017)
R X Pretrial inc. X Other sanction		-0.063**	-0.007	-0.018	0.018	-0.012	0.194	0.056	-0.034	-0.055**
		(0.022)	(0.052)	(0.018)	(0.019)	(0.020)	(0.218)	(0.096)	(0.056)	(0.018)
Fixed effects										
Control variables	X	X	X	X	X	X	X	X	X	X
Month and year	X	X	X	X	X	X	X	X	X	X
R^2	0.255	0.255	0.255	0.255	0.256	0.255	0.255	0.255	0.255	0.255
*N*T*	150374	150374	150374	150374	150374	150374	150374	150374	150374	150374
N	75187	75187	75187	75187	75187	75187	75187	75187	75187	75187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A.4. Summary table of robustness analyses for share living with their children.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Robustness description	Main results	> 3 months	> 6 months	After 2002	First spell	Copen-hagen	Longest 5 pct.	Longest 10 pct.	Longest 25 pct.	Longest 50 pct.
Before admission										
Convicted only (intercept)	0.119**	0.123**	0.121**	0.119**	0.123**	0.121**	0.120**	0.121**	0.122**	0.120**
	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)
Pretrial incarcerated	-0.013	-0.014	-0.017	-0.014	0.023	-0.008	-0.013	-0.013	-0.014	-0.011
	(0.009)	(0.010)	(0.009)	(0.013)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)	(0.012)
Pretrial inc. X Not convicted	-0.001	0.002	0.003	0.011	0.022	-0.003	-0.000	-0.001	-0.001	-0.001
	(0.011)	(0.011)	(0.011)	(0.016)	(0.015)	(0.014)	(0.011)	(0.011)	(0.011)	(0.014)
Pretrial inc. X Imprisoned	0.009	0.002	0.006	0.012	0.011	0.005	0.006	0.004	0.001	0.003
	(0.009)	(0.010)	(0.009)	(0.013)	(0.012)	(0.012)	(0.009)	(0.009)	(0.009)	(0.012)
Pretrial inc. X Other sanction	-0.011	-0.003	-0.006	-0.002	-0.012	-0.014	-0.012	-0.012	-0.010	-0.006
	(0.010)	(0.011)	(0.010)	(0.014)	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.013)
Robustness group effects										
R X Pretrial incarcerated		0.010	0.093	0.002	-0.040*	-0.012	-0.001	-0.024	0.018	-0.004
		(0.024)	(0.053)	(0.018)	(0.018)	(0.017)	(0.104)	(0.046)	(0.045)	(0.018)
R X Pretrial inc. X Not convicted		-0.023	-0.083	-0.021	-0.037	0.005	-0.120	0.011	0.006	-0.004
		(0.030)	(0.059)	(0.021)	(0.021)	(0.021)	(0.132)	(0.076)	(0.055)	(0.022)
R X Pretrial inc. X Imprisoned		0.002	-0.078	-0.005	-0.004	-0.005	0.039	0.050	-0.000	0.009
		(0.025)	(0.053)	(0.018)	(0.017)	(0.019)	(0.104)	(0.046)	(0.046)	(0.018)
R X Pretrial inc. X Other sanction		-0.029	-0.098	-0.020	0.001	0.003	0.017	0.043	-0.019	-0.012
		(0.026)	(0.054)	(0.019)	(0.019)	(0.020)	(0.106)	(0.049)	(0.047)	(0.019)

After release

Convicted only (post dummy)	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Pretrial incarcerated	0.011	0.010	0.014	0.008	0.010	0.018	0.011	0.011	0.012	0.000
	(0.010)	(0.010)	(0.010)	(0.014)	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.012)
Pretrial inc. X Not convicted	0.010	0.012	0.006	0.024	0.010	-0.001	0.010	0.010	0.008	0.020
	(0.012)	(0.012)	(0.012)	(0.018)	(0.017)	(0.015)	(0.012)	(0.012)	(0.012)	(0.014)
Pretrial inc. X Imprisoned	-0.009	-0.004	-0.012	-0.009	-0.005	-0.016	-0.009	-0.010	-0.010	0.009
	(0.010)	(0.011)	(0.010)	(0.014)	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.013)
Pretrial inc. X Other sanction	-0.008	0.009	-0.004	-0.004	-0.003	-0.020	-0.005	-0.005	-0.003	0.022
	(0.010)	(0.011)	(0.011)	(0.015)	(0.015)	(0.014)	(0.010)	(0.011)	(0.011)	(0.014)
Robustness group effects										
R X Pretrial incarcerated		0.006	-0.084	0.005	0.002	-0.018	-0.012	-0.022	-0.040	0.022
		(0.029)	(0.062)	(0.019)	(0.019)	(0.019)	(0.016)	(0.013)	(0.063)	(0.019)
R X Pretrial inc. X Not convicted		-0.008	0.098	-0.023	0.000	0.028	-0.004	0.072	0.062	-0.021
		(0.034)	(0.069)	(0.023)	(0.023)	(0.023)	(0.017)	(0.082)	(0.073)	(0.024)
R X Pretrial inc. X Imprisoned		-0.012	0.084	0.001	-0.006	0.017	0.013	0.028*	0.040	-0.030
		(0.029)	(0.062)	(0.019)	(0.019)	(0.020)	(0.018)	(0.014)	(0.063)	(0.020)
R X Pretrial inc. X Other sanction		-0.058	0.026	-0.009	-0.008	0.034	-0.064*	-0.062*	-0.026	-0.065**
		(0.030)	(0.063)	(0.021)	(0.021)	(0.021)	(0.028)	(0.025)	(0.064)	(0.021)
Fixed effects										
Control variables	X	X	X	X	X	X	X	X	X	X
Month and year	X	X	X	X	X	X	X	X	X	X
R2	0.290	0.290	0.290	0.290	0.290	0.290	0.290	0.290	0.290	0.290
*N*T*	150374	150374	150374	150374	150374	150374	150374	150374	150374	150374
N	75187	75187	75187	75187	75187	75187	75187	75187	75187	75187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A.5. Summary table of robustness analyses for share convicted.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Robustness description	Main results	> 3 months	> 6 months	After 2002	First spell	Copen-hagen	Longest 5 pct.	Longest 10 pct.	Longest 25 pct.	Longest 50 pct.
Before admission										
Convicted only (intercept)	0.321***	0.319***	0.317***	0.331***	0.313***	0.320***	0.318***	0.317***	0.316***	0.318***
	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)
Pretrial incarcerated	0.184***	0.182***	0.187***	0.170***	0.075***	0.161***	0.185***	0.186***	0.186***	0.195***
	(0.012)	(0.013)	(0.012)	(0.016)	(0.014)	(0.016)	(0.012)	(0.012)	(0.012)	(0.015)
Pretrial inc. X Not convicted	-0.025	-0.020	-0.027	-0.017	-0.003	-0.003	-0.028*	-0.028*	-0.026	-0.035*
	(0.014)	(0.015)	(0.014)	(0.020)	(0.017)	(0.018)	(0.014)	(0.014)	(0.014)	(0.017)
Pretrial inc. X Imprisoned	0.048***	0.039**	0.048***	0.042**	0.023	0.072***	0.051***	0.053***	0.052***	0.023
	(0.012)	(0.013)	(0.012)	(0.016)	(0.013)	(0.016)	(0.012)	(0.012)	(0.012)	(0.016)
Pretrial inc. X Other sanction	-0.041**	-0.044**	-0.045***	-0.055**	0.006	-0.024	-0.042**	-0.043**	-0.044***	-0.067***
	(0.013)	(0.014)	(0.014)	(0.018)	(0.016)	(0.017)	(0.013)	(0.013)	(0.013)	(0.017)
Robustness group effects										
R X Pretrial incarcerated		0.016	-0.054	0.028	0.113***	0.057*	-0.250	-0.086	-0.029	-0.023
		(0.033)	(0.058)	(0.024)	(0.023)	(0.023)	(0.130)	(0.097)	(0.060)	(0.024)
R X Pretrial inc. X Not convicted		-0.040	0.025	-0.014	-0.027	-0.056*	0.464*	0.139	0.005	0.018
		(0.041)	(0.068)	(0.028)	(0.027)	(0.028)	(0.184)	(0.127)	(0.073)	(0.029)
R X Pretrial inc. X Imprisoned		-0.000	0.049	0.011	0.045*	-0.096***	0.205	0.056	0.019	0.039
		(0.033)	(0.058)	(0.024)	(0.023)	(0.025)	(0.131)	(0.097)	(0.060)	(0.024)
R X Pretrial inc. X Other sanction		0.001	0.067	0.032	-0.052*	-0.036	0.250	0.096	0.046	0.056*
		(0.035)	(0.060)	(0.026)	(0.025)	(0.027)	(0.134)	(0.101)	(0.063)	(0.026)

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After release

Convicted only (post dummy)	-0.025***	-0.025***	-0.025***	-0.025***	-0.025***	-0.025***	-0.025***	-0.025***	-0.025***	-0.025***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Pretrial incarcerated	-0.022	-0.016	-0.024	-0.023	-0.069***	-0.008	-0.024	-0.023	-0.024	0.001
	(0.016)	(0.017)	(0.016)	(0.021)	(0.020)	(0.021)	(0.016)	(0.016)	(0.016)	(0.020)
Pretrial inc. X Not convicted	0.017	0.021	0.026	-0.003	-0.021	0.013	0.021	0.021	0.025	0.013
	(0.019)	(0.020)	(0.019)	(0.026)	(0.026)	(0.025)	(0.019)	(0.019)	(0.019)	(0.023)
Pretrial inc. X Imprisoned	-0.047**	-0.014	-0.025	-0.033	-0.023	-0.062**	-0.041**	-0.038*	-0.028	-0.020
	(0.016)	(0.017)	(0.016)	(0.021)	(0.020)	(0.021)	(0.016)	(0.016)	(0.016)	(0.021)
Pretrial inc. X Other sanction	-0.044*	-0.024	-0.031	-0.032	-0.104***	-0.052*	-0.036*	-0.036*	-0.032	-0.029
	(0.017)	(0.019)	(0.018)	(0.023)	(0.024)	(0.023)	(0.017)	(0.017)	(0.018)	(0.022)
Robustness group effects										
R X Pretrial incarcerated		-0.041	0.044	0.001	0.084**	-0.034	0.340	0.087	0.041	-0.049
		(0.046)	(0.093)	(0.031)	(0.030)	(0.031)	(0.214)	(0.200)	(0.104)	(0.031)
R X Pretrial inc. X Not convicted		-0.038	-0.171	0.034	0.047	0.010	-0.993***	-0.383	-0.171	-0.021
		(0.057)	(0.105)	(0.037)	(0.037)	(0.037)	(0.260)	(0.228)	(0.117)	(0.039)
R X Pretrial inc. X Imprisoned		-0.013	-0.083	-0.026	-0.042	0.061	-0.395	-0.137	-0.080	-0.009
		(0.046)	(0.094)	(0.031)	(0.031)	(0.032)	(0.215)	(0.200)	(0.104)	(0.032)
R X Pretrial inc. X Other sanction		-0.045	-0.128	-0.026	0.055	0.010	-0.509*	-0.243	-0.142	-0.036
		(0.049)	(0.096)	(0.035)	(0.035)	(0.035)	(0.219)	(0.204)	(0.107)	(0.035)
Fixed effects										
Control variables	X	X	X	X	X	X	X	X	X	X
Month and year	X	X	X	X	X	X	X	X	X	X
R2	0.346	0.347	0.347	0.346	0.350	0.346	0.347	0.347	0.347	0.347
*N*T*	150374	150374	150374	150374	150374	150374	150374	150374	150374	150374
N	75187	75187	75187	75187	75187	75187	75187	75187	75187	75187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

Table A.6. Summary table of robustness analyses for share arrested.

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Robustness description	Main results	> 3 months	> 6 months	After 2002	First spell	Copen-hagen	Longest 5 pct.	Longest 10 pct.	Longest 25 pct.	Longest 50 pct.
Before admission										
Convicted only (intercept)	0.357***	0.355***	0.352***	0.356***	0.361***	0.354***	0.354***	0.352***	0.351***	0.354***
	(0.058)	(0.057)	(0.058)	(0.058)	(0.058)	(0.058)	(0.057)	(0.057)	(0.057)	(0.058)
Pretrial incarcerated	0.243***	0.243***	0.244***	0.258***	0.247***	0.225***	0.243***	0.242***	0.243***	0.265***
	(0.013)	(0.014)	(0.013)	(0.017)	(0.016)	(0.017)	(0.013)	(0.013)	(0.013)	(0.017)
Pretrial inc. X Not convicted	-0.020	-0.016	-0.017	-0.021	0.016	-0.004	-0.021	-0.020	-0.020	-0.032
	(0.015)	(0.016)	(0.015)	(0.020)	(0.018)	(0.019)	(0.015)	(0.015)	(0.015)	(0.019)
Pretrial inc. X Imprisoned	0.042***	0.043**	0.048***	0.026	0.036*	0.061***	0.048***	0.051***	0.051***	0.024
	(0.012)	(0.014)	(0.013)	(0.017)	(0.014)	(0.017)	(0.013)	(0.013)	(0.013)	(0.017)
Pretrial inc. X Other sanction	-0.053***	-0.044**	-0.051***	-0.067***	0.020	-0.036*	-0.050***	-0.051***	-0.054***	-0.082***
	(0.014)	(0.015)	(0.014)	(0.018)	(0.017)	(0.018)	(0.014)	(0.014)	(0.014)	(0.018)
Robustness group effects										
R X Pretrial incarcerated		0.001	0.005	-0.029	-0.013	0.047	0.251	0.216*	0.014	-0.045
		(0.035)	(0.062)	(0.025)	(0.025)	(0.025)	(0.225)	(0.095)	(0.066)	(0.025)
R X Pretrial inc. X Not convicted		-0.035	-0.075	0.005	-0.052	-0.038	-0.055	-0.219	-0.038	0.010
		(0.043)	(0.072)	(0.029)	(0.028)	(0.029)	(0.253)	(0.131)	(0.078)	(0.031)
R X Pretrial inc. X Imprisoned		-0.002	-0.015	0.031	0.009	-0.062*	-0.303	-0.253**	-0.030	0.042
		(0.035)	(0.062)	(0.025)	(0.024)	(0.026)	(0.225)	(0.095)	(0.066)	(0.025)
R X Pretrial inc. X Other sanction		-0.026	-0.014	0.028	-0.089***	-0.039	-0.289	-0.226*	0.003	0.062*
		(0.037)	(0.064)	(0.027)	(0.027)	(0.028)	(0.228)	(0.100)	(0.069)	(0.028)

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After release

Convicted only (post dummy)	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Pretrial incarcerated	-0.006	-0.006	-0.006	-0.001	-0.041	0.002	-0.004	-0.004	-0.005	-0.012
	(0.016)	(0.017)	(0.016)	(0.021)	(0.022)	(0.021)	(0.016)	(0.016)	(0.016)	(0.021)
Pretrial inc. X Not convicted	0.000	0.006	0.005	-0.026	-0.027	0.003	0.000	-0.000	0.004	0.011
	(0.019)	(0.020)	(0.019)	(0.027)	(0.027)	(0.024)	(0.019)	(0.019)	(0.019)	(0.024)
Pretrial inc. X Imprisoned	-0.039*	-0.009	-0.016	-0.034	-0.025	-0.048*	-0.035*	-0.031	-0.019	-0.003
	(0.016)	(0.018)	(0.016)	(0.021)	(0.022)	(0.021)	(0.016)	(0.016)	(0.016)	(0.022)
Pretrial inc. X Other sanction	-0.032	0.003	-0.013	-0.030	-0.106***	-0.041	-0.028	-0.027	-0.015	0.028
	(0.018)	(0.019)	(0.018)	(0.023)	(0.026)	(0.022)	(0.018)	(0.018)	(0.018)	(0.023)
Robustness group effects										
R X Pretrial incarcerated		0.005	0.018	-0.009	0.065*	-0.018	-0.416	-0.209	-0.022	0.014
		(0.045)	(0.083)	(0.031)	(0.031)	(0.032)	(0.256)	(0.150)	(0.094)	(0.032)
R X Pretrial inc. X Not convicted		-0.042	-0.080	0.046	0.033	-0.008	-0.002	0.089	-0.062	-0.028
		(0.057)	(0.098)	(0.038)	(0.038)	(0.039)	(0.300)	(0.197)	(0.110)	(0.040)
R X Pretrial inc. X Imprisoned		-0.045	-0.060	-0.011	-0.023	0.052	0.351	0.153	-0.023	-0.047
		(0.046)	(0.083)	(0.032)	(0.031)	(0.034)	(0.256)	(0.150)	(0.094)	(0.032)
R X Pretrial inc. X Other sanction		-0.117*	-0.149	-0.007	0.079*	0.022	0.269	0.058	-0.149	-0.132***
		(0.048)	(0.086)	(0.035)	(0.036)	(0.036)	(0.259)	(0.155)	(0.097)	(0.035)
Fixed effects										
Control variables	X	X	X	X	X	X	X	X	X	X
Month and year	X	X	X	X	X	X	X	X	X	X
R2	0.335	0.336	0.336	0.335	0.336	0.335	0.337	0.337	0.337	0.336
*N*T*	150374	150374	150374	150374	150374	150374	150374	150374	150374	150374
N	75187	75187	75187	75187	75187	75187	75187	75187	75187	75187

Note: Standard errors in parentheses are clustered at the individual level. * $p < .05$; ** $p < .01$; *** $p < .001$.

APPENDIX B: CHOICE OF PRE AND POST DETENTION MEASUREMENT POINTS

In this appendix we show that our main results for labor market outcomes are insensitive to the choice of pre and post detention measurement points. Results, such as ours, that rely on a common trends assumption are always likely to be sensitive to such choice. Our approach to showing that the main results – which compare the average labor market outcomes from the entire pre to post detention periods – are insensitive to *when* we measure pre and post outcomes is strictly empirical and simply consists in showing that when we chose all possible combinations of pre and post detention measurement points, the overall conclusion remains unaltered. We only report results from this sensitivity analysis for labor market outcomes because we only found substantial effects of pretrial detention (with and without conviction) for these outcomes, and because the family outcomes offer too few pre and post detention measurement points to conduct the following sensitivity analysis.

We have monthly measurement points for months -36 to -12 prior to detention and, similarly, for months +12 to +36 following detention. Taking the measurement point -36, for example, we have +12 to +36 (including both) = 25 possible post detention measurement points. We run our empirical model for the -36 measurement point and each of the 24 post detention points, and we then save the corresponding empirical estimates and their standard errors. We then redo this exercise for each of the pre detention points, resulting in a total of 625 estimates derived from all possible combinations of pre and post measurement points. Having this large number of point estimates obtained from the same statistical model allows us to draw the empirical distribution of point estimates that comes from analyzing all potential choices of pre and post detention measurement points. If our statistically significant main results were sensitive to the choice of measurement points, the empirical distribution of point estimates would show that our main results were likely to be "extreme" observations, i.e. fall in one of the tails of the empirical distributions.

Figure B.1 shows the distribution of the empirical estimates and confirms that the main results. For all combinations of measurement points we, in the left subfigures of Figure 1.B, observe a statistically significant negative impact of experiencing pretrial detention on labor earnings and on employment (measured through the share with labor earnings). The right subfigures of Figure 1.B confirm that there are no statistically significant additional effects of being detained pretrial and then acquitted (we find both positive and negative estimates and all confidence intervals overlap with zero).

Figure B.1. Empirical distribution of effect estimates obtained from different choices of pre and post incarceration measurement points.