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THE HEALTH EFFECTS OF PRISON

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The Health Effects of Prison[*]

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Abstract

This paper studies the effect on mortality of two Swedish early release reforms in 1993 and 1999 that held prison sentences constant but increased the share of time inmates were required to serve from one-half to two-thirds. Contrary to previous correlational evidence, we find that reform exposure, and the corresponding increase in time served, did not harm post-release prisoner health. Rather, the overall risk of death decreases, with especially large and significant effects for those who are positively selected in terms of their criminal careers and connection to society. We also find (i) significant and persistent reductions in the chance of suicide, (ii) short-term reductions in violent death, and (iii) long-term improvement in general health (circulatory death). These cause-specific effects are driven by particular at-risk populations – individuals with pre-incarceration mental health problems, violent offenders, and older offenders, respectively. We argue that these findings are primarily driven by a direct in-prison health treatment and services mechanism: we demonstrate that health care utilization and program participation increases with time served. We also find that reform exposure decreases recidivism and has some very short-term beneficial labor market effects. Our main findings, however, do not appear to be driven by these life-style changes.

JEL Codes: K42, I14
Keywords: Prison, Health, Mortality, Crime, Recidivism

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1. Introduction

Former prisoners around the world have much higher death rates from natural and unnatural causes than the general population (e.g. Binswanger et al, 2007; Skardhamar et al, 2013; and Jones et al, 2017).[1] These mortality risks are even larger with more time in prison: a recent study of New York State parolees finds that each additional prison year translates into a 2-year decline in life expectancy (Patterson, 2013). This positive correlation between prison exposure and mortality risk is perhaps not surprising given the negative selection of prisoners on many dimensions related to mortality, including socioeconomic status and pre-incarceration health. For example, about 50% of U.S. prisoners have a known history of mental health problems, while 60% meet the criteria for drug dependence or abuse (Bronson and Berzofsky, 2017; Bronson et al., 2017).[2] Yet, the correlational nature of this literature leaves a number of key policy relevant questions unanswered: What share of the prison-health correlation (if any) is due to the *causal* impact of prison on health? Must prison be harmful to one's health?

We address these causal questions by taking advantage of Sweden's 1993 and 1999 early release reforms, which held sentences constant, but changed the share of time inmates were required to serve from 50% to 67%. Exposure to the two-thirds reform depended on the date of conviction and sentence length. Shorter sentences (4-12 months) were fully treated by the first reform and longer sentences ($\geq$ 24 months) by the latter; intermediate sentences were partially treated by both. We estimate the reduced form effect of exposure to the two-thirds reform on post-release mortality overall and by the main natural (circulatory, digestive, and cancer) and unnatural (suicide, violent death, and drugs and/or alcohol) causes of death. Thus, this paper presents the first causal estimates of the intensive margin impact of prison – i.e. more time in prison – on post-release health.

Identifying the causal impact of prison on offender health is important for many reasons. First, extensive evidence documents the relative disadvantage of the prison population: does prison exacerbate or mitigate this inequality? The World Health Organization argues that increasing healthcare for the disadvantaged prison population is both an effective way to reduce health inequality and improve public health overall, including non-inmates (WHO 2014). Second, a growing literature highlights the challenges faced by former prisoners reintegrating into society (DuRose et al., 2014; Doleac, 2019). Taken together with the theoretical role of

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[1] See the Fazel and Baillargeon (2011) review. Piquero et al. (2014) overviews the offender-mortality literature.
[2] Similarly, more than 60% of U.K. prisoners suffer from personality disorders and 50% from depression or anxiety (Burkhi, 2017); more than 50% of Swedish prisoners had been previously diagnosed with a psychiatric disorder, most commonly a substance abuse disorder (Haglund et al., 2014). Swedish prisoners also have worse self-reported health when compared with an appropriate group of non-prisoners (Nilsson, 2002).

human capital (in this case, health) in determining crime participation (Becker, 1968) and recent evidence that health care access can causally reduce crime, this suggests that the ability of ex-prisoners to desist from crime could depend, in part, on the health effects of prison.[3] Third, healthcare comprises a significant component of prison budgets — about 20% of 2015 U.S. prison expenditures (more than $8 billion) (Pew Charitable Trusts, 2017). Yet, we know little about the individual and social returns to these expenditures. Finally, as a substantial share of prisoners have minor children – more than 50% in the U.S. (Glaze and Maruschak, 2008) – parental incarceration can have second-generation impacts via parental health.[4]

More time in prison could affect an individual's health through multiple channels. Contemporaneous effects could be negative due to the spread of communicable diseases (a phenomenon highlighted with the current COVID-19 crisis), exposure to a violent and stressful environment, poor nutrition, and low (or no) access to proper healthcare. But, they could also be positive if inmates are kept sober and drug free or receive health care they either could not afford or did not seek when not in prison. Health screening upon intake can also identify previously unknown illnesses and lead to treatment. Moreover, treatment program participation (including type and intensity of treatment) can be contingent on expected prison stays of a long enough duration. In fact, Swedish inmates with stays that are too short are not assigned to treatment programs, implying that the potential for health improving effects of prison may be at the intensive margin studied here rather than the extensive margin of any prison.

Post-release health can be directly affected by the persistence of these contemporaneous effects or indirectly via the impact of prison on the former inmate's post-release environment and lifestyle, including criminal and labor market activities. Worse lifestyles for instance could directly increase the former inmates' exposure to violence while the associated financial and emotional strains can translate into poor health outcomes and behaviors, including high blood pressure, depression, anxiety, poor eating, smoking, or substance abuse.

Because individuals given harsher punishments tend to systematically differ on (un)observable dimensions related to post-release health, simply controlling for observables is not sufficient to disentangle correlation from causation. Yet, this is the state of the current literature: incarceration is associated with worse health outcomes and behaviors, including

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[3] See Doleac (2018) for a popular science review. Bondurant et al. (2018) find that expanding access to substance abuse treatment facilities reduce local violent and financially motivated crimes. Using various expansions to Medicaid coverage, Wen et al. (2017), Vogler (2017), and Aslim et al. (2019) all find evidence of decreases crime (or recidivism) behavior; some argue that the results are driven by increased access to substance abuse treatment.
[4] In the extreme, if prison affects mortality, then the economics literature on early parental death suggests potentially important second-generation consequences (e.g. Gertler et al., (2004) and Lang and Zagorski (2001)).

problems that prevent work (Schnittker and John, 2007), depression (Turney et al., 2012), fast food consumption and smoking (Porter, 2014), stress-related illnesses and infectious diseases (Massoglia, 2008), and higher mortality (Sailas et al., 2005; Haglund et al., 2014).

There is just one contemporaneous paper that attempts to get at the causal effect of prison on post-release health. Using administrative data from Ohio, Norris et al. (2020) compare the mortality outcomes of convicted felons who were and were not sent to prison. They find that prison has a large protective effect while incarcerated, i.e. prison "incapacitates" death. Convicted felons not sent to prison have higher mortality rates, mainly from homicides and overdoses but also other causes. Importantly, Norris et al. (2020) can rule out the existence of large, detrimental post-release health effects.

Numerous papers study the causal effect of prison on non-health outcomes, e.g. recidivism and employment, using exogenous variation from both random judge assignment and natural experiments based on sentencing guidelines or reforms.[5, 6] Even when using the same research design, the results are mixed. With random judges, longer prison sentences have been found to result in no effect on employment and earnings in Florida and California (Kling, 2006), less crime in Seattle (Roach and Schanzenbach, 2015), and worse recidivism and labor market outcomes in Texas (Mueller-Smith, 2015).[7] Three papers with alternative research designs find that longer prison sentences improve outcomes. Using discontinuities in Georgia's parole board guidelines, Kuziemko (2013) finds that one more month in prison reduces the 3-year chance of recidivism by 1.3 percentage points. Landerso (2015) finds that increasing violent offender incarceration lengths in Denmark by one month (about 50%) reduces unemployment and increases earnings. Using discontinuities in North Carolina's sentencing guidelines, Rose and Shem-Tov (2019) also find reduced recidivism, but that the effect diminishes as sentences get longer. The conflicting findings of these studies are likely driven, in part, by institutional differences in prison conditions and experiences across countries; one

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[5] Prison-health papers that do not study the effect of prison sentences on post release health include: (i) Johnson and Raphael (2009), who argue that higher black male incarceration rates explain much of the racial disparity in AIDS infection among men and women, (ii) Campaniello et al. (2017), who find that Italian collective pardons decreased prisoner suicide rates, (iii) Raphael and Stoll (2013), who find that the US de-institutionalization of the mentally ill accounts for 4-7% of incarceration growth from 1980 to 2000, and (iv) Boylan and Mocan (2014), who find lower inmate mortality rates after court orders condemning state prison overcrowding.

[6] Drago et al. (2009), Drago and Galbiati (2012), and Buonanno and Raphael (2013) use a 2006 Italian collective pardon to study deterrence, peer effects, and incapacitation respectively. Barbarino and Mastrobuoni (2014) and Maurin and Ouss (2009) also utilize collective pardons in Italy and France, respectively.

[7] Most judge random assignment papers study the extensive margin of incarceration. At this margin, Aizer and Doyle (2015) and Di Tella and Schargrodsky (2013) find harmful effects in Chicago and Argentina respectively, while Bhuller et al. (2020) find beneficial effects in Norway and Dobbie et al. (2018) find little effect of Swedish incarceration on recidivism (though the emphasis of this paper is on child outcomes).

such factor that may be relevant, but has not been discussed, is the role of prison healthcare and the post-release effect of prison on health.

We estimate the impact of exposure to Sweden's early release (two-thirds) reform using matched Swedish register data. Our analysis sample includes nearly 47,000 prison sentences of 4-48 months. These prisoners are as negatively selected in terms of their health as prisoners around the world: more than 20% were admitted to a psychiatric ward in the five years prior to prison. The baseline specification estimates the effect of being treated by the two-thirds reform, while conditioning on fixed effects for each prison sentence month bin (each 'bin' contains all sentences that can be rounded down to the same number of months) and trends in the date of conviction. This within bin design means that we compare individuals who received the same sentence but spent different amounts of time in prison due to the timing of treatment (which varies across bins). A causal interpretation relies on two assumptions. The first is relevance: reform exposure, on average, resulted in a 46-day increase in time served. In support of conditional independence, we demonstrate that (i) both the effect on day-served and the main results are independent of observables (including criminal justice, demographic, socioeconomic, and pre-incarceration health measures), and (ii) the observables themselves do not change discontinuously with reform exposure.

In stark contrast to previous correlational literature, we find that exposure to the two-thirds reform does not harm post-release health, and actually improves it. Though the reduction in mortality risk is not quite significant when looking at the entire sample, these aggregate results mask important heterogeneity in two dimensions. First, significant reductions in the overall chance of death (especially in the first two post-release years) are seen for sub-samples who are positively selected in terms of their criminal careers and connection to society, including those with no past prison exposure, property offenders, relatively young offenders, and those with some past employment. Second, significant effects are seen for the whole sample when zooming in on causes of death particularly relevant for this population. There is a large, significant and immediate reduction in the chance of suicide, which persists in the long-run: the chance of suicide is reduced by almost 80% and 40%, respectively, in the three and ten years after release. These suicide results are especially driven by individuals with previously identified mental health issues and violent offenders. Extended prison exposure does not just improve mental health. A significant reduction in violent death is seen immediately after release, which is stronger and lasts longer for the high-risk violent offender population. Finally, there are significant improvements in medium and long-run general health.

Driven by relatively older prisoners and those serving longer sentences, there is a significant decrease in the chance of circulatory death starting around year five and lasting until year ten.

Why did exposure to the Swedish two-thirds reform improve general and mental health? We first ask whether any other changes to the prison experience (e.g. overcrowding, facility assignment, peers), besides the increase in days served, could drive our reduced form effects. We provide qualitative and empirical evidence that the answer is no. Why then does more time in prison yield improved post-release health? We consider two channels: the direct effect of in-prison healthcare and treatment and an indirect post-release improved lifestyle effect.

With respect to the former, we conduct an out-of-sample analysis of the healthcare utilization of inmates sentenced from 2009 to 2013 (for whom we have detailed prison healthcare data). This yields two important conclusions. First, healthcare in Swedish prisons is of high quality. Second, healthcare and treatment increases with time served. More time in prison is positively related to: (i) visits with medical professionals (doctors, nurses, and psychologists) at both extensive and intensive margins, (ii) medication (at both margins), and (iii) both starting and completing treatment programs. High quality healthcare that increases with time served is consistent with our findings of the health improving effects of the reform.

With respect to the second mechanism, we assess whether extended prison exposure improved the post-release environment via reduced recidivism and improved labor market outcomes. An important take-away in and of itself is that increased exposure to Swedish prisons does generally not lead to worse outcomes in either dimension. In fact, recidivism is sometimes significantly lower and there is a very short-term positive labor market effect. Hetereogeneity analyses indicate that the recidivism results are primarily driven by subsamples that are negatively selected in terms of the stage of their criminal careers or societal connections. This contrasts sharply with parallel analyses for mortality, making it hard to accept a life-style improvement explanation as the only channel underlying the reform's beneficial health effects.

The remainder of the paper proceeds as follows. Sections 2 describes the reform and the relevant Swedish institutions. Section 3 presents the research design, while Section 4 describes the data. Section 5 studies the implementation of the reform and identifying assumptions. Section 6 presents the results. Mechanisms are discussed in Section 7 and Section 8 concludes.

2. Institutional Background
2.1. Sweden's Early Release Policy Reforms
The Swedish Prison and Probation Service has had an early release and probation system in place since 1906 (proposition 1906:49). It aims to help inmates reintegrate into life outside of

prison and prevent recidivism by giving prison authorities a period of time after release during which they can make explicit demands on the inmate (e.g. probation officer contacts, substance abuse programs, or active job search). This paper studies the effect of changes to these early release laws in the 1990s.

In 1990, inmates serving sentences of 2-months or less were not eligible for early release. Inmates serving 3-months were released after two-thirds of their sentence, while those serving 4-months or more were released after one-half of their sentence. Only particularly dangerous criminals could be held longer – up to two-thirds – though this was quite rare.

The rules for early release changed on July 1, 1993 (proposition 1992/93:4).[8] The new rules stated that all prisoners sentenced to 4-24 months would be required to serve two-thirds of their sentence, but that those with more than 24-months should still be released after serving half of their sentences. To avoid threshold effects, a graduated scale was applied in practice for those serving 13 – 24 months (SOU 2005:54).[9]

On January 1, 1999 the early release and probation law changed again, such that all prisoners sentenced to more than 1-month were required to serve two-thirds of their sentence (proposition 1997/98:96).[10] Although the law still stated that early release was at the discretion of the parole board, in practice the Swedish Prison and Probation Service applied the two-thirds rule quite strictly with few and only minor deviations, regardless of inmate behavior and/or characteristics. At this time, a serious infraction of prison rules could lead to a delay in early release of at most 15 days (per infraction) and these delays were used quite sparingly.

The post-release probation rules did not change. Regardless of sentence length, probation lasts for at least 12 months, and at most the amount of time remaining on the original prison sentence. However, only the first 12 months of probation are "active". Any remaining months are "passive", with few or no demands placed on former inmates. Increasing the share of time served to two-thirds did not change the number of active probation months; but, for sentences longer than 24 months, time on passive probation decreased after the 1999 reform.

We use the 1993 and 1999 early release reforms as a source of exogenous variation in the number of days an individual actually spends in prison. Figure 1 visualizes the changes made to Sweden's early release policies between 1990 and 2002. For all possible prison sentence lengths, it shows the share of a prison sentence an inmate is required to serve before

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[8] The first formal motion concerning the new law was filed in January 1992. The new law was voted on and passed by the Swedish Parliament on December 10, 1992.
[9] The graduated scale is stated in proposition 1992/93:4. Those with 13-24 month sentences should serve 8 months plus one-third of the time exceeding one year. An 18 month sentence results in 8+(6/3) = 10 months served (56%).
[10] The first formal motion was filed in March 1998 and was passed by the Swedish Parliament on June 3, 1998.

being released. 'Year' refers to the year of conviction. The exact shares for each sentence length used to generate Figure 1 can be seen in Appendix Table 1.

Given that there were other criminal justice reforms that affected those sentenced to less than 4-months (described in more detail in Section 5.4), our analysis focuses on sentences of 4-48 months in prison. Before the 1993 reform, individuals were required to serve 50% of such sentences and after the 1999 reform, the requirement was now 67% (two-thirds). But, as illustrated in Figure 1, the timing of being fully exposed to the reform (i.e. *treated*) depended on the conviction date and sentence length, with shorter sentences being treated in 1993, longer sentences in 1999, and the remaining sentences partially treated by both reforms.[11] This variation in reform exposure lies at the heart of our identification strategy.

2.2. Prisons in Sweden

Over the last 30 years, Sweden's incarceration rate has fluctuated between a high of 79 (in 2006) and low of 53 (in 1985) inmates per 100,000 persons, which is roughly 25% lower than the Western European average and 10 times lower than the US. (See Panel A of Appendix Figure 1). Sentences are also shorter in Sweden; 84% of inmates convicted between 1991 and 2001 had sentences shorter than one year, with an average time spent in prison of 4.7 months. The average time served was about 8 months in Western Europe in 2001 (European Council 2002) and more than 30 months in U.S. state and federal prisons (BJS 2001a, 2001b).[12]

In 1991, there were 82 Swedish prisons. Many were relatively small. The largest was (and still is) Kumla – a high security prison with space for 420 inmates. The number of prisons began to decline in the mid-1990s with the closure of the smallest and oldest facilities. Capacity was maintained by building six new prisons and through the expansion of several existing prisons.[13] Average prison size across the entire sample period studied was 85 inmates.

To understand the potential effects of more prison exposure in Sweden on post-release health, a first-order question clearly relates to prison conditions and healthcare provision. Sweden and the other Nordic countries are well known for their relatively good prison conditions (Pratt 2008; Ugelvik and Dullum 2012). In fact, Sweden spends more money per

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[11] Since the Swedish constitution prohibits the application of new sentencing laws retroactively unless they benefit the offender, the new early release policies, which increased the share of time served, were to be applied to people convicted after the implementation date of each reform. The only exception is the reduction in share of time served from 100% to 67% for those sentenced to 2-months around the 1999 reform, whom we do not study here.

[12] Since the U.S. estimates from the National Corrections Reporting Program Series and the Federal Criminal Case Processing Statistics exclude short jail sentences, the U.S. vs. Europe difference may be exaggerated.

[13] Today, there are 45 prisons; 12 open facilities, 4 mixed (open and medium security), 22 medium security facilities, and 7 high security facilities. Security class and prison assignment are based on (i) crime severity, (ii) escape risk, (iii) gender, (iv) age 18 to 21, (v) rehabilitative needs, and (vi) family ties.

inmate than any other country and has one of the world's lowest staff-to-inmate ratios (1.15 in 2015). See Panels B and C of Appendix Figure 1. One notable feature of Swedish prisons is that each inmate has his or her own private cell. While incarcerated, an inmate's time is governed by a treatment and activity plan designed during their first week. This plan includes: (i) details about working, education, and substance abuse or psychological treatment, (ii) visitation rights, and (iii) a clear end date for the inmate's sentence.

The treatment plan can also include health information and routines, based on the results of a health exam given to all new prisoners. All prisons have their own health clinics with nurses on call every day and doctors available one or two days a week. Larger prisons often have their own full-time psychologist. An acutely ill inmate will be transported to a local hospital, while specially trained custodial staff members are responsible for providing daily medication to inmates who need it. In most respects, the prison health care system is quite similar in quality and quantity to the health care system outside of prison.[14] Using detailed data about healthcare utilization for prisoners from more modern cohorts (2009-2013) than our reform sample, we will demonstrate in Section 7.2 that healthcare utilization in prison is high and that both utilization and treatment intensity increase with time served.

3. Empirical Strategy

Obtaining unbiased estimates of the causal effect of days served in prison on post-release outcomes is challenging given the many observable and unobservable factors that determine both time in prison and these outcomes, including mortality, recidivism, and labor market participation. Moreover, even if one can control for actual sentence length, there is still variation in actual days served due to endogenous factors, like behavior in prison or time in pre-trial detention. Unfortunately, such variables, which potentially proxy for criminality and recidivism risk, are both unobservable to us and likely related to post-release outcomes.

Given this potential for omitted variable bias, we utilize an identification strategy that relies on exogenous variation in days served driven by the two Swedish early release reforms. We thus estimate the reduced form effect of reform exposure, which for our analysis sample of 4-48 month sentences, increased the share of time served for a given sentence from one-half to two-thirds. The underlying intuition is that we compare individuals with the same sentence (and offense characteristics), but who serve different amounts of time in prison because they

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[14] This description of conditions and healthcare access in Swedish prisons contrasts those of the U.S. prisons. Katz et al. (2003) analysis of prison conditions in the U.S. uses death rates in custody as a proxy, and highlights this measure as being an indication of inadequate health care, which was the subject of many lawsuits.

are convicted before or after one of the reforms. Thus, our strategy takes advantage of one of the distinguishing features of the early release reforms: they affect the share of time spent in prison without impacting the actual sentence length.

Equation (1) presents the baseline reduced form specification, where *Treated* equals one for sentences that are fully exposed to the reform (i.e. the law prescribes two-thirds for that conviction date *t* and sentence month bin *s*) and zero for non-exposed sentences (i.e. the law prescribes one-half time served for that conviction date and sentence). As sentences of 13-23 months are partially treated by both reforms, *Treated* for these sentences equals 0 prior to the 1993 reform, 1 after the 1999 reform, and the fraction of treatment for the in-between period.[15]

(1) $\quad Y_{its}^m = \delta Treated_{ts} + \alpha_s + ConvMonth_{trend} + ConvYear_{fe} + X_i\theta + \varepsilon_{its}$

The baseline specification includes sentence month bin fixed effects, α_s, to compare individuals with the same sentence but exposed to different early release laws. X_i includes a full set of criminal justice controls (crime type dummies, age at prison, and number of contemporaneous crimes, past crimes and prison sentences) that should affect sentence length (e.g. within bin variation). To increase precision, we also include court and calendar month of conviction dummies, demographic controls, and pre-incarceration measures of socioeconomic status and hospitalization history. Our results are robust to excluding these controls.

To the extent that there are trends over time in criminal justice or prison policies, the baseline includes conviction year fixed effects, *ConvYear_{fe}*, and a linear time trend in the month of conviction, *ConvMonth_{trend}*. The former makes intuitive sense if one thinks of the reduced form as a differences-in-differences specification, where we want to control for any other shocks common to sentence month bins that are both treated and untreated by the reforms. The latter makes intuitive sense if one had decided to model the reduced form as a regression discontinuity design, with date of conviction as the running variable. To some extent, one can imagine our strategy as taking advantage of a series of discontinuities of varying magnitudes, which occur at two dates and across sentencing bins. We demonstrate that our results are robust to a number of alternative specifications that are chosen in light of these two frameworks: differences-in-differences (e.g. sentence bin by year fixed effects and bin specific trends) and regression discontinuity (e.g. a non-linear trends and dropping donuts around the reforms).

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[15] For instance, a 16 month sentence in this period would be assigned (0.58-0.5)/(0.67-0.5) = 0.47 between the two reforms. Appendix Table 1 displays the values prescribed by the law across all sentence bins and periods.

Interpreting the coefficient on *treated* causally relies on two assumptions. The first is that the reform was actually implemented as it should be, and impacted the share of time served but not the sentence. In other words, the reform is 'relevant' in that it effects prison days served. The second is conditional independence: exposure to the reform should be unrelated to individual characteristics. We provide evidence in support of these assumptions in Section 5. However, these assumptions are not enough to conclude that it is more time in prison (though this is the first-order impact of the reform) that underlies the causal effect. Section 7 considers whether these reduced form findings can be driven by the effect of reform on other aspects of the prison experience, including overcrowding, peers, and facility assignment.

Finally, we note a number of features of the empirical implementation. First, in accordance with how the treatment is defined, we cluster standard errors at the sentence month bin level.[16] Second, we allow individuals to be *at risk* as of the release date; i.e., we measure all outcomes in terms of months since release. We also demonstrate that our results are robust to allowing individuals to be at risk as of the start date, thereby including deaths in prison in the analysis.[17] Finally, we trace out the post-release dynamic effects of longer time in prison, measured at various points (e.g. 12, 24, 36, ….120 months) since release. We condition the analysis appropriately on those for whom such an outcome can be observed (e.g. for recidivism, on being alive and never emigrating at month *m,* and for mortality, on never emigrating).

4. Data
4.1. Data Description

We begin constructing our sample using data from the Swedish Prison and Probation Service covering all individuals who entered prison since 1992. We use the dates for when each person enters and exits prison to calculate the exact number of days spent in prison. Combining this

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[16] Since we have variation in treatment within clusters, our standard errors will be somewhat conservative (see the discussion at the end of Section 3.4 in Abadie et al., 2017). Standard errors are generally smaller when we, instead, cluster on sentence month by reform time period bins. But this alternative clustering strategy generates only a marginal improvement in efficiency and does not lead to any changes in the interpretation of the results. These alternative standard errors also come with the unappealing assumption that the error terms within the same sentence month bin are uncorrelated across these time periods.

[17] The appropriate date at which to measure 'at risk' is often debated in the literature, especially when studying recidivism: should the at-risk date be conviction (in which case one has to disentangle incapacitation from deterrence) or release (which leads to concerns about biases arising from the age-crime profile)? This issue is discussed extensively, for instance, in Rose and Shem-Tov (2019). Given our interest in post-release health, we use date of release as our baseline but demonstrate robustness to using the (less endogenous) prison start date. Given (i) the substantial variation in the amount of time between conviction and prison start dates, which is a function of many of observable and unobservable factors, and (ii) our goal to study the effect of treatment *in prison*, we do not use conviction dates.

with information on the sentence length handed down by the courts, we can calculate the share of any prison sentence that is actually served.[18]

Using the personal identification number assigned to each Swedish resident (including foreign inmates), we match the prison data to the convictions register maintained by the Swedish National Council for Crime Prevention. The conviction data span 1973 to 2016 and include information on offense and conviction dates, crime types, and sanctions. We demonstrate below that knowing both the conviction date and start date of a prison sentence is crucial to correctly assign treatment status. We also use the conviction data to measure past offenses and prison spells, current offense characteristics, and post-release recidivism.

Mortality data (our main outcome of interest) come from the Swedish National Board of Health and Welfare's cause of death register. We study both all-cause and cause-specific mortality. The main cause of death is classified using mutually exclusive ICD10 codes for: (i) suicide, (ii) intentional violence, (iii) cancer, (iv) circulatory disease, and (v) digestive disease. In each case, the coroner also notes whether the death was alcohol and/or narcotics related. We therefore consider as an additional outcome an alcohol or narcotics flagged death that is not otherwise classified as one of our primary ICD10 codes. As the date of death variable is incomplete in this register, we use the death date provided by Statistics Sweden.

Finally, we create measures of pre-incarceration health/healthcare utilization using data from the National Board of Health and Welfare's hospital inpatient registers, which are available from 1987 onwards. These data include the dates of admission and release as well as the admitting ward, which we categorize as: (i) psychiatric, (ii) alcohol, (iii) narcotics, and (iv) general (excluding maternity wards).

The remaining variables used in this paper – including birth dates, immigration and emigration dates, gender, income, employment status in November, marital status, number of children, and education – are sourced from various Statistics Sweden registers. We have a long panel, which allows for variables both contemporaneous and prior to a person's conviction.

4.2. The Analysis Sample

To treat both reforms in a reasonably symmetric manner, we focus on sentences that start between 1992 and 2001. Appendix Table 2 shows the number of observations dropped due to each sample restriction for both the whole sample (N = 108,439) and the analysis sub-sample

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[18] As data on days in pre-trial detention do not exist, and since this time is subtracted from the days an individual must serve, our measure of the share of time served will almost always lie slightly below what the law prescribes.

of sentences greater than 3 months (N = 57,310). We are left with 86,109 sentences of 0-48 months, and 46,815 in the main analysis sample of 4-48 months.

We first match prison sentences to conviction dates by searching for an individual's last conviction before the prison start date that included a prison sentence. For about 98% of the sample, we can identify such a date. About half of the sample starts their sentence within 3 months of conviction, 90% within 400 days, and 97% within two years. While large differences between conviction and start dates could theoretically exist (e.g. due to an extended appeal), such cases can also represent measurement error in our matching process. We therefore drop those with a more than two-year lag until starting prison (yielding N = 102,762). We also drop about 2,000 sentences longer than 48 months, which are too scarce to analyze. We also omit 467 juveniles (start, conviction, or offense occurred before age 18), mostly from 0-3 month sentence bins, who face different sentencing laws and/or facilities. Individuals who both start and end their sentence in post-trial detention (i.e. a temporary placement) are also excluded, as share time served laws would not apply (60% have sentences of 3 months or less).

We drop individuals for whom 'treatment' is uncertain (N=8,691). Though time-served should be determined by the conviction date, Section 5.2 demonstrates that at least a sub-set of individuals who were convicted before but started their sentence after each reform were treated using the start date. Including individuals whose conviction and start dates 'straddle' the reforms would lead to measurement error in assigning treatment. Nevertheless, we find that the results are robust to including these individuals, but slightly attenuated as expected.

Finally, we drop individuals for whom days served would be unaffected by the reform because they: (i) had a life sentence (N=6), (ii) died in prison (N =71), or (iii) were sent to a foreign prison (N =149). We also exclude those who serve more than 110% of their sentence (N =982) or less than 10% (N =246). The former could occur, for instance, due to misbehavior related sentence extensions and to how strictly probation revocations were enforced. The latter are primarily due to time served in pre-trial detention (for which we have no data).

4.3. Summary Statistics

Table 1 provides summary statistics for the main analysis sample of 46,815 4-48 month sentences, for which the average number of months (days) sentenced is 11.7 (354), and for comparison purposes, sample means for 0-3 month sentences (outside our sample). Different types of offenses lead to longer sentences; 37% of the 0-3 month sample is driving under the influence (dui) offenders (primarily from the early 1990s) and 24% and 20% are charged with violent and property offenses, respectively. In contrast, just 3% of the analysis sample are

charged with dui, while 33%, 43%, and 15% are convicted of violent, property, and drug and alcohol offenses, respectively. The 4-48 month sample is negatively selected in terms of criminal history and observables, such as pre-incarceration employment, average income and health. Given our emphasis on post-release health, pre-incarceration health is clearly of interest. The only variable available during this period is hospitalization, which likely captures a combination of both health and health care utilization. The analysis sample has a similar average number of hospitalization days in alcohol (about 0.4) and general (about 2.5) wards over the last three years compared to the 0-3 month sample but significantly greater days in narcotics (0.84 vs. 0.51 days) and especially psychiatric (5.38 vs. 2.79 days) wards.

Of course, this negative 'selection' for those with 4-48 months versus 0-3 months is not the margin we are studying. But, it highlights that those treated by the two-thirds reform (our analysis sample) are a particularly disadvantaged subset of the criminal population. Table 1 also presents statistics separately for sentences of 4-12 (N=33,799), 13-24 (N=8,968), and 25-48 months (N=4,048). These highlight again differences in observables, but also that observables are not monotonically 'worse' as sentences get longer. Rather, they are likely in part driven by the fact that inmates with longer sentences have relatively more violent and drug and alcohol offenses while those with shorter sentences have more property crimes. The distribution of drug/alcohol, property, and violent offenses in each group is: 13%, 49%, and 28% (4-12 months), 17%, 35%, and 43% (13-24 months), and 28%, 18%, and 52% (25-48 months). In terms of hospitalization, those with the longest sentences have on average fewer hospitalization days in the last three years compared to the other groups; the most days is seen for 4-12 month sentences. Psychiatric days are the most prominent for all sub-samples.

Figure 2 presents information on the post-release health of our analysis sample. Specifically, it traces out the dynamic path of mortality (solid line) over time. More than 10% of the analysis sample has died by 8-years post-release and 12% die by age 50; this compares to just 3% in the full Swedish population (with similar demographic characteristics). Hospital utilization rates are also quite high: By 8-years post release, 60% have been hospitalized at least once (dashed line). This figure also shows that recidivism rates (dash-dot line) are steep: more than 50% are re-incarcerated within 12 months and 60% by 48 months.[19]

5. Sentencing Reform Implementation

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[19] Hospitalization and recidivism statistics are based on the sample alive and never emigrated from Sweden t months since release. Mortality statistics condition on the sample still in Sweden.

This section (i) provides a visualization of the exogenous variation used in our identification strategy, (ii) empirically assesses the implementation of the reforms – a necessary step to correctly code reform exposure, and (iii) discusses the identifying assumptions.

5.1. No Impact on Sentence Length or Earlier Judicial Decisions

Our identification strategy relies on the fact that the reforms should only affect the share of time served for a given sentence, but not the actual sentence: the former is determined by the prison authorities, while the latter is decided by the judge and lay judges. Similarly, earlier stage judicial decisions, e.g. arrest, charge, or pre-trial detention, should not in theory be affected by the reforms. However, as previous research finds that criminal justice agents, including prosecutors and juries, may try to offset sanction increases by charging or convicting defendants of lesser crimes, we assess whether this is a concern in the Swedish context.[20]

We begin with sentencing, which is perhaps the most salient dimension on which such manipulation could occur. Panels A and B of Figure 3 show the sentencing distributions (measured in prison sentence days) for those convicted in a four-year window around the 1993 and 1999 reforms, respectively. For each reform, we look at the distribution for three sub-samples: those who are convicted and start their sentence prior to the reform (solid line), those convicted and start their sentence after the reform (dotted line), and those convicted before but start their sentence after the reform (dashed line). We refer to this last sub-group as the 'straddle sample'. These figures demonstrate that the sentence length distributions do not change around the reforms. There is no evidence of a 'shift down' in the treated sentence month bin regions. Rather, the sentence length distributions lie practically on top of each other for each sub-sample. Thus, there is no evidence of manipulation in sentencing. Furthermore, Appendix Figure 2 provides evidence that the reforms did not systematically affect the share of cases that received a waiver of prosecution, a summary sanction order, or a courtroom conviction. Nor did it affect the use of various sanction types: prison, fines, or other sanctions.

These findings are not surprising given the lack of plea-bargaining in the Swedish judicial system. The prosecutor must charge a defendant with a specific crime(s) in agreement with the evidence, limiting the extent to which defendants could in practice be charged with a lesser crime.[21] Importantly, the prosecutor is not involved in sentencing, which is left to the judge

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[20] Bjerk (2005), Ulmer et al. (2007) and Starr and Rehavi (2013) study the effect of sentences on the discretionary behavior of prosecutors. Bindler and Hjalmarsson (2018) show that historical English juries were more likely to convict upon the abolition of capital punishment.

[21] Though prosecutors routinely drop lesser charges to focus on more serious ones, we see no change around the reforms in this probability, using the Swedish National Council for Crime Prevention's suspects register.

and lay judges; discretionary sentencing decisions are limited by the sentencing 'window' (the minimum and maximum sentence for each crime) being pre-specified and relatively narrow.

5.2. Was the Reform Implemented Correctly?

Exposure to the reform should be determined by a defendant's conviction date. A person convicted before the reform should serve the share of time prescribed under the earlier regime, unless the sentence starts after the reform *and* the post-reform regime is more lenient. As both the 1993 and 1999 reforms increased the share of time-served for all sentencing bins (besides 2-months in 1999), the conviction date rather than the start date should in theory be the binding date for our analysis sample of 4-48 month sentences. In practice, however, we find evidence that the prison authorities did not strictly adhere to this policy. This can be seen by returning to our three sub-samples (pre, post, and straddle) in Panels C and D of Figure 3. These figures display the distribution of the share of time served for each sub-group. Though the distributions for the straddle sample (dashed line) should look like that of the pre-reform sample (solid line) – i.e. the straddle sample should not be treated – we find clear evidence that these intermediate samples are partially treated: for both reforms, the share of time served for the straddle sample is markedly shifted to the right. Moreover, the share of time served shows concentrations of observations around the value prescribed after the reform – two-thirds; this implies that at least some of the straddle sample was treated by the prison authorities. Therefore, as highlighted previously, we exclude individuals at risk of such measurement error in the assignment of treatment exposure, i.e. those convicted before but who start their sentence after the reform.

5.3. Relevance: The Impact of the Reform on Share of Time and Prison Days Served

How did the share of time and actual number of days served in prison change in each sentence bin? For the sample of convictions within two-years of July 1993, Panels A and B of Figure 4 present the results of regressing the share of time and number of days, respectively, served on a dummy indicating whether the conviction occurred after July 1, 1993 separately for each 0-48 month sentence bin. Vertical bars are placed at 4, 12, and 24 months, as these are the treatment thresholds defined in the law. Consistent with the law, there is no effect for 0-3 month sentences. For 4-12 month sentences, share served significantly increases (by 13-16 percentage points) in each bin. Thus, the reform had a large and significant impact on the intended sentence month bins, the magnitude of which was close to that prescribed by the law. The effect on share of time served decreases as sentence length increases from 13 to 24 months, with no visible effect for sentences of 20 months or longer; this is consistent with the theoretical effect of the

reform only being 3 percentage points for a 20-month sentence, and zero for sentences longer than 24 months. Despite the equal treatment with respect to the share of time served in the 4-12 month bins, there is an increasing effect on the number of days served, ranging from 17 days for 4-month sentences to 59 days for a 12-month sentence. The number of additional days served decreases with sentence lengths in the 13-24 month range.

Panels C and D of Figure 4 demonstrate the same pattern for the 1999 reform: (i) Share of time served only changes for bins that should be affected, (ii) these changes are consistent with the reform, as the share of time served decreases in the 2-month bin but increases for sentences longer than 12 months (with larger effects for longer sentences) and (iii) the magnitudes are close to what the reform predicts. Panel D shows that the effect on the number of days served increases up to a maximum of more than 130 days for 25-month sentences.

We summarize the relevance of the reform by estimating our baseline specification from equation (1) but replacing the dependent variable with the number of days served in prison. These results are presented in Panel A of Table 2. For the full sample (column (1)), being *Treated* or fully exposed to the two-thirds reform on average increases the number of days served in prison by 46, with an associated F-statistic of 109. Each additional column of Table 2 corresponds to the sample that is alive and has never emigrated from Sweden up until date t (1, 2, 3, and 10 years) relative to release. The relationship between reform exposure and prison days served is completely insensitive to sample attrition due to death or emigration, suggesting that the reform was not differentially applied for those with differential mortality or migration propensities. Moreover, Appendix Table 3 demonstrates that the reform is relevant across a wide range of sub-samples characterizing the offender's current and past offense history, demographic and socioeconomic characteristics, and pre-incarceration health. These results point towards the validity of our heterogeneity analyses.

5.4. Conditional Independence

For the reduced form estimates to be interpreted causally, the early release regime to which an individual is exposed should be unrelated to individual defendant and case characteristics that could also affect the defendant's post-release outcomes. Such correlation could occur, for instance, if there was a systematic response on the part of the criminal justice system (e.g. judges or prosecutors) to the reform; we have already shown in Section 5.1 that this is not the case. Panel B of Table 2 tests conditional independence more directly by presenting the first stage when excluding all observable controls, X. If reform exposure is unrelated to these characteristics, then their exclusion should not change the estimates. This is what we find.

Appendix Table 4 directly tests whether observables – current and past offense characteristics, demographics, socioeconomic characteristics, pre-incarceration health – change discontinuously with exposure to the reform. As identification is driven by temporal variation in exposure to the reform, one may be particularly concerned about other contemporaneous changes in society, such as Sweden's economic crisis in the early 1990s. Specifically, we estimate our baseline specification with each observable characteristic as the dependent variable (for 20 such variables). Only one coefficient – the number of contemporaneous crimes – is significant at the 5% level while most others are close to zero and/or far from significant. The lack of change in these observables is also supported by the robustness of our results (shown later) to the inclusion/exclusion of all observable controls.

Violation of the conditional independence assumption can also occur if other contemporaneous criminal justice reforms systematically affect the sentence that defendants with certain characteristics should receive. Our analysis already suggests this is unlikely, as defendant and case controls did not affect our estimates of the relevance of the reform in Table 2. This is unsurprising given that other reforms in this period only affected shorter sentences of 0-3 months (which are not included in our estimation sample of 4-48 months), drunk driving offenses (of which there are very few in our estimation sample), and youths (who are excluded from our estimation sample).[22]

6. The Effect of Reform Exposure on Mortality

6.1. Baseline Results

Table 3 presents the effect of exposure to the two-thirds reform (i.e. the coefficient on *Treated* in equation (1)) on the chance of death overall and death by cause measured t (12, 24, 36, and 120) months since date of release. Overall, exposure to the two-thirds reform *reduces* the chance of death; i.e. increased prison exposure improves health as measured by mortality. However, these results are only significant (at the 10% level) in the 24 month window in Table

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[22] In February 1994, Sweden passed a series of reforms targeting drunk driving. Although the law text was made harsher (it lowered the blood alcohol content threshold and raised the maximum allowable punishment), it also increased the availability of substance abuse treatment programs, which in practice led to fewer individuals spending more time in prison (BRÅ 1998:7). Our summary statistics table showed that dui offenses are concentrated in the 0-3 month sentencing bins and, hence, outside of our estimation sample. A pilot program for electronic monitoring in the home for those with 1-2 month sentences began in August 1994. It was expanded to 3-month sentences and the whole country in 1997 and was made permanent in 1999. Our 4-48 month estimation sample is unaffected by the introduction of electronic monitoring. Two additional sanction types were introduced in January 1999. Community service with probation could be used as an alternative to prison sentences of three months or less and secure youth treatment centers could be used instead of prison for young offenders. Again, our sample is unaffected by these reforms.

3. Panel A of Figure 5 traces out the dynamics of the effect at each of the first 10 years post-release. Though these results are somewhat imprecise, we highlight here that there is never a positive coefficient on the overall chance of death for the whole sample and, in fact, some of these estimates become more precise in the robustness checks presented in Section 6.2.

Moreover, these aggregate results mask important heterogeneity in the effect of prison on (i) the *cause* of death and (ii) the chance of death for different sub-populations. Specifically, as highlighted earlier, this population has a disproportionate amount of mental health problems. Moreover, criminals lead generally risky lifestyles, which puts them into contact with violent individuals or neighborhoods. For these reasons, we also consider the effect of extended prison exposure on the chance of suicide and violent death. Drug and alcohol related deaths could be affected by treatment programs while in prison and a change in lifestyle, including the lack of or limited access to drugs and alcohol. Finally, lifestyle changes in prison (e.g. controlled diets and exercise) and regular healthcare can even affect natural causes of death. We consider the three largest such categories: circulatory, digestive, and cancer related deaths. These lifestyle behaviors may be most relevant for circulatory disease. It is harder to identify direct channels through which digestive and cancer related deaths can be affected, except perhaps by disease diagnosis; we note that smoking is not banned in prison. Table 3 presents the effect of being *Treated* by the reform on each cause of death 12, 24, 36, and 120 months post-release. The dynamics are traced out for suicide, violent and circulatory death in Figure 5 and cancer, digestive and alcohol and/or narcotics in Appendix Figure 3.

The results in Table 3 show that increased prison exposure indeed has significant effects on a subset of causes of death that are particularly relevant to the population studied. Most prominently, there is a significant reduction in the chance of suicide in both the short (12-36 months) and long term (120 months); Panel B of Figure 5 confirms that this effect is seen in each of the first 10 post-release years. How large is this effect? Relative to the mean post-release suicide rates (at the bottom of the table), reform exposure reduces the chance of suicide by 79% in the first three years; a 38% reduction remains 10-years post release.

Turning to violent deaths, we find a negative coefficient for the first three post-release years, which switches in sign by year 10. The immediate (12-month) short-run reduction in the violent death chance is significant at the 10% level; while it does not change in magnitude over the next two years, precision decreases. Given that violent death is relatively rare, even for this population, the relative magnitude of these effects are quite large (a 100% reduction). Panel C of Figure 5 shows that the reduction in violent deaths is short-term in nature, and by year 4 post-release the estimates have swung (permanently) positive.

The next set of results in Table 3 is for circulatory, cancer and digestive related deaths. For the first three years in each category, there is an insignificant effect. However, in the 10-year post release long-term, there is a significant reduction (10% level) in the chance of a circulatory death: reform exposure reduces the chance of circulatory death by 27%. Panel D of Figure 5 demonstrates that this general health improvement seen at year 10 is not an anomaly but rather shows up (significantly) about five years post release and then persists. Considering that these causes of death are more common as individuals age and often attributed to life-style behaviors over an extended period of time, it is perhaps not surprising that this effect is only seen in the medium and long-run. This pattern is not seen for cancer but one that is similar (yet mostly insignificant) is seen for digestive deaths (Panels B and C of Appendix Figure 3).

The final panel of Table 3 shows the results for narcotics and/or alcohol related deaths that are not already attributed to one of the above ICD10 codes. There is no significant effect at any of the short and long-term periods shown in the table. The dynamic path for these alcohol/narcotic related deaths is traced out in Panel A of Appendix Figure 3. Though the point estimates swing positive at year 4, they are never significant.

6.2. Robustness Analyses: Specification, Culling, At-Risk Date, and Sample Decisions
Table 4 demonstrates the robustness of these results to specification and estimation decisions. For comparison purposes, the baseline results are presented in Panel A for any death and suicide measured 12, 24, 36, and 120 months post release and short-term violent death (12 months) and long-term circulatory death (120 months). Panel B demonstrates robustness to excluding all observable controls. To the extent one is concerned about differential trends across sentence month bins (in the spirit of difference-in-differences designs), Panels C and D present the results of including sentence month bin by year fixed effects and bin specific conviction month trends, respectively. Panels E and F demonstrate robustness to specification checks motivated by the parallels of our design to regression discontinuity analyses. Panel E drops a 4-month donut around each reform, demonstrating robustness to any potential manipulation (minimal or non-existent though it may be) while Panel F includes a quadratic conviction month trend. Finally, Panel G replaces the baseline prison sentence month bin fixed effects with prison sentence day fixed effects, comparing individuals with exactly the same sentence, while Panel H presents marginal effects from a probit specification. Though precision increases with the latter, nothing becomes significant that was not at least marginally significant in the baseline.

Another concern is whether the results are driven by a culling from the sample of the 'least healthy' individuals. 53 individuals with sentences of 4-48 months died in prison. Were

such deaths systematically more likely after the reform? A simple look at the data suggests that this is unlikely, as these deaths are fairly evenly distributed across sentence bins and over time. They are also not concentrated amongst suicides, but rather the largest share is circulatory, digestive, and cancer related (25% combined). We more formally address this question by estimating the baseline specification where the dependent variable is death in prison. But, we find no evidence that reform exposure significantly affects the chance of death in prison (results available on request).

Finally, Appendix Table 5 demonstrates robustness to the choice of at-risk date and inclusion/exclusion of the straddle sample. Panel A presents the baseline (at-risk from prison release and excludes the straddle sample) while Panel B allows individuals to be at-risk as of their prison start date. In doing this, we are again including those individuals who died in prison, and controlling for possible culling effects. Similar mortality reducing effects are still seen. The effects on overall death are negative (and significant at 24 and 36 months post admission) while the significant suicide effect kicks in at 36 months and violent death at 24 months. To the extent that the previous findings were driven by *post-release* behavior, these somewhat delayed effects are consistent. Panels C and D include the straddle sample (i.e. those convicted before a reform but whose sentences start after) using the prison start date and end date to measure at-risk, respectively. The same pattern and significance of results in seen, with in most cases slightly smaller coefficients – consistent with the idea that including the straddle sample induces measurement error in the treatment variable.[23]

6.3. Heterogeneity in Mortality Results

The baseline results highlight that increased prison exposure improves prisoner post-release health in multiple dimensions: the overall chance of death (though not quite significant), mental health (suicide) in the short and long-run, exposure to violence (violent death) in the short-run, and general health (circulatory death) in the long run. This section considers two dimensions of heterogeneity: (i) whether the cause of death results are driven by particular subsamples at highest risk of these types of deaths, and (ii) whether there are particular sub-samples for whom the reform significantly decreased (or increased) the overall risk of death.

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[23] The results are robust to a number of additional tests. As some individuals are seen multiple times in the data, we have tested for robustness to using just the samples of first 4-48 month sentences and last 4-48 month sentences; by default, these two samples will put more weight on samples treated by the 1993 and 1999 reforms respectively. Results are also robust to including the various subsamples dropped when creating the data: those with post trial detention only, trimmed high and low shares of time served, and juveniles.

Beginning with the cause-specific results, we first consider whether the reduced chance of suicide is driven by those with pre-identified mental health problems. We proxy for pre-incarceration mental health problems by looking at those who have ever been admitted to a psychiatric ward in the five years prior to starting their sentence. We also look at those admitted to a general ward and those never admitted to any hospital. Note that the first two categories are not mutually exclusive. Pre-incarceration hospitalization is a strong predictor of suicide. The suicide rate in each sub-sample, measured 10 years post release is: 2% (psych admission), 1.3% (general admission), and 0.6% (no admission). Panel A of Figure 6 presents the 10-year dynamics for each sub-sample. There are persistent effects that are largest for those previously admitted to psychiatric wards, while there is a smaller but still significant effect for those in general wards (these effects are not significantly different from each other). There is no significant effect for those who were 'healthy' pre admission. Exposure to the two-thirds reform reduces the 10-year post-release chance of suicide by about 75% and 50% for the psychiatric and general ward samples, respectively.

Panel B of Figure 6 assesses whether the violent death effect is driven by violent offenders: the results are presented separately for those whose current offense is violent (solid circle), property (open circle) and drugs or alcohol (square). Summary statistics show that violent offenders are at the highest risk of violent death: the 10-year violent death rate is almost twice as large for violent (0.7%) than property or drug/alcohol offenders (both 0.4%). Figure 6 demonstrates that increased prison exposure only reduces the risk of violent death in the short-run for violent offenders; moreover, this effect is no longer just significant 12 months post-release, but rather lasts for 36 months. For property offenders, there is no effect on the chance of violent death, while a significant increase is seen for drug and alcohol offenders that begins within 2 years of release, and persists over time. The latter could occur, for instance, if more time in prison reinforces and/or expands drug networks (i.e. criminal capital) that results in more exposure to violence upon release (see Bayer et al (2008)). The former could occur if violent offenders end up 'incapacitated' by returning to prison (we will return to this when looking at recidivism) or if they are removed from crime and criminal networks.

Finally, we turn to the long-run effect on circulatory deaths, which are more prevalent for older populations. Panel C of Figure 6 splits the sample according to whether individuals were older or younger than 33 (the median age) at the prison start date. The share of the older than 33 sample who died from circulatory related deaths 10 years post-release is 2.7%, compared to less than 0.4% of the younger sample. The medium and long-run reduction in circulatory deaths are driven by the relatively older sub-sample.

The heterogeneity results presented in these figures do not, however, paint the whole picture. First, even though reductions are observed in cause-specific deaths for these sub-populations, these do not necessarily translate into a reduction in the overall risk of death. Second, there may be sub-populations for which increased prison-exposure decreases the risk of death, which are not a high risk for a particular type of death. Table 5 demonstrates the heterogeneous effect of the two-thirds reform on subsamples characterized by (i) criminal history and offense characteristics, (ii) demographic characters, and (iii) pre-incarceration socioeconomic status and health characteristics. A number of interesting findings emerge.

First, the significant cause-specific effects for these high-risk populations do not always translate into a reduction in the overall chance of death: despite the large and significant reduction in the chance of suicide for those with mental health problems, the overall chance of death does not significantly decrease. This is in part because suicide represents such a small share of overall deaths and because of potential offsetting positive effects for drug and alcohol related deaths (though these are generally not significant).[24] Similarly, the increase in violent deaths for drug and alcohol offenders does not translate into an increase in overall mortality; rather there is still a negative (insignificant) overall effect, which is likely driven by a significant reduction in drug and alcohol related deaths. In contrast, the violent death effect for violent offenders does contribute to an overall (marginally significant) reduction.

The second finding is that there are indeed sub-samples for whom the overall chance of death decreases due to reform exposure: significant effects (often in the first two years post release) are seen for those with no past prison exposure, property offenders, younger offenders, and those with some past employment. Overall, increased prison exposure seems to improve the health of those who are positively selected in terms of the stage of their criminal careers or connection to society.

6.4. Heterogeneous Effects by Sentence Length?

Before turning to the mechanisms underlying these health improving effects of increased prison exposure, we consider whether these results are heterogeneous across sentence bins. Table 6 presents the results of expanding equation (1) by interacting reform exposure (*Treated*) with dummy variables indicating if the sentence is 4-12, 13-23, or 24-48 months. If there are differential effects across these bin groups, what would they capture? One possibility is heterogeneous treatment effects, given the previous findings that the effects of reform exposure

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[24] For these same high-risk subsamples, Appendix Table 7 presents results for death overall and by cause.

differ across subsamples and, from the summary statistics table, that sample composition differs across bins. Another possibility is that there are non-linear effects, given that exposure to the reform increases the number of days in prison more for longer sentences. Finally, given that the 4-12 and 24-48 month bins are treated by different reforms, it could be that the effects of the reform interact with the post-release environment; this differs substantially for the years around the 1993 and 1999 reforms, for instance, in terms of the labor market and economy.

For the most part, however, significant differences are not seen across sentence month bin groups. This is true for both death overall and for suicide: while the suicide effects are most precise and significant for the 4-12 month bin, we note that a disproportionate share of the sample lies in these bins and that the effect sizes relative to the bin specific means are fairly similar. The short-term violent death effects and longer term circulatory effects do appear to be driven by specific bins: the former by the 4-12 month bins and the latter by the 24-48 month bins. It is certainly feasible that the latter represents a non-linear effect, in that it is hard to imagine short-term lifestyle changes that can impact a cause of death that is long-term in nature.

7. Mechanisms and Discussion

Our analysis indicates that the increased exposure to Swedish prisons generated by the early release reform improved health, as measured by mortality. In this section, we want to assess whether the reform only affected the prison experience by increasing the number of days in prison or if it also entailed other significant changes to the prison experience. What is actually included in the reduced form package that we study? Then we go on to explore two potential mechanisms through which spending more time in prison could improve mental and physical health: the direct effect of healthcare provision in Swedish prisons and an indirect channel via the effect of more time in prison on recidivism and labor market outcomes.

7.1. Prison Days versus Other Changes to the Prison Experience

Thus far, we have focused on the fact that the early release reform increased the number of days inmates spend in prison. We have ascribed the entire reduced form effect to this increase. We now explore whether our reduced form findings could (at least in part) be driven by the effect of the reform on other aspects of the prison experience. We first argue that our identification strategy, which estimates the 'discontinuous' effect of exposure to the reform over and above trends in conviction date, actually rules out many of these alternative channels. This is because time served in prison changes discontinuously for prisoners convicted before

and after the reform, while these other characteristics, e.g. prison overcrowding, should change more gradually. That is, even though inmates will serve more days in prison, it is only when we reach the additional days that there should even begin to be a change in capacity or other environmental factors. This should rule out, for instance, that the observed reduced form effects are driven by differential economic conditions faced by individuals re-entering society with different release dates.[25] We provide qualitative and empirical evidence in support of this argument below – namely that other factors do not change discontinuously with the reform.

One expected consequence of increasing the share of time an inmate must serve is that (all else equal) the stock of prisoners will grow. Though this could lead to prison overcrowding and conditions that may be detrimental to inmates' health and well-being, such overcrowding is not observed immediately after the 1993 or 1999 reforms (see Appendix Figure 4). To further examine this possibility, we use data on all prison inmates from January 1992 to December 2004, including those not in our estimation sample. We calculate the number of inmates in each prison during each month and then average these across all prisons to construct a monthly time series. In Panel A of Appendix Figure 5, we see that there are no trend breaks in the average number of inmates per prison around (or just after) the reforms. The same is true for two measures of prison capacity utilization (see Panels B and C). Since the majority of inmates in our estimation sample reside in open and medium security prisons, we also created the same types of figures (available on request) by facility type (open, medium, and high security). There is no indication of overcrowding around (or just after) the early release reforms.

Another concern is that prison authorities may have reacted to the expected increase in prisoner numbers by changing: the types of facilities, programs or treatment to which they assigned inmates and the quality of care. Though we cannot observe program assignment during our sample period, we can test for such changes explicitly in facility assignment. We see no significant effects of reform exposure on the chance of being assigned to a facility classified dichotomously as low, medium, mixed (low and medium) or high security when re-estimating our baseline specification with facility type as the dependent variable.[26]

One may also be concerned that peer composition changes with the reform. While it may in the long-run, this should again not change discontinuously for individuals on either side of the reforms. To consider this channel, we proxy for peer quality with sentence length. Panel D of Appendix Figure 6 plots the average sentence length of inmates. This does not change around

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[25] Schnepel (2018) finds employment opportunities affect the recidivism behavior of offenders released from California prisons from 1993 to 2008.

[26] The estimates are very small and display no regularities. Regression results are available upon request.

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(or just after) the reforms. Nor does it change when looking at open, medium security, and high security prisons separately (available on request). Taken together, these figures support the idea that the average quality of an inmates' peers (as measured by the length of their sentence) while in prison does not change discontinuously around the reforms.

Finally, did the reform change the post-release experience? We remind the reader here that the early release reforms did not change the amount of time spent on active probation, since this is 12 months for all former inmates regardless of their initial sentence length.

Taken together, the results from these exercises lead us to believe that the main effect of the reform is, in fact, through days served and not through other changes to the prison experience. [27]

7.2. Healthcare Provision and Treatment Programs in Swedish Prisons

This section demonstrates that more time in a Swedish prison could feasibly directly improve an inmate's health. As discussed in Section 2.2, Sweden is known for its relatively high quality prison conditions, including the provision of health care and treatment programs. We provide further insight into the healthcare utilization of Swedish inmates by studying all inmates with 4-48 month sentences entering the prison system between 2009 and 2013 (N=37,054). Though outside the sample frame of our core analysis, much more information is available from the Swedish Prison and Probation Service for these cohorts, including: (i) all visits to doctors, nurses, and psychologists while in prison, (ii) the administration of medicines, and (iii) various treatment programs in which inmates are enrolled.

Health care variables by sentence month bin are shown in Figure 7. These exclude the initial health examination that all inmates receive upon intake. The extensive margin variables in Panel A show us that the take-up rate of healthcare services in prison is quite high and increases as inmates spend more time in prison. Panel B depicts the average number of doctor, nurse, and psychologist visits inmates make during their time in prison. High utilization intensity that increases with time spent in prison is seen for all intensive margin variables. Panel B also tells us that inmates are most likely receiving the medication that they need.

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[27] Earlier versions of the paper present an additional test of these arguments, which relies on the idea that as one gets closer to the reform dates, one can increasingly rule out that anything else changes in the prison experience besides time served. We thus re-estimate our baseline specification for smaller and smaller windows around the 1993 and 1999 reforms separately, beginning with individuals convicted +/- 2 years from the reform date. Though similar patterns of results are seen (suggesting other aspects of prison experience do not change discontinuously), there is a substantial loss in precision, especially with a window of 6-months on either side of the reform.

Furthermore, necessary medicines are administered by trained personnel on a daily basis, which may actually help some inmates properly follow medication regiments.[28]

Many inmates take part in professional treatment programs to help address mental health issues and/or alcohol and drug abuse. Panel A of Figure 8 depicts the share of inmates (by sentence month bins) who complete any such program. We also show the share who complete one of the three most widely used programs (all of which focus on mental well-being and substance abuse): (i) motivational interview, (ii) Alcoholic/Addicts Anonymous' 12-step program, and (iii) the Correctional Service of Canada's offender substance abuse pre-release program (OSAPP). Similar programs were also available to inmates during the 1990's.

Figure 8 shows that more than 80% of those serving sentences longer than 24 months complete at least one such program (some complete more than one). Notably, the probability of completing a treatment program rises rapidly as we move from short to medium length sentences – but levels off for those serving long sentences. The U.S. National Institute on Drug Abuse argues that "one of the most reliable findings in treatment research is that lasting reductions in criminal activity and drug abuse are related to length of treatment. Generally, better outcomes are associated with treatment that lasts longer than 90 days, with treatment completers achieving the greatest reductions in drug abuse and criminal behavior." (NIDA 2014, p. 20) Thus, staying somewhat longer in prison may improve the health of inmates by increasing the efficacy of the treatment programs in which they are engaged.

Importantly, several treatment programs are given in more than one version. For example, AA's 12-step program is given in both basic and extended versions, with 200 and 400 hours of course participation, respectively. The extended course is only offered to (and completed by) those sentenced to at least 12 months and actually serving at least 8 months in prison (see Panel A in Figure 8). Thus, in many instances, both the chance of completing a treatment program and the program's intensity increase as inmates spend more time in prison. Moreover, Panel B in Figure 8 also demonstrates that the quantity of treatment increases with time served, as inmates with longer sentences typically complete multiple program.

With this additional descriptive evidence in hand, we conclude that the Swedish Prison and Probation Service supplies a large amount of healthcare services and treatment programs

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[28] Non-adherence to prescribed medication regimes is a leading cause of preventable morbidity and mortality (Krueger et al. 2005). U.S. and Swedish prison officials are acutely aware of this issue. Though the Swedish Prison and Probation Service cannot force an inmate to take his/her medicine, they do provide additional services/information to those at-risk of non-adherence. The U.S. Department of Justice argues that the structured environment of prison can be used to boost adherence among those with traditionally low levels of adherence, but also stress the need to coordinate with post-release health-care services to maintain adherence outside of prison (USDJ 2012).

to inmates, many of which emphasize mental health and substance abuse. In-prison take-up rates are high, and increasing with sentence length (and time served). These facts can potentially explain why longer prison exposure improves both mental and general health.

7.3. Recidivism and Labor Market Outcomes

Table 7 presents the results of applying our identification strategy to outcomes measuring recidivism behavior and labor market performance for the first three years post-release. Panels A-C consider three measures of recidivism: any conviction, more than one conviction, and any prison. On average, being treated by the two-thirds reform *reduces* post-release recidivism. Negative coefficients are seen for all outcomes and years. They remain negative 10 years out for having more than one conviction and returning to prison. Specifically, exposure to the reform reduces the chance of returning to prison within two years by almost 4%.

Panels D and E present the results for being employed in each of the first three post-release Novembers and annual earnings in 1990 prices. Means of the dependent variables (in italics) highlight the disadvantaged nature of this population: within the first year of release, just 14% are employed in November and the average labor market earnings in the first year after release is around 22,000 Swedish kronor (the median is 0). The results indicate that exposure to the two-thirds reform has a short-term beneficial effect (in the first 12 months), but that this effect disappears thereafter. There is a significant increase of two percentage points (almost 16%) in the chance of being employed in the first post-release November.[29]

The first takeaway from these recidivism and labor market results is that exposure to the reform does not yield worse post-release behavior in terms of criminal activity and labor market participation. Moreover, there is evidence that individuals commit fewer crimes and are less likely to return to prison. This is consistent with Bhuller et al.'s (2020) findings that incarceration (at the extensive margin) in Norway reduces recidivism, and an increasing number of papers that reach similar conclusions.[30] While these results are important in their

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[29] These findings are similar to the decrease in unemployment and increase in earnings seen as a result of increasing violent offender incarceration lengths in Denmark, with the exception that the Danish labor market effects persisted longer (Landerso, 2015).

[30] See Kuziemko (2013) and Rose and Shem-Tov (2019) as well as Hjalmarsson (2009), who studies juveniles in Washington state. This is also consistent with Hinnerich et al. (2016), whose study of Swedish drunk driving sentences finds a reduction in post release offending for those sentenced to a minimum-security institution or electronic monitoring rather than probation. Dobbie et al. (2018) also study Swedish prisons and find little effect of incarceration at the extensive margin on recidivism (though the main focus of this paper is on child outcomes); of course, the offender on the margin of being sentenced to prison or not is also very different than our sample of 4-48 month sentences.

own right, the recidivism analysis can also speak to the mechanisms underlying the health effects of increased prison exposure.

First, one possible explanation of a reduction in mortality is that more time in prison increases the chances that one returns to prison. If this protective and healthy Swedish prison environment 'incapacitates' death while incarcerated, then this could be a mechanical explanation of our mortality results. On the contrary, however, we find that reform exposure *reduced* recidivism.

A second possible explanation is that the reduction in mortality is not driven by a direct improvement in health but rather indirectly by the effect of more prison time on the post-release lifestyle and environment of offenders. Though not made worse, earnings and labor market outcomes are not made substantially better, at least not such that they could affect life decisions, such as health care utilization, neighborhood, and living conditions. Yet, the overall improvement in recidivism and short term employment effect means that we cannot yet rule out this indirect channel.

To say something further about the importance of this mechanism, Appendix Table 7 presents a heterogeneity analysis for recidivism and employment. The first takeaway is that the short-term improvement in labor market participation is seen for almost every sub-sample. The second takeaway is that the reduction in recidivism is driven by: those with a prison history, property offenders, older offenders, and those with no recent history of employment. These subsamples generally represent individuals who are negatively selected in terms of their criminal careers or connections to society. This conclusion contrasts sharply with that for the mortality heterogeneity analysis (Table 5), making it hard to accept a life-style improvement explanation as the only mechanism underlying the reform's health improving effects.

8. Conclusion

In stark contrast to previous correlational literature, we find that exposure to Sweden's two-thirds early release reform, which increased time served in prison, does not harm post-release health, and actually improves it. The overall chance of death, especially in the short term, is lowered for sub-samples who are positively selected in terms of their criminal careers and connection to society. Moreover, even in the whole sample, increased exposure to prison reduces the chance of causes of death that that are particularly relevant for this high-risk population. Most prominently, there is a significant and persistent reduction in the chance of suicide, which is driven by those with previous mental health problems and violent offenders.

Our analysis of the potential mechanisms underlying the physical and mental health improving (reduced form) effect of the reform yields three conclusions. First, these effects are driven by the first-order impact of the reform on days served, rather than changes in other aspects of the prison experience. Second, improved post-release health is plausibly (and likely) driven by a direct effect of healthcare and treatment in prison. We demonstrate that healthcare provision is high in Swedish prisons, and fundamental to its role as a mechanism, utilization and treatment increases with time served. Third, though there is some evidence that the reform improved post-release outcomes more generally (i.e. lower chances of recidivism and a (very) short-term improvement in employment), there is not sufficient evidence to conclude that these indirect effects explain the reduction in mortality.

What are the policy implications of these findings? The answer to this question is of course dependent on the Swedish context of the study, i.e. a country with amongst the highest per prisoner expenditures in the world. Thus, the main policy implication cannot simply be that more time in prison improves prisoner health and outcomes; this clearly depends on the quality of the prison conditions. Rather, this paper demonstrates that more time in prison *can* improve post-release health. These findings may seem surprising at face value, especially in light of the existing correlational evidence. However, even the most careful U.S. study to date can conclude that prison did not harm post-release health (Norris et al., 2020). Moreover, the policy implications of our research are closely related to the channel via which these health improving effects occur. As we argue that healthcare and treatment program participation in prison plays a critical role, our findings emphasize the potential importance that improving prison conditions (including health care) can have on post-release outcomes and reintegration success. To the extent that pre-incarceration health care access may be more limited in other countries, high quality prison health care could even have larger beneficial effects than those we find in the Swedish context.

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Figure 1. Sweden's Early Release Policies 1990-2002
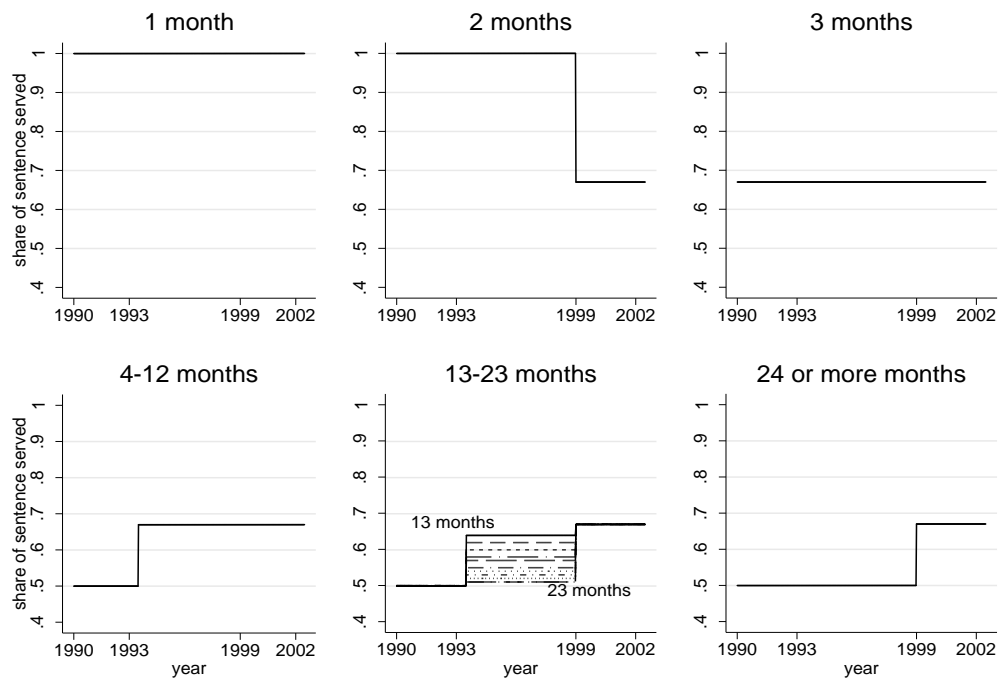
Note – This figure depicts the share of a prison sentence that must be served as stated in Sweden's early release law by sentence length and conviction year. In July 1993, share served was increased for those with sentences of 4 – 12 months in prison from one-half to two-thirds. Those with sentences of 13 – 23 months were required to serve 8 months plus one-third of the time exceeding one year. Those serving 24 months or more were unaffected. A second reform was carried out in January 1999 that required all inmates serving 2 or more months in prison to serve two-thirds of their sentences.

Figure 2. Descriptive Statistics: Dynamic Paths of Mortality, Recidivism, and Hospitalization
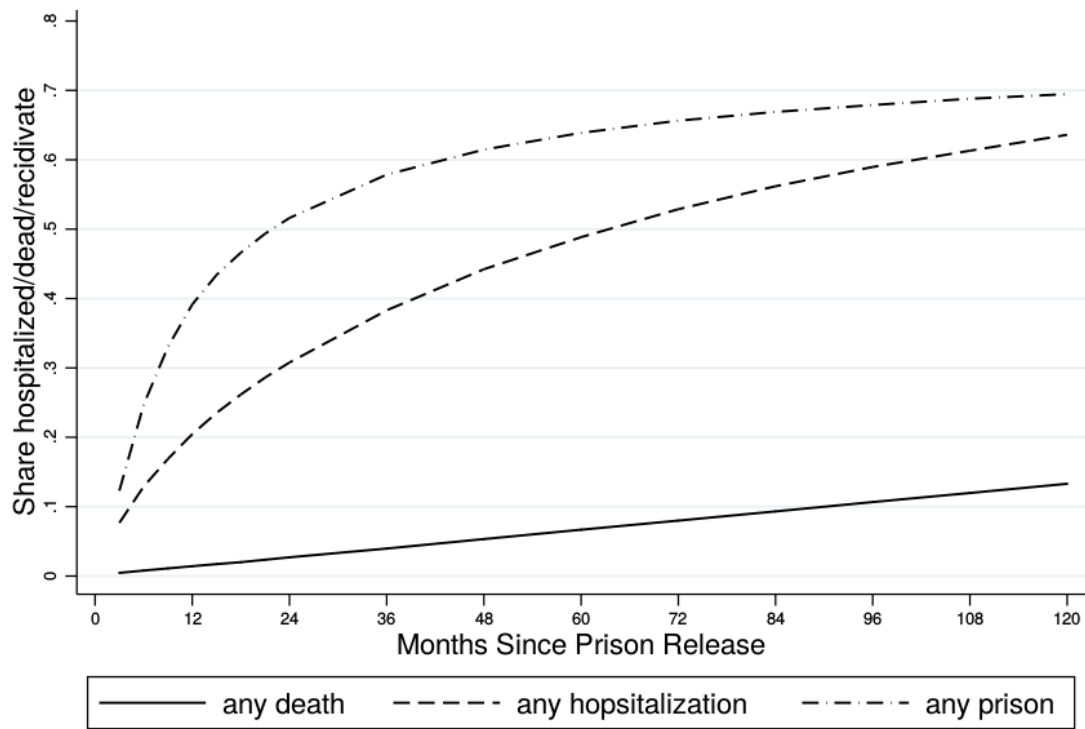
Note − For the all 4-48 month sentences, this figures shows the dynamic paths for death due to any cause (solid line), any hospitalization (dashed), and any prison (dash-dot), which are measured at *t* months post release, and condition on the sample alive and in Sweden at that time.

Figure 3. Kernal Densities: Sentence and Share Time Served Distributions for Pre-Reform, Post-Reform, and Straddle Samples

Panel A. Sentence Distribution for 1993 Reform Samples
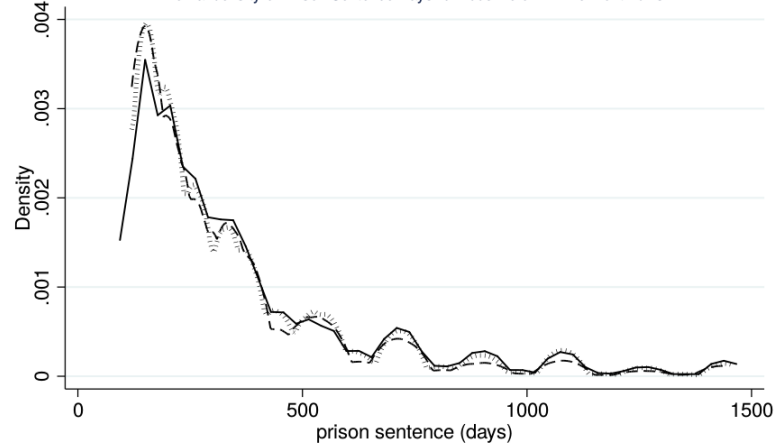
Panel B. Sentence Distribution for 1999 Reform Samples
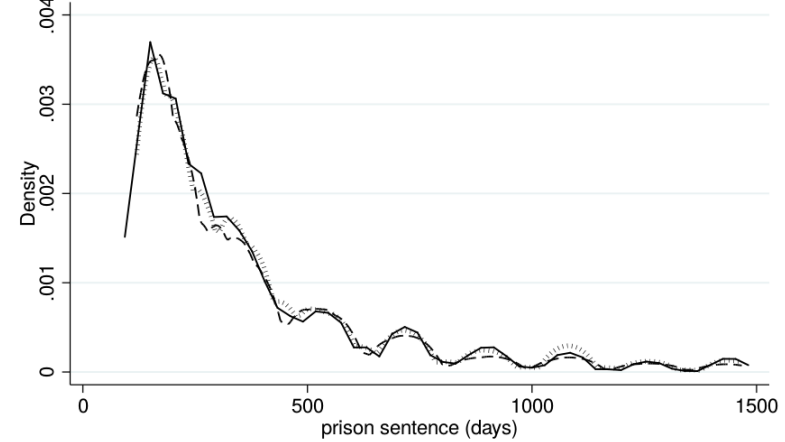
Panel C. Share Time Served Distribution for 1993 Reform Samples
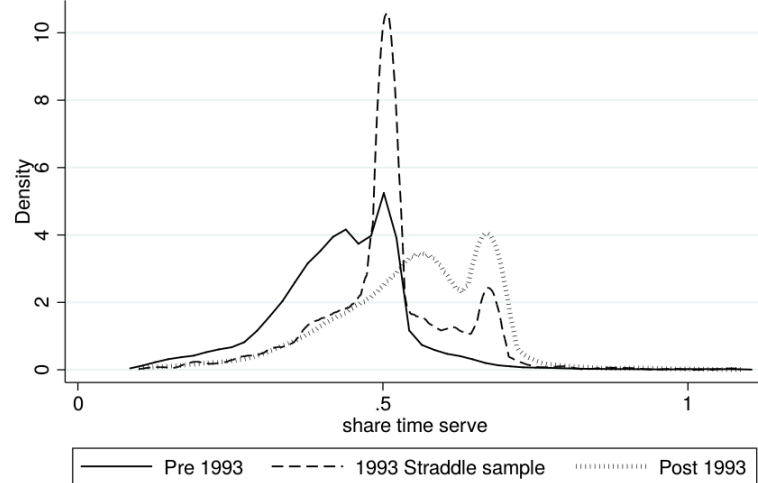
Panel D. Share Time Served Distribution for 1999 Reform Samples
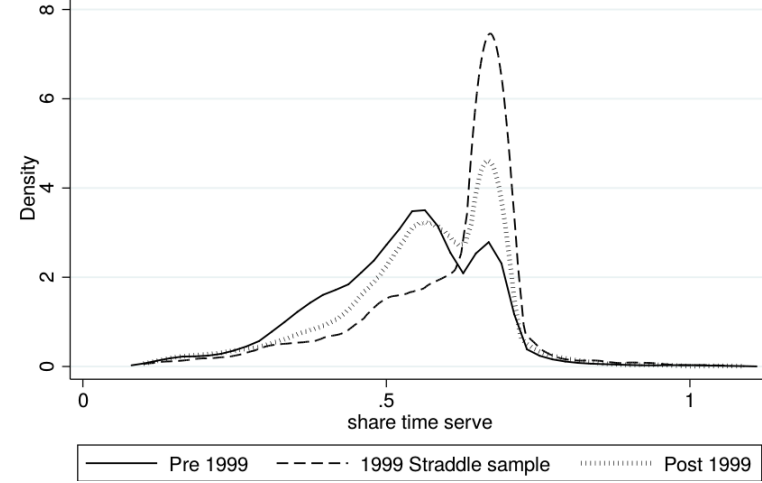
Note – This figure uses kernel densities to demonstrate the distributions of prison sentences in days and the share of time served (Panels A and C for the 1993 reform sample and Panels B and D for the 1999 reform sample). The 1993 (1999) samples include all individuals convicted within 2 years of the reforms (on either side of the cutoff). We decompose these samples into three subsamples: pre (solid line, conviction and sentence start date pre reform), post (dotted line, conviction and sentence start date post reform), and straddle (dashed line, conviction pre and sentence start date post reform).

Figure 4. Implementation of 1993 and 1999 Reforms: Effect on Share of Time and Actual Days Served by Sentence Length Bins

Panel A. 1993 Share Time Served
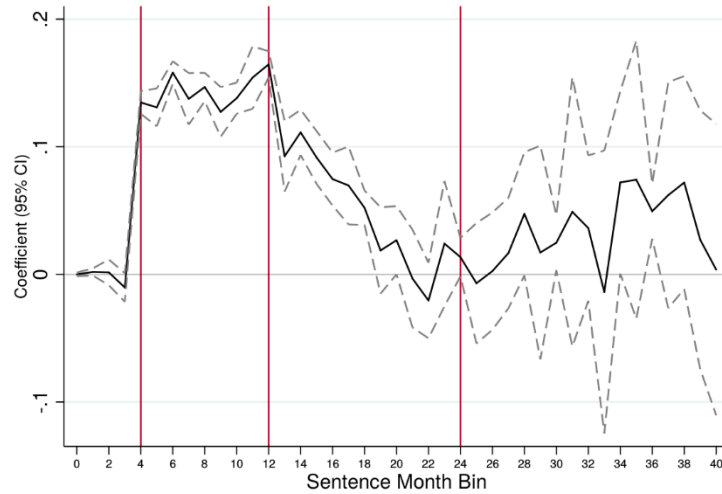
Panel B. 1993: Prison Days
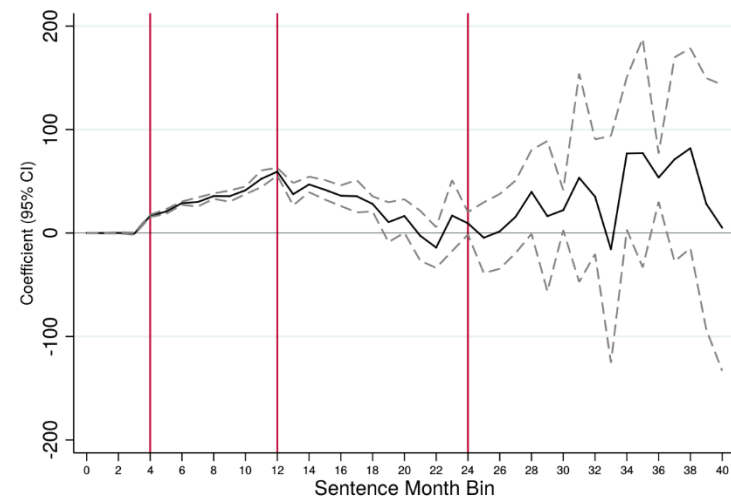
Panel C. 1999 Share Time Served

Panel D.1999 Prison Days
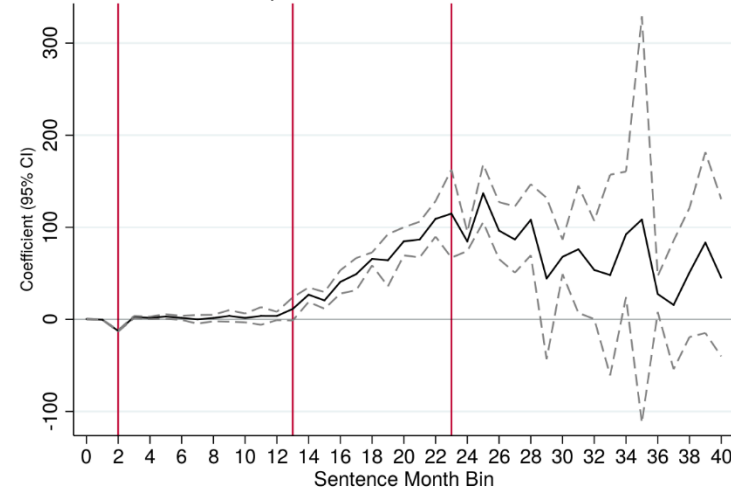
Note – Panels A and C present regressions of the share of time served on post reform dummies for the 1993 and 1999 reforms separately, using samples of convictions in a 4-year window around each reform; Panels B and D show the same thing for prison days served. Results are estimated separately for each sentence month bin; the coefficients and 95% confidence interval are plotted.

Figure 5. Dynamics of Mortality Effects

Panel A. Death (Any Cause)
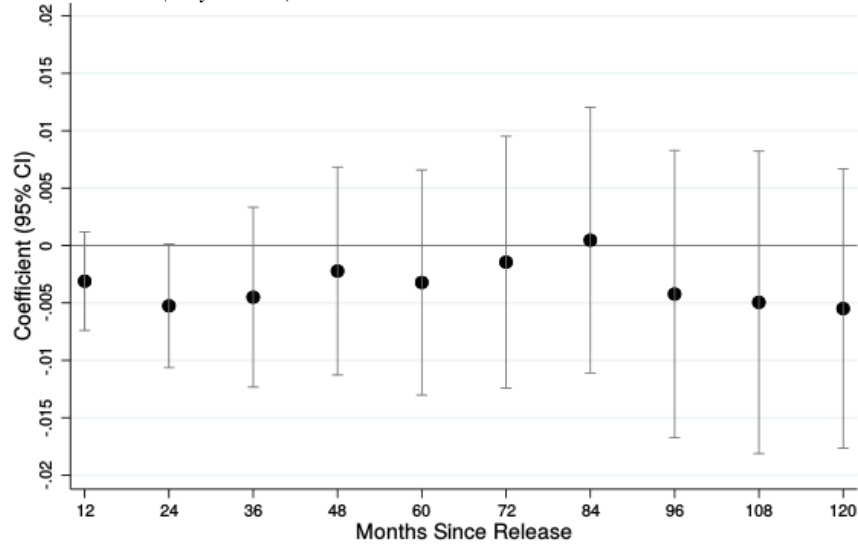
Panel B. Suicide
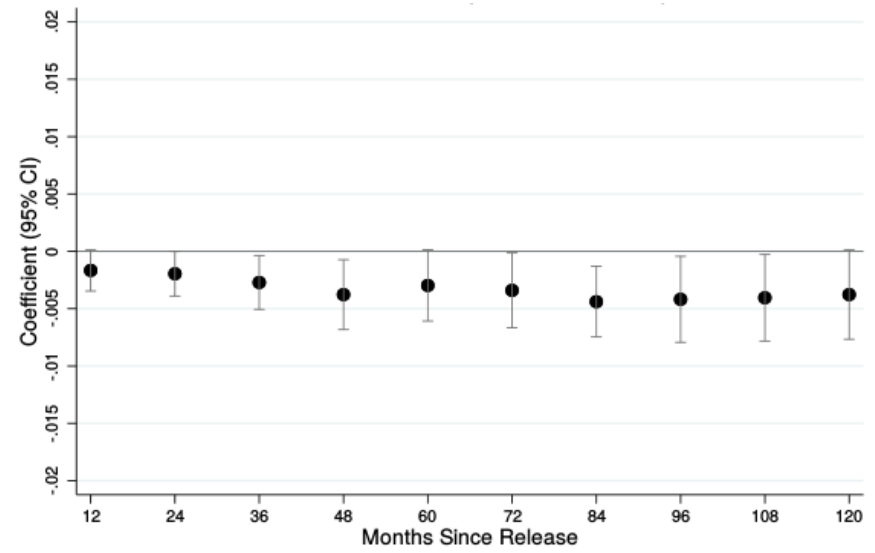
Panel C. Violent Death
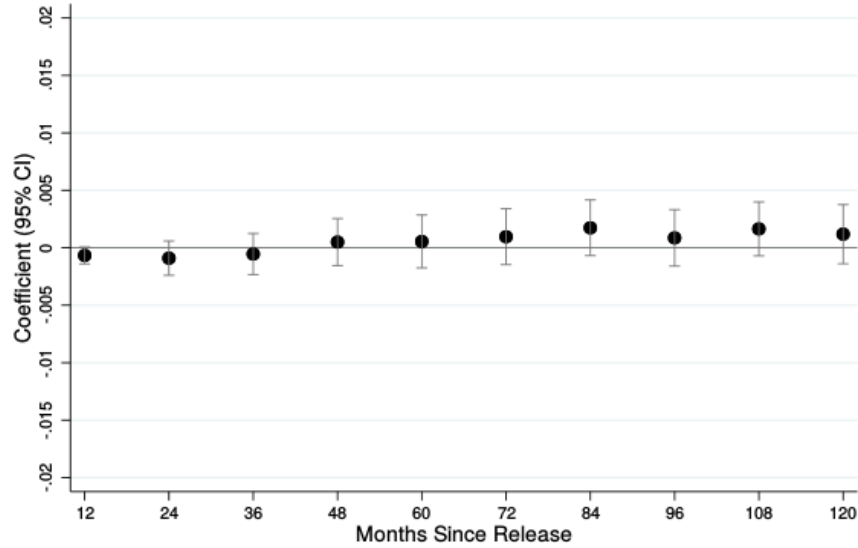
Panel D. Circulatory Death
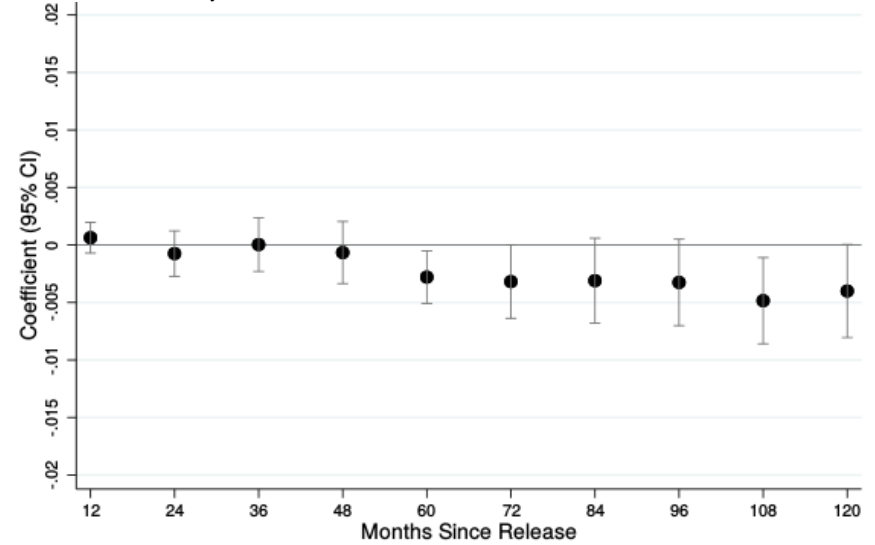
Note – Panels A-D present the estimated coefficient and 95% confidence interval of exposure to the reform (*treated*) on the outcome listed measured at *t* months since release. All specifications condition on not having migrated from Sweden by month *t*.

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Figure 6. Heterogeneity Analysis of Mortality Effects for High Risk Sub-Groups
Panel A. Suicide Effects by Pre-Incarceration Hospitalization
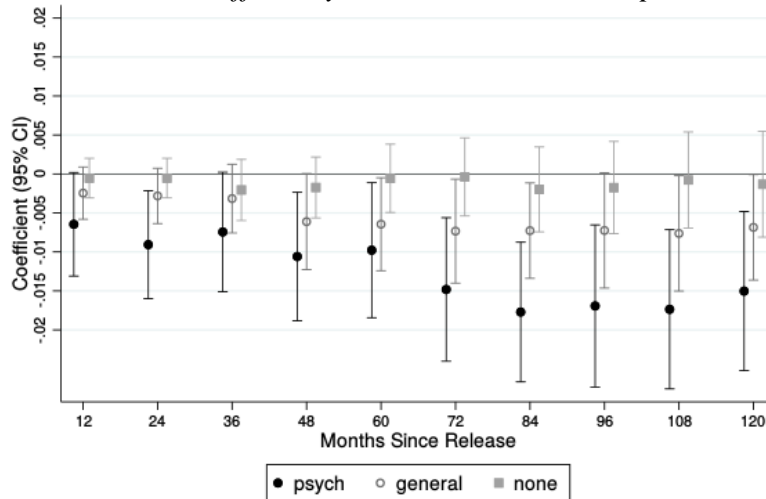
Panel B. Violent Death Effects by Current Offense Type
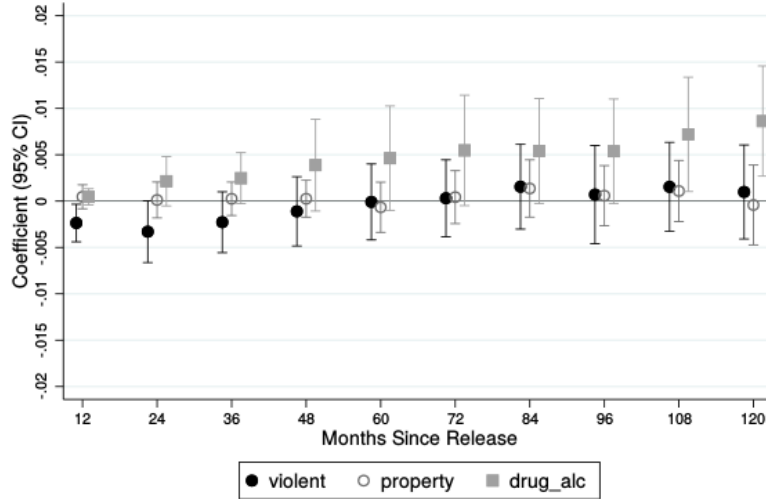
Panel C. Circulatory Death Effect by Age at Incarceration
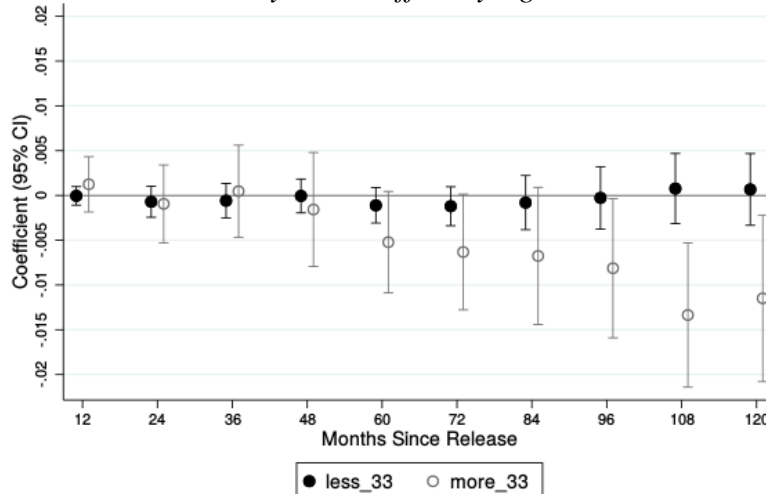
Note – These figures present the estimated effect of exposure to the reform (*treated*) on mortality measured at *t* months since release. Panel A considers suicide and presents the results separately by pre-incarceration hospitalization in a psychiatric ward (N = 9,897 at 12 months), general ward (N = 19,176 at 12 months), or no hospitalization (N = 22073 at 12 months). Panel B considers violent death and presents the results by current offense type: violent (N = 15243), property (N = 20084) and drug and alcohol (N=6968). Panel C presents the circulatory death results separately by median age at prison admission (<= 33 (N = 23410) and >33 (N = 21150)). All specifications condition on being in Sweden at month *t*.

Figure 7. In-Prison Medical Data for All Inmates with Sentences of 4-48 Months Entering Prison from 2009-2013

Panel A. Share with any visit or medication

Panel B. Average number of visits or medications
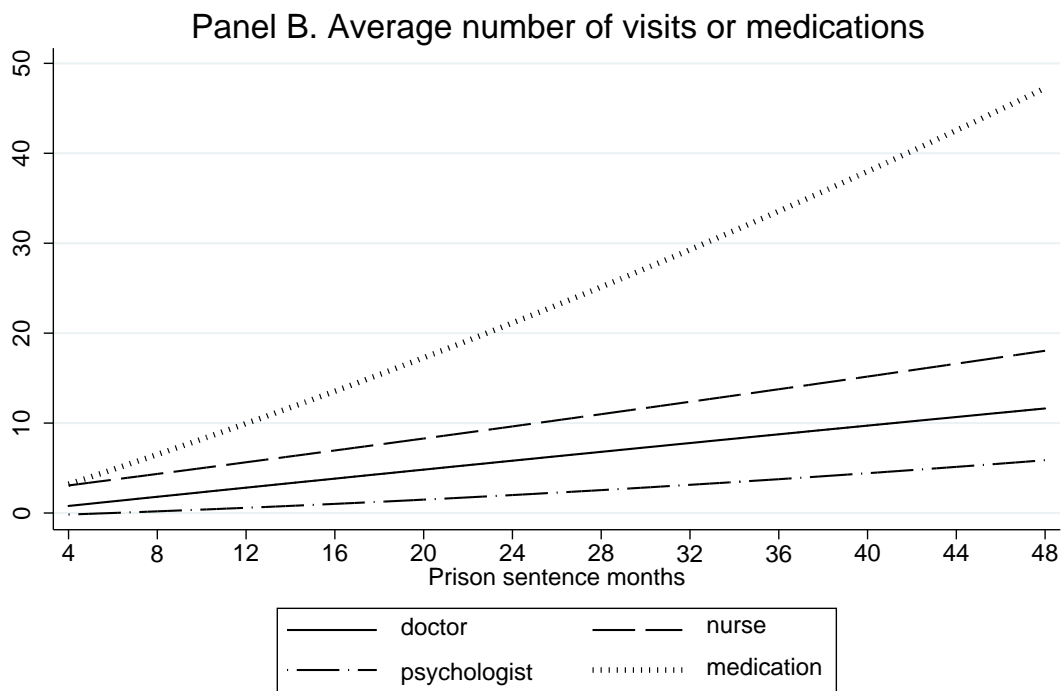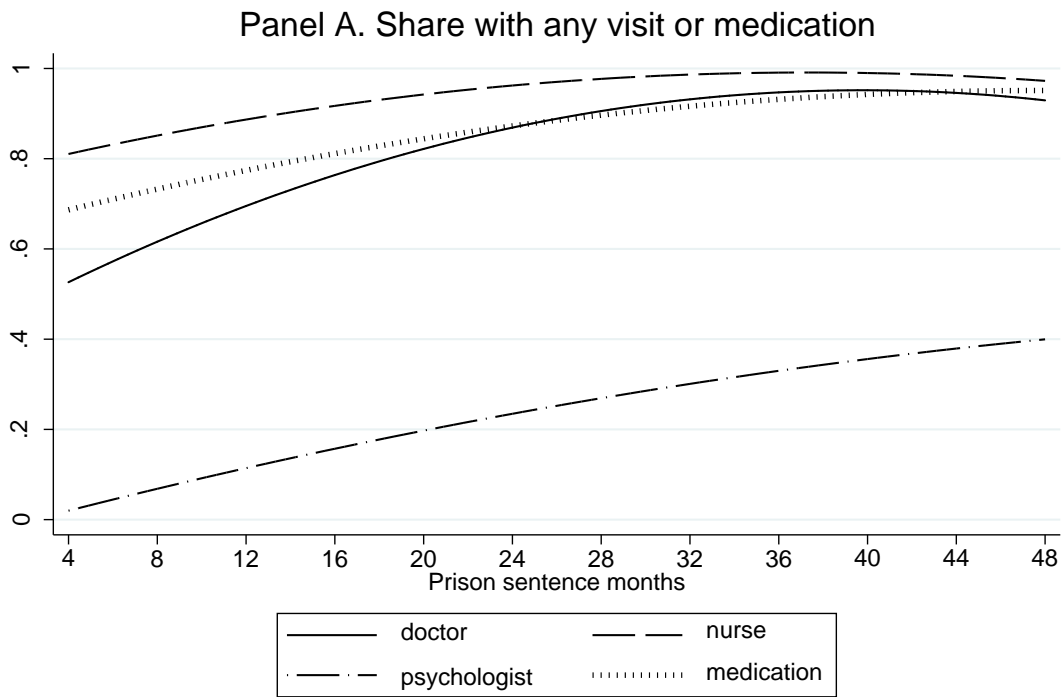
Note – All lines are generated by fitting quadratic trends to the data. Panel A presents the extensive margin for each visit or medication while Panel B presents the intensive margin. In-prison medication is a count of each time the medical staff administers a single medicine to an inmate. These counts can be quite high for those who take medicine on a regular basis, since inmates are not allowed to self-medicate.

Figure 8. In-Prison Treatment Program Participation of Inmates with 4-48 Month Sentences Entering Prison Between 2009 and 2013.

Panel A. Share who started/completed at least one treatment program

Panel B. Average number of treatment programs started/completed
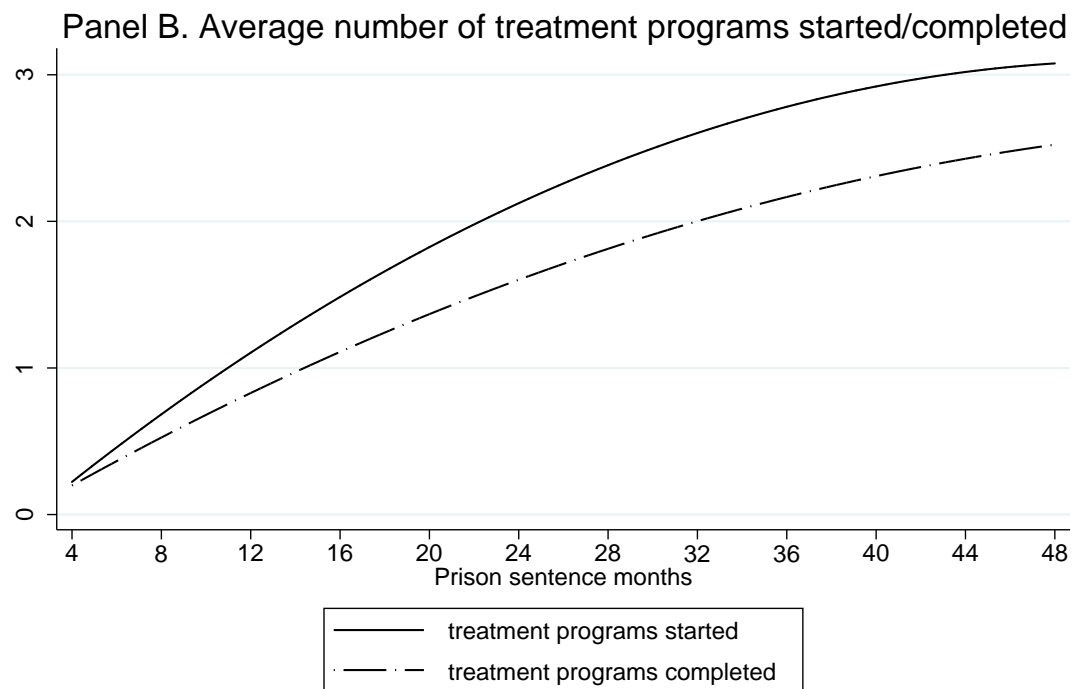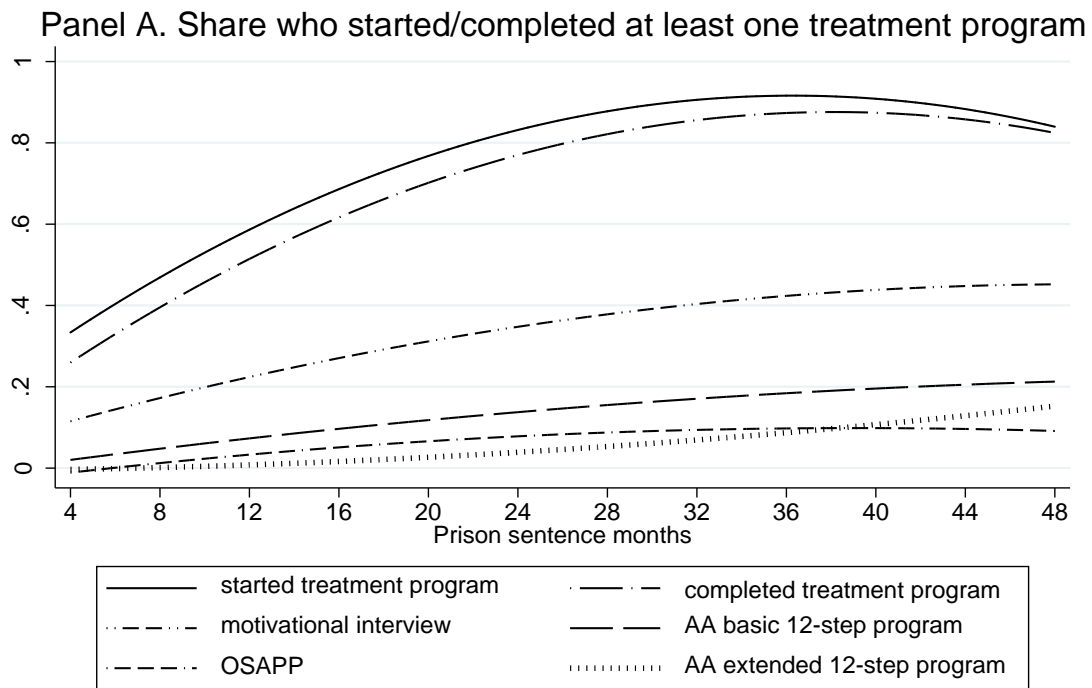
Note – All lines are generated by fitting quadratic trends to the data.

Table 1. Summary Statistics

variable	Main Analysis Sample 4-48 Month Bins N=46815		0-3 Month Bins N=39294	4-12 Month Bins N=33799	13-24 Month Bins N=8968	25-48 Month Bins N=4048
	mean	sd	mean	mean	mean	mean
Prison sentence (months)	11.71	9.15	1.54	7.16	18.09	35.59
Prison sentence (days)	353.92	274.22	48.15	217.49	545.19	1069.38
Treated (1 if fully exposed to 2/3 reform)	0.7	0.44		0.81	0.48	0.3
Early release law (share time served law)	0.62	0.07	0.92	0.64	0.58	0.55
Share time served (actual)	0.52	0.13	0.88	0.53	0.5	0.53
Prison days (days in prison)	184.55	154.38	38.89	114.64	271.68	575.23
Dui	0.03	0.17	0.37	0.04	0.01	0
drugs alcohol	0.15	0.36	0.06	0.13	0.17	0.28
traffic	0.01	0.12	0.06	0.02	0.01	0
property	0.43	0.5	0.2	0.49	0.35	0.18
violent	0.33	0.47	0.24	0.28	0.43	0.52
other	0.04	0.2	0.07	0.04	0.04	0.02
number_crimes_contemporaneous	7.17	7.02	3.25	7.55	6.77	4.91
number_crimes_past	59.51	68.61	29.98	63.3	56.15	35.27
number_prison_past	5.54	6.86	3.03	5.9	5.17	3.29
Any post trial detention	0.72	0.45	0.24	0.68	0.79	0.87
male	0.95	0.22	0.94	0.95	0.95	0.95
Swedish citizen	0.82	0.39	0.84	0.82	0.81	0.77
Born sweden	0.76	0.42	0.78	0.78	0.75	0.68
Age at prison start	33.81	9.57	35.82	33.94	33.28	33.82
primary school_lag1	0.1	0.3	0.12	0.1	0.1	0.1
short high school_lag1	0.41	0.49	0.33	0.41	0.41	0.36
long high school_lag1	0.39	0.49	0.45	0.39	0.38	0.42
Married lag1	0.2	0.4	0.23	0.19	0.21	0.24
Number children at sentence	1.07	1.34	1.18	1.06	1.07	1.18
Number times employed last 3 november	0.58	0.96	1.09	0.55	0.61	0.78
log average income_lag1to3	10.44	1.39	10.95	10.42	10.4	10.63
hospital days alcohol_lag1to3	0.4	3.48	0.42	0.44	0.32	0.21
hospital days narcotics_lag1to3	0.84	7.93	0.51	0.96	0.53	0.46
hospital days psychiatric_lag1to3	5.38	39.26	2.79	5.56	5.34	3.94
hospital days other_lag1to3	2.62	10.76	2.47	2.68	2.52	2.4

Note – Sample observations are listed at the top of the table. All variables are complete (with missing education defined as a separate category) except income, which is missing for about 19% of the main analysis sample. A dummy indicating whether it is missing is included in regression specifications.

Table 2. Relevance: The Effect of the Two-Thirds Sentencing Reform on Days Served

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Days Served in Prison				
	All sample	1 year post release	2 years post release	3 years post release	10 years post release
Panel A: Baseline Specification (Includes Full Set of Controls)					
treated	46.209	46.743	46.742	46.943	46.79
	(4.427)***	(4.483)***	(4.429)***	(4.384)***	(4.376)***
F-stat	109	109	111	115	114
Panel B: Baseline Minus Controls (Just Bin Fixed Effects, Year FE, and Conv Month trend)					
treated	46.517	46.986	47.037	47.213	47.269
	(4.398)***	(4.461)***	(4.410)***	(4.379)***	(4.368)***
F-stat	112	111	114	116	117
Cumulative # died	0	659	1242	1812	5996
Cumulative # emigrated	0	527	879	1183	2185
Observations	46815	45629	44694	43820	38634

Note – Each column includes the sample alive and never emigrated from Sweden X years since date of release. Panel A (the baseline spec) includes controls for 60+ crime type dummies, # current offense, # past crimes, # past prison admissions, age <=21, and age at prison, as well as calendar month dummies of conviction and court dummies, and demographics and socioeconomic and health characteristics at time of incarceration, including: gender, Swedish citizenship, born in Sweden, education attainment categories, marital status, number of children, and (measured in the last three years) employment in November, average income, and number of hospital days in each ward. Panel B drops all observable controls. Standard errors clustered on sentence month bin in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3. The Effect of Exposure to the Two-Thirds Reform on Mortality Overall and by Cause

Dep Variable	Measured t months post-release:			
	12	24	36	120
Death (Any Cause)	-0.0031	-0.0053*	-0.0045	-0.0055
	[0.0021]	[0.0027]	[0.0039]	[0.0060]
Death by Type (ICD Codes):				
Suicide	-0.0017*	-0.0020**	-0.0027**	-0.0038*
	[0.0009]	[0.0010]	[0.0012]	[0.0019]
Violent	-0.0007*	-0.0009	-0.0005	0.0012
	[0.0004]	[0.0007]	[0.0009]	[0.0013]
Circulatory	0.0006	-0.0007	0	-0.0040*
	[0.0007]	[0.0010]	[0.0012]	[0.0020]
Cancer	-0.0001	-0.0006	-0.0002	-0.0026
	[0.0005]	[0.0007]	[0.0007]	[0.0021]
Digestive	0.0002	0.0005	-0.0003	-0.0015
	[0.0004]	[0.0005]	[0.0008]	[0.0020]
Alc/Narc (coroner flag exclusive)	-0.0013	-0.0005	0.0002	0.0043
	[0.0013]	[0.0019]	[0.0024]	[0.0034]
Share dead	0.014	0.027	0.040	0.13
Share suicide	0.0013	0.0022	0.0034	0.010
Share Violent death	0.00069	0.0011	0.0018	0.0050
Share Circulatory Death	0.00097	0.00190	0.00280	0.01500
Share Cancer Death	0.00039	0.00091	0.00170	0.00870
Share Digestive Death	0.00050	0.00110	0.00160	0.00680
Share Alc/Narc Only Death	0.0072	0.0136	0.0200	0.0601
N	46287	45934	45629	44560

Note – This table presents the results of estimating the baseline specification. Specifically, for each mortality outcome listed in column (1) and the number of months post release in the top row, we regress mortality on *treated* (1 indicates full exposure to the two-thirds reform), sentence month bin fixed effects, conviction year fixed effects, a conviction month trend and the full set of observable controls. The coefficient on *treated* is reported. Means of the dependent variables are presented at the bottom of the table. Standard errors clustered on sentence month bin in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4. Robustness Checks of the Effect of the Two-Thirds Reform on Mortality

	Death (Any Cause)				Suicide				Violent Death	Circulatory
	12	24	36	120	12	24	36	120	12	120
Panel A. Baseline										
	-0.0031	-0.0053*	-0.0045	-0.0055	-0.0017*	-0.0020**	-0.0027**	-0.0038*	-0.0007*	-0.0040*
	[0.0021]	[0.0027]	[0.0039]	[0.0060]	[0.0009]	[0.0010]	[0.0012]	[0.0019]	[0.0004]	[0.0020]
Panel B. Excluding all observable controls										
	-0.0036*	-0.0062**	-0.0052	-0.0053	-0.0016*	-0.0018*	-0.0026**	-0.0032*	-0.0007**	-0.0037**
	[0.0021]	[0.0028]	[0.0039]	[0.0060]	[0.0009]	[0.0009]	[0.0012]	[0.0019]	[0.0003]	[0.0018]
Panel C. With Sentence Month Bin x Year Fixed Effects										
	-0.0027	0	-0.0092*	-0.0152*	-0.0035**	-0.0031**	-0.0066***	-0.0043	-0.0005	-0.0024
	[0.0029]	[0.0040]	[0.0047]	[0.0081]	[0.0013]	[0.0015]	[0.0024]	[0.0033]	[0.0007]	[0.0025]
Panel D. With Sentence Month Bin Specific Conviction Month Trend										
	-0.0034	-0.0050*	-0.0041	-0.005	-0.0017*	-0.0020**	-0.0028**	-0.0039*	-0.0008**	-0.0034
	[0.0022]	[0.0028]	[0.0040]	[0.0064]	[0.0009]	[0.0010]	[0.0012]	[0.0020]	[0.0004]	[0.0023]
Panel E. Donut: Dropping 4-month conviction Interval around Each Reform										
	-0.0060**	-0.0088**	-0.0072	-0.0065	-0.0017*	-0.0023**	-0.0029**	-0.0047**	-0.0008*	-0.0043*
	[0.0025]	[0.0033]	[0.0048]	[0.0067]	[0.0010]	[0.0010]	[0.0013]	[0.0021]	[0.0004]	[0.0023]
Panel F. Quadratic Conviction Month Trend										
	-0.003	-0.0051*	-0.0043	-0.0046	-0.0016*	-0.0017*	-0.0025**	-0.0039*	-0.0008*	-0.0039*
	[0.0022]	[0.0028]	[0.0042]	[0.0066]	[0.0010]	[0.0010]	[0.0011]	[0.0020]	[0.0004]	[0.0022]
Panel G. Prison Sentence Day Fixed Effects (instead of month bins)										
	-0.0027	-0.0044*	-0.0035	-0.0044	-0.0017*	-0.0019*	-0.0028**	-0.0036*	-0.0006	-0.0043*
	[0.0020]	[0.0026]	[0.0039]	[0.0066]	[0.0009]	[0.0010]	[0.0012]	[0.0019]	[0.0004]	[0.0023]
Panel H. Baseline with Probit										
	-0.0028	-0.0058**	-0.0055	-0.0083	-0.0010***	-0.0017**	-0.0028***	-0.0032*	-0.0005***	-0.0027***
	[0.0018]	[0.0024]	[0.0035]	[0.0060]	[0.0001]	[0.0008]	[0.0010]	[0.0017]	[0.0001]	[0.0010]

Note – Panel A presents the baseline results, while the remaining panels present a series of robustness tests (as indicated in the panel title). Each panel is independent from one another, i.e. the specifications do not build on each other, but rather alter the baseline. The coefficient on the variable *treated* is presented for each specification. Standard errors clustered on sentence month bin in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5. Heterogeneous Effects of the Two-Thirds Reform on Overall Mortality

		Dep. Variable = Death (Any Cause) at month:			
Sample	N (12 months)	12	24	36	120
Baseline	*46287*	-0.0031	-0.0053*	-0.0045	-0.0055
		[0.0021]	[0.0027]	[0.0039]	[0.0060]

Heteroegeneity By Criminal History and Offense Characteristics

No Past Prison	*7896*	-0.012***	-0.016***	-0.009	-0.01
		[0.004]	[0.005]	[0.007]	[0.011]
One past prison sentence	*8893*	0.001	-0.004	0.003	0.001
		[0.003]	[0.004]	[0.007]	[0.016]
More than 1 Past Prison	*29498*	-0.002	-0.003	-0.007	-0.007
		[0.003]	[0.005]	[0.006]	[0.008]
property offender	*20084*	-0.006*	-0.006	-0.012**	-0.011
		[0.003]	[0.004]	[0.005]	[0.007]
violent offender	*15243*	-0.002	-0.009*	-0.002	-0.003
		[0.004]	[0.004]	[0.006]	[0.009]
Drug/alc offender	*6968*	-0.004	-0.002	-0.004	-0.01
		[0.006]	[0.007]	[0.009]	[0.021]

Heteroegeneity By Demographic Characteristics

< 33 years old	*24417*	-0.006**	-0.009**	-0.009**	-0.001
		[0.002]	[0.004]	[0.004]	[0.008]
>= 33 years old	*21870*	0	0	0.001	-0.012
		[0.004]	[0.005]	[0.007]	[0.012]
Not Born in Sweden	*10699*	-0.003	0	-0.002	0.028*
		[0.004]	[0.005]	[0.007]	[0.014]
Born in Sweden	*35588*	-0.003	-0.007*	-0.005	-0.014*
		[0.003]	[0.003]	[0.004]	[0.008]

Heteroegeneity By Pre-Incarceration SES and Health Characteristics

Unemployed Last 3 years	*31209*	-0.001	-0.003	-0.003	-0.01
		[0.003]	[0.004]	[0.006]	[0.007]
Employed at Least Once	*15078*	-0.007***	-0.009**	-0.007	0.001
		[0.002]	[0.004]	[0.007]	[0.009]
psychiatric hospitalization in the last five years	*9897*	-0.004	-0.011	-0.007	-0.006
		[0.008]	[0.010]	[0.012]	[0.015]
general ward hospitalization last five years	*19176*	-0.004	-0.009	-0.006	-0.009
		[0.004]	[0.006]	[0.009]	[0.012]
no hospitalization in the last five years	*22073*	-0.004*	-0.004	-0.005	-0.004
		[0.002]	[0.004]	[0.004]	[0.007]

Note – This table presents the estimated coefficient for *treated* from the baseline specification for the various subsamples listed in the first column. Results are shown for overall mortality measured 1, 2, 3 and 10 years post-release. Standard errors clustered on sentence month bin in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Heterogeneous Effects By Sentence Month Bin

Dep Variable	Measured t months post-release:							
	12		24		36		120	
Death (Any Cause)								
*treated*bins4-12*	-0.0044*	*-30%*	-0.0053	*-19%*	-0.0053	*-13%*	-0.0041	*-3%*
	[0.0023]		[0.0032]		[0.0043]		[0.0073]	
*treated*bins13-23*	-0.0021	*-14%*	-0.0054	*-19%*	-0.0097	*-24%*	-0.0106	*-8%*
	[0.0035]		[0.0043]		[0.0062]		[0.0115]	
*treated*bins24-48*	-0.0008	*-8%*	-0.0052	*-25%*	-0.0011	*-4%*	-0.0062	*-6%*
	[0.0036]		[0.0037]		[0.0052]		[0.0099]	
Suicide								
*treated*bins4-12*	-0.0020**	*-154%*	-0.0020**	*-87%*	-0.0029**	*-83%*	-0.0043**	*-41%*
	[0.0009]		[0.0010]		[0.0014]		[0.0020]	
*treated*bins13-23*	-0.0022*	*-147%*	-0.0026	*-108%*	-0.0035	*-121%*	-0.0082**	*-80%*
	[0.0012]		[0.0017]		[0.0021]		[0.0031]	
*treated*bins24-48*	-0.0008	*-73%*	-0.0016	*-103%*	-0.0021	*-75%*	-0.0013	*-16%*
	[0.0010]		[0.0011]		[0.0013]		[0.0025]	
Violent Death								
*treated*bins4-12*	-0.0012**	*-167%*	-0.0013	*-131%*	-0.0009	*-56%*	0.0014	*29%*
	[0.0005]		[0.0009]		[0.0009]		[0.0016]	
*treated*bins13-23*	-0.0003	*-43%*	-0.0008	*-53%*	-0.0002	*-9%*	0.0026	*51%*
	[0.0006]		[0.0007]		[0.0011]		[0.0024]	
*treated*bins24-48*	0.0002	*36%*	-0.0003	*-20%*	0	*0%*	0.0003	*6%*
	[0.0004]		[0.0010]		[0.0014]		[0.0022]	
Circulatory Death								
*treated*bins4-12*	0.0007	*83%*	-0.0008	*-47%*	0	*0%*	-0.0035	*-24%*
	[0.0010]		[0.0012]		[0.0013]		[0.0029]	
*treated*bins13-23*	0.0018	*164%*	0.0001	*4%*	0.0005	*16%*	-0.0003	*-2%*
	[0.0019]		[0.0021]		[0.0025]		[0.0043]	
*treated*bins24-48*	0.0001	*6%*	-0.0009	*-35%*	-0.0001	*-3%*	-0.0063**	*-43%*
	[0.0012]		[0.0019]		[0.0022]		[0.0030]	
Alc/Narc (coroner flag exclusive)								
*treated*bins4-12*	-0.0004	*-5%*	0.0013	*9%*	0.0017	*8%*	0.007	*11%*
	[0.0015]		[0.0024]		[0.0031]		[0.0056]	
*treated*bins13-23*	-0.0039*	*-54%*	-0.0041*	*-31%*	-0.0051	*-26%*	-0.0021	*-4%*
	[0.0020]		[0.0024]		[0.0052]		[0.0066]	
*treated*bins24-48*	-0.002	*-48%*	-0.0028	*-34%*	-0.0009	*-7%*	0.0016	*5%*
	[0.0018]		[0.0033]		[0.0036]		[0.0063]	
N	46287		45934		45629		44560	

Note – This table presents the results of expanding the baseline specification by interacting *Treated* with dummy variables for being in the 4-12 month sentence bins, 13-23 month bins and 24 to 48 month bins. Standard errors clustered on sentence month bins are in brackets, and effect sizes relative to dependent variable means for the corresponding sentence month bins are presented in italics to the right of the point estimates. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7. The Effects of Exposure to the Two-Thirds Reform on Recidivism and Labor Market Outcomes

	Dependent variable measured at t		
	12	24	36
Panel A. Dep Var = Any Conviction at Month t			
treated	-0.015*	-0.018	-0.006
	[0.008]	[0.013]	[0.011]
	0.563	*0.700*	*0.758*
Panel B. Dep Var = > 1 Conviction at Month t			
treated	-0.017**	-0.017*	-0.025**
	[0.007]	[0.009]	[0.010]
	0.314	*0.514*	*0.607*
Panel C. Dep Var = Any Prison at Month t			
Treated	-0.029**	-0.020*	-0.018
	[0.012]	[0.011]	[0.012]
	0.391	*0.516*	*0.579*
Panel D. Dep Var = November Employment			
Treated	0.0229***	0.0012	-0.0045
	[0.0075]	[0.0089]	[0.0081]
	0.144	*0.162*	*0.179*
Panel E. Dep Var = earnings			
Treated	1,838.4185*	-432.2496	-1,755.24
	[1,041.6461]	[1,068.7532]	[1,169.7285]
	22192	*26182*	*29810*
Observations	45626	44691	43817

Note – This table presents the effects of full exposure to the two-thirds reform on three measures of recidivism (any conviction, more than 1 conviction, and return to prison) measured at 12, 24, and 36 months post-release in Panels A-C, respecitively. Panels D and E present the results for two labor market outcomes: being employed in the first, second and third Novembers post release and actual earnings. All specifications condition on the sample that is alive and in Sweden at time t. Standard errors, clustered at the prison sentence month bin level, are in brackets. The mean of the dependent variable is in italics. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Figure 1. Swedish Prisons in International Perspective

Panel A: Prison population per 100,000 individuals

Legend: Sweden — — — W. European average — — United States

Panel B: Cost per prisoner in 2015

Panel C: Prison inmate to staff ratio 2015
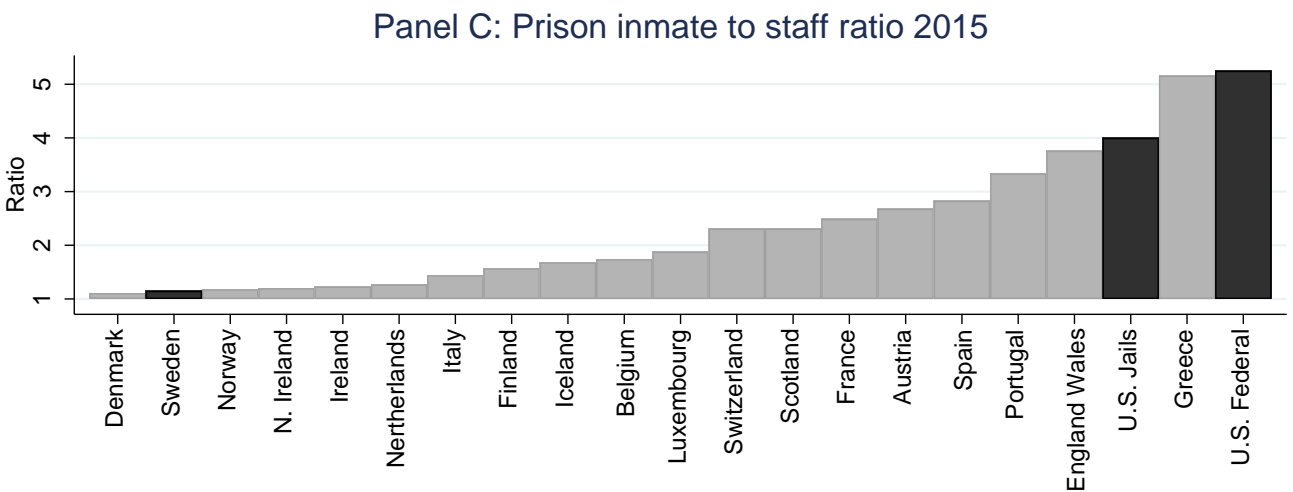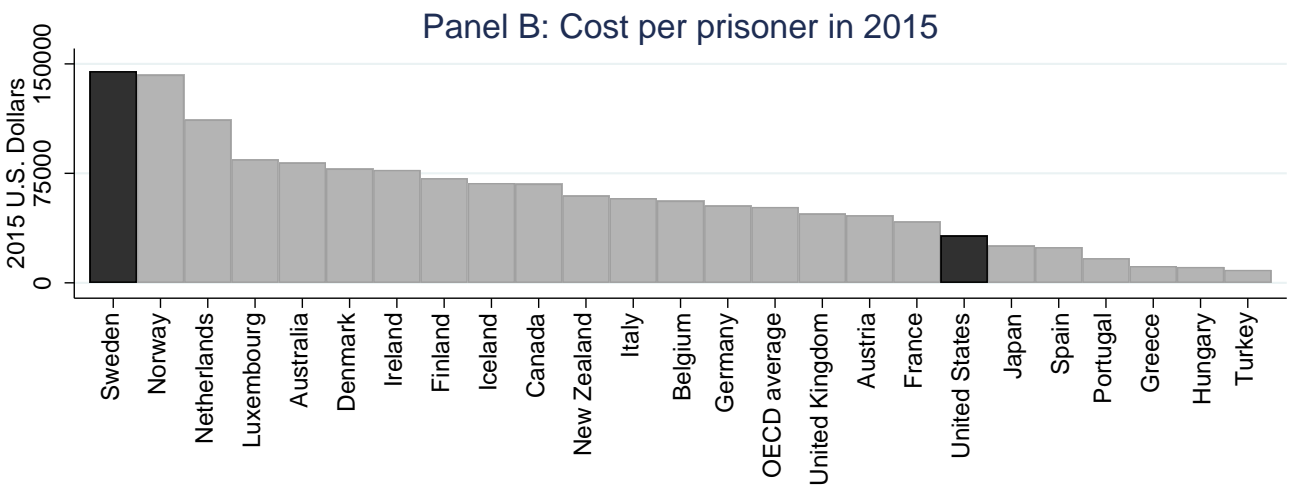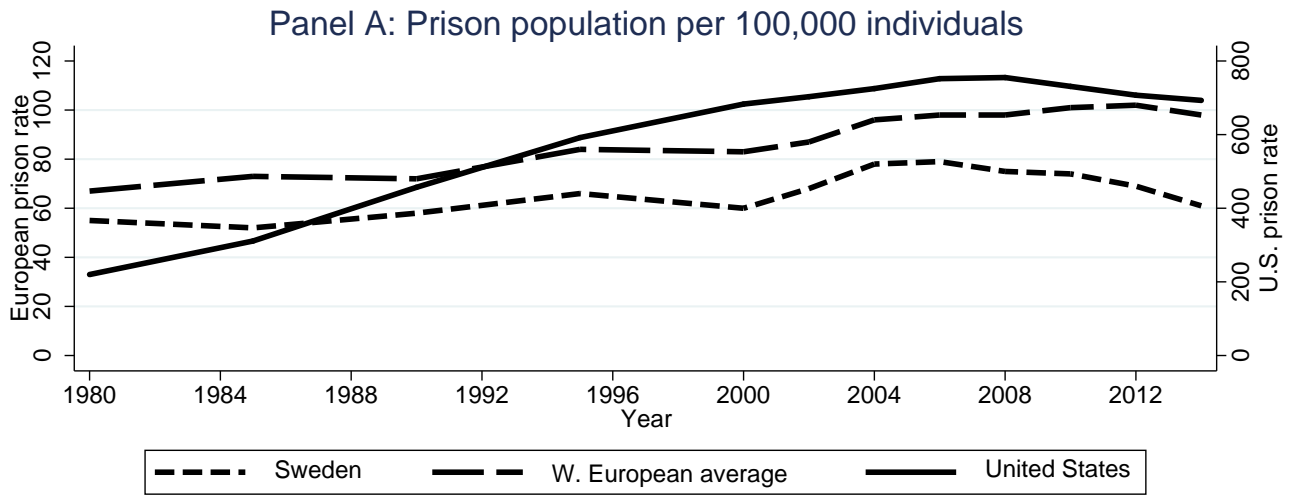
Notes – Panel A: The W. European average is a country average and not a population weighted average. Source: Institute for Criminal Policy Research, U.K. Panel B: Institute for Public Affairs, Australia. Panel C: Source: SPACE-I, Council of Europe, Annual Penal Statistics (2018). Information on U.S. Jails is from the Bureau of Justice Statistics (2018) and refers to the year 2016. Information on U.S. Federal prisons is from the Bureau of Prisons (2012) and refers to the year 2011

Appendix Figure 2. Implementation: No manipulation of prosecutor or judge decisions.

Panel A: Pre and Post July 1993 Reform

Panel B: Pre and Post January 1999 Reform
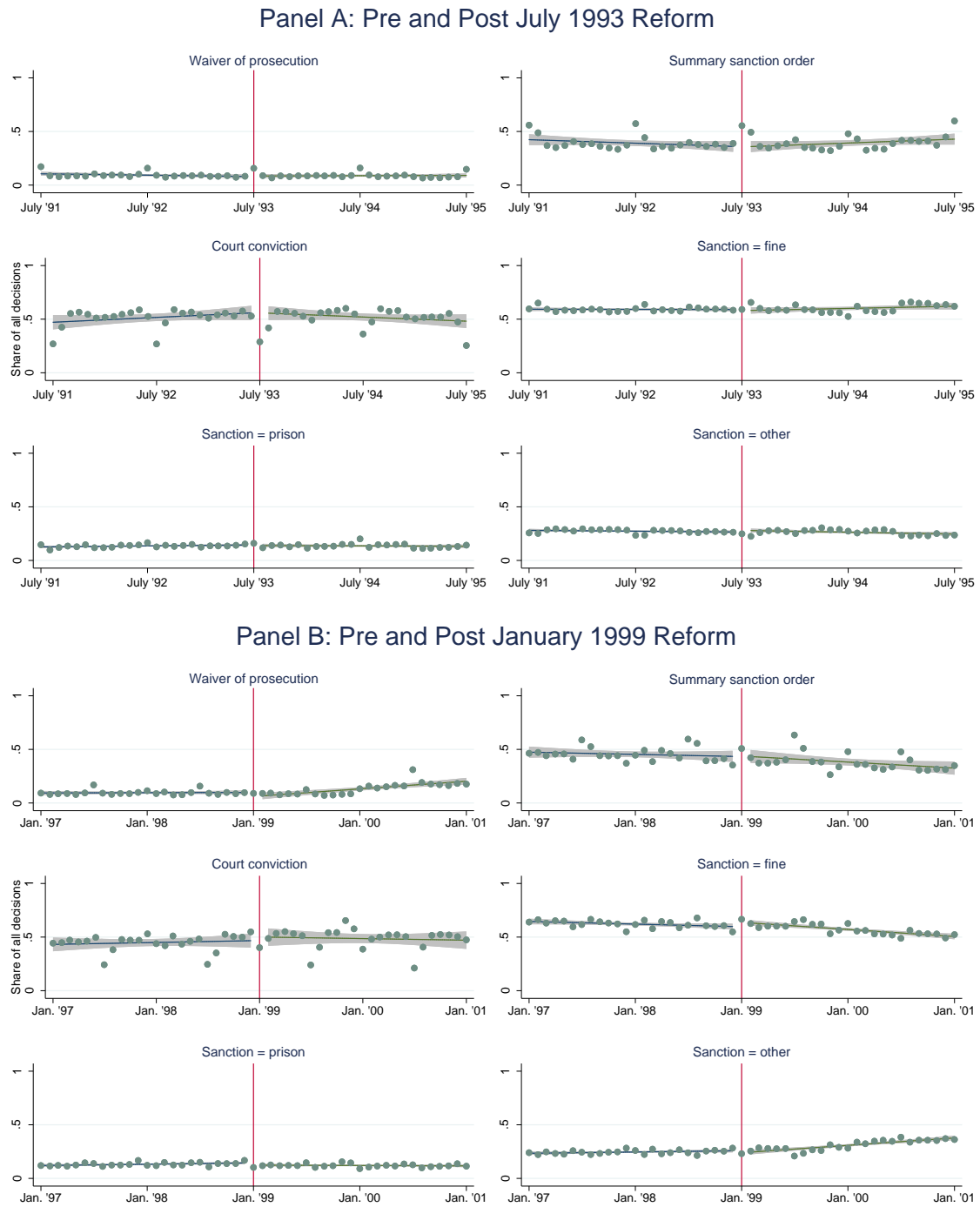
Note – This figure was created using the universe of all decision types and convictions included in the official convictions register for those aged 18 or older in the four years around each reform. We then collapsed these data by the month that each decision or conviction was made. We plot these data and draw linear regression lines (along with 95% confidence intervals) to the left and to the right of the reform month. There are clear seasonal patterns in the raw data, but no meaningful changes in the share of each decision type around the two reforms.

Appendix Figure 3. Dynamics of Mortality Effects for Other Causes of Death

Panel A. Alcohol and/or Narcotics Death (and not other main category)   *Panel B. Cancer death*
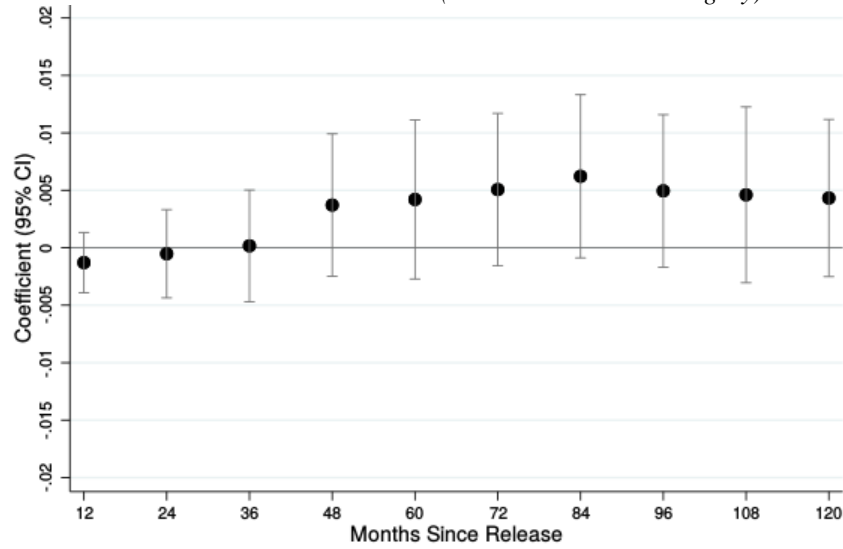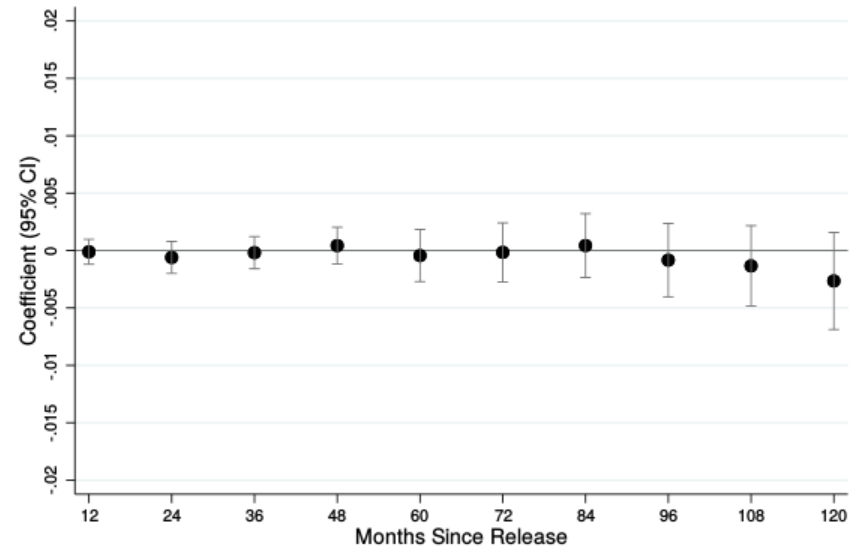
Panel C. Digestive Death
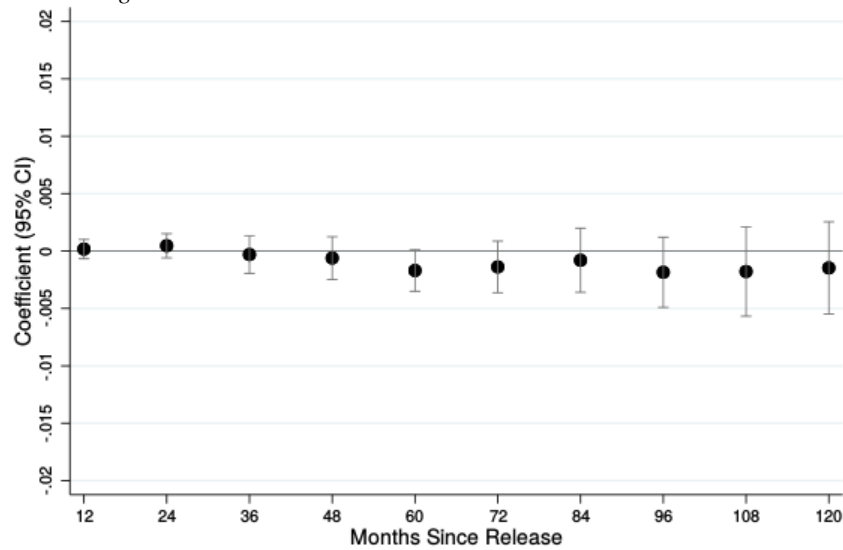
Note – Panels A-C present the estimated coefficient and 95% confidence interval of exposure to the reform (*treated*) on the outcome listed measured at *t* months since release. All specifications condition on not having migrated from Sweden by month *t*. The other causes of death are presented in the main body of the paper.

Appendix Figure 4. Average Annual Stock of Inmates and Average Annual Prison Capacity
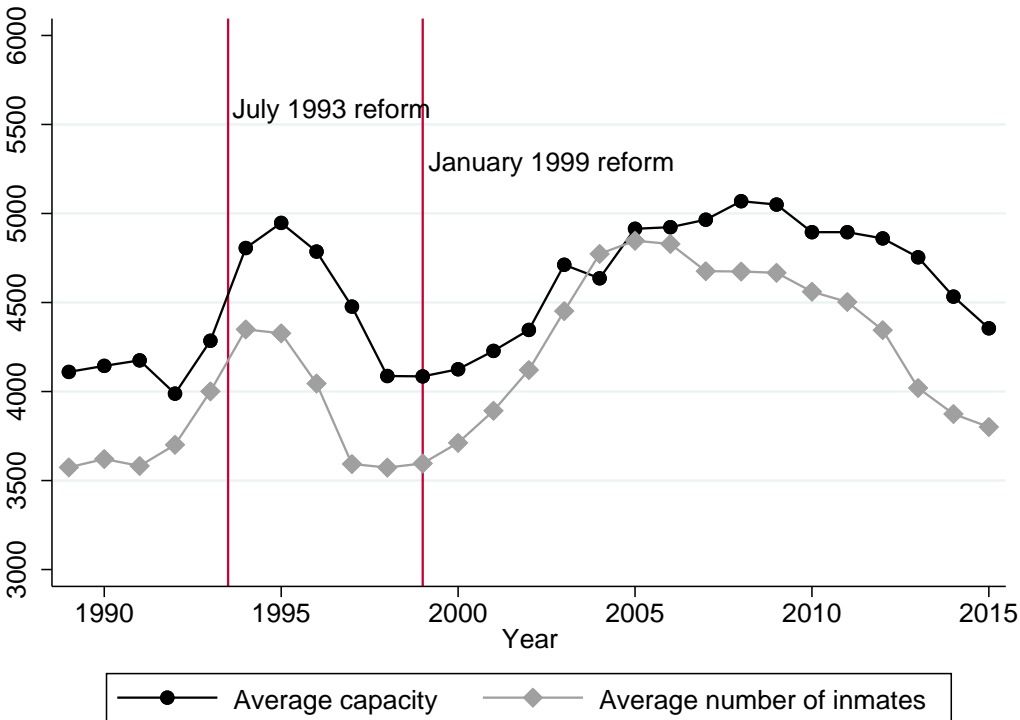
Note – This figure plots the average annual prison capacity against the average annual number of prison inmates. The average occupancy rate for 1989 to 2015 is 90%. Source: The Swedish Prison and Probation Service (2019).

Appendix Figure 5. Monthly Prison Level Descriptive Statistics, 1992-2004.

Panel A. Average number of inmates per prison.
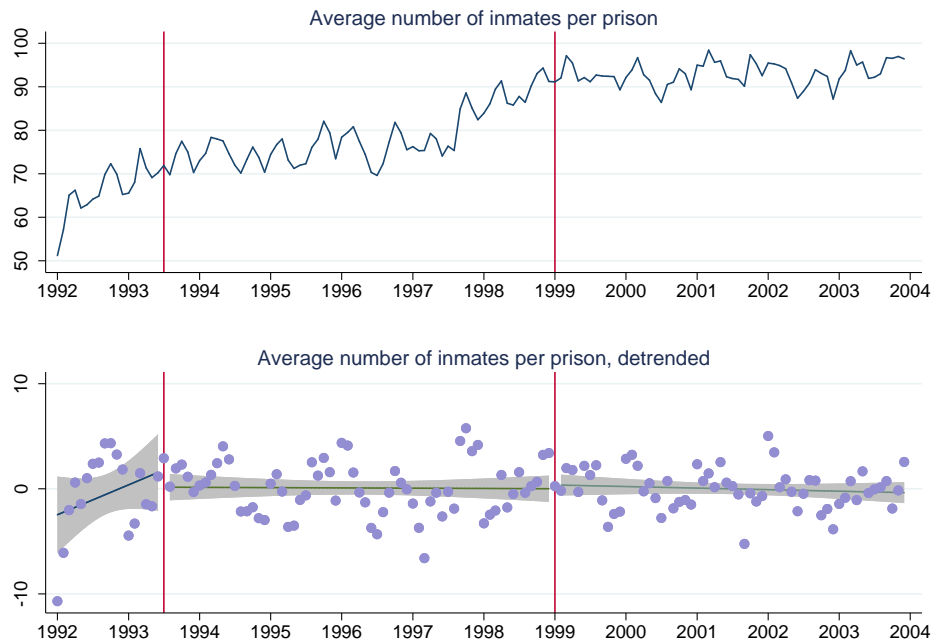
Panel B. Average capacity utilization across prisons measured by the number of inmates in a prison relative to the mean number of inmates ever in that prison.
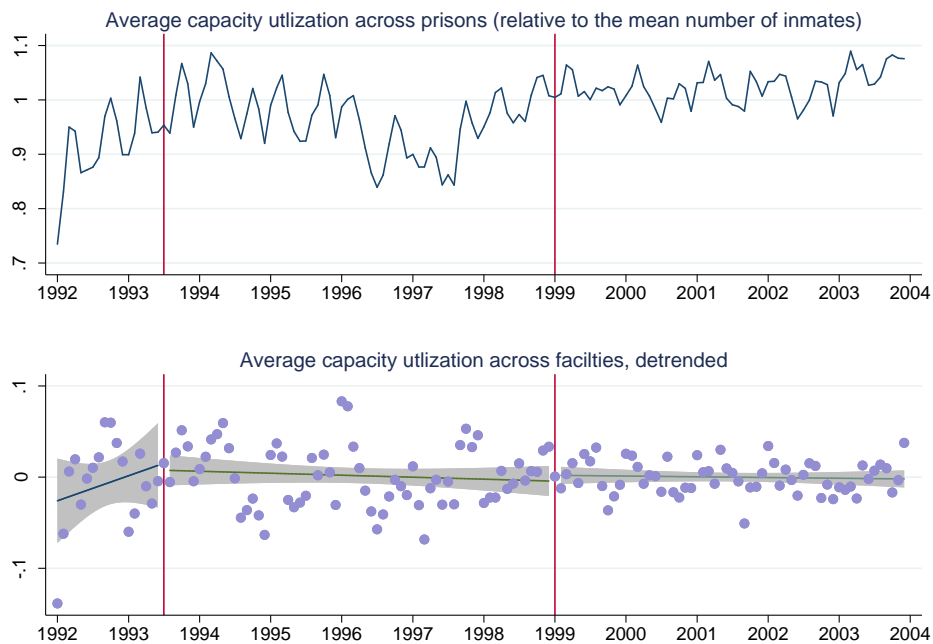
Note – Monthly time series are calculated using information on all inmates (including those not in our estimation sample) and all prisons. Monthly time series are detrended using year fixed effects, month of the year fixed effects (to remove seasonality), and a continuous month trend (just as in our baseline specifications). We plot these data and draw linear regression lines (along with 95% confidence intervals) to the left and to the right of the reform months.

Appendix Figure 5 (continued). Monthly Prison Level Descriptive Statistics, 1992-2004.

Panel C. Average capacity utilization across prisons measured by the number of inmates in a prison relative to the maximum number of inmates ever in that prison.
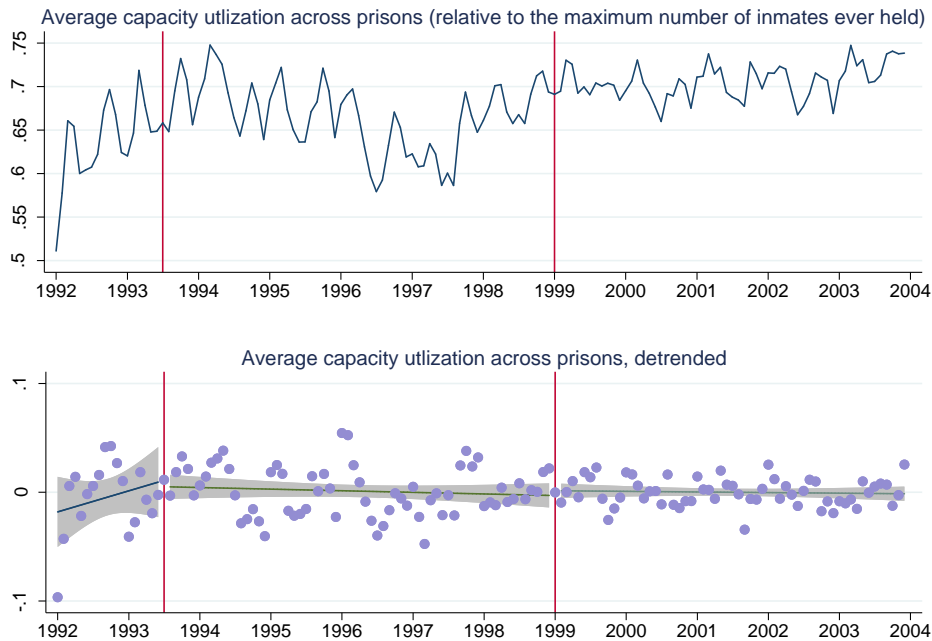
Panel D. Average sentence length of inmates averaged across all prisons.
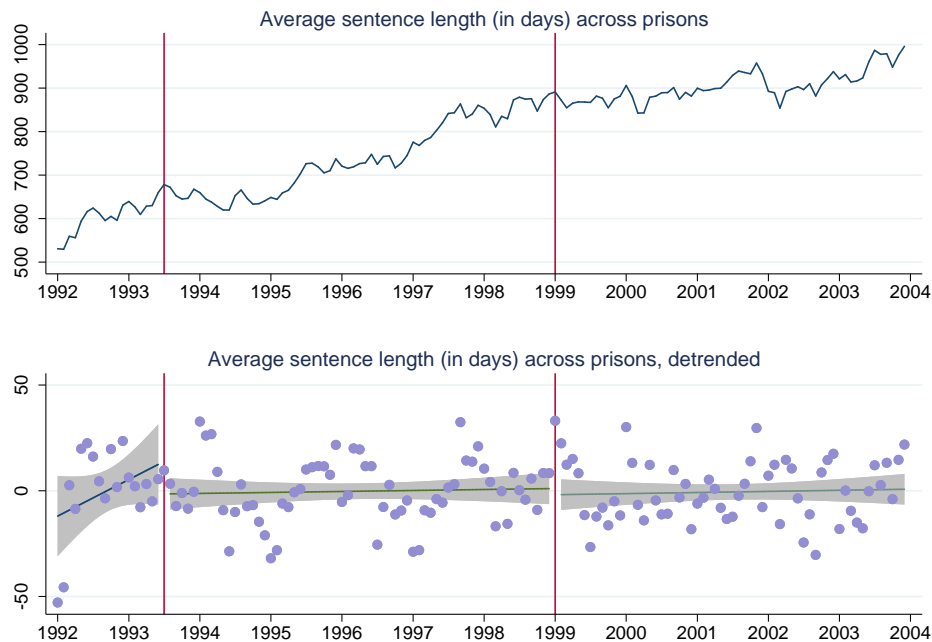
Note – Monthly time series are calculated using information on all inmates (including those not in our estimation sample) and all prisons. Monthly time series are detrended using year fixed effects, month of the year fixed effects (to remove seasonality), and a continuous month trend (just as in our baseline specifications). We plot these data and draw linear regression lines (along with 95% confidence intervals) to the left and to the right of the reform months.

Appendix Table 1. Share Time Served Prescribed By the Law: By Sentence Month Bin and Reform Period.

Sentence Month Bin	Law Pre-1993 Reform	Law Between Reforms	Law Post 1999 Reform	N in each bin
0	1	1	1	2668
1	1	1	1	20088
2	1	1	0.67	9003
3	0.67	0.67	0.67	7535
4	0.5	0.67	0.67	7242
5	0.5	0.67	0.67	3280
6	0.5	0.67	0.67	7017
7	0.5	0.67	0.67	2013
8	0.5	0.67	0.67	4634
9	0.5	0.67	0.67	1622
10	0.5	0.67	0.67	3170
11	0.5	0.67	0.67	873
12	0.5	0.67	0.67	3948
13	0.5	0.64	0.67	593
14	0.5	0.62	0.67	1270
15	0.5	0.60	0.67	911
16	0.5	0.58	0.67	882
17	0.5	0.57	0.67	280
18	0.5	0.55	0.67	1934
19	0.5	0.54	0.67	229
20	0.5	0.53	0.67	577
21	0.5	0.52	0.67	282
22	0.5	0.51	0.67	309
23	0.5	0.51	0.67	83
24	0.5	0.50	0.67	1618
25	0.5	0.5	0.67	100
26	0.5	0.5	0.67	155
27	0.5	0.5	0.67	173
28	0.5	0.5	0.67	148
29	0.5	0.5	0.67	48
30	0.5	0.5	0.67	886
31	0.5	0.5	0.67	51
32	0.5	0.5	0.67	134
33	0.5	0.5	0.67	51
34	0.5	0.5	0.67	73
35	0.5	0.5	0.67	20
36	0.5	0.5	0.67	946
37	0.5	0.5	0.67	41
38	0.5	0.5	0.67	62
39	0.5	0.5	0.67	53
40	0.5	0.5	0.67	45
41	0.5	0.5	0.67	14
42	0.5	0.5	0.67	375
43	0.5	0.5	0.67	15
44	0.5	0.5	0.67	44
45	0.5	0.5	0.67	28
46	0.5	0.5	0.67	26
47	0.5	0.5	0.67	7
48	0.5	0.5	0.67	553

Note –For each sentence bin, the table shows the share of time served prescribed by the law before the July 1, 1993 reform, between the 1993 and 1999 reform, and after January 1999. We also present the number of sentences observed in each sentence bin. The main analysis converts these laws into the treatment variable by considering whether a given 4-48 month sentence was fully exposed to the reform (increase from one-half to two-thirds time).Thus, when the law prescribes 50%, a sentence is untreated (treatment = 0) and when the law prescribes two-thirds, a sentence is fully treated (treatment = 1). Partially treated sentences are assigned the share of full treatment.

Appendix Table 2. Sample Restrictions

Restriction	N (whole sample)	N (sentences >=4 months)
Baseline sample of all prison sentences from 1992-2001	108,439	57,310
Matching kriminalvården and brå conviction data: keep those with a conviction date up to 2 years before the start date	102,762	54,952
Keep: Sentences 48 months or shorter	100,593	52,783
Drop: Start date before age 18	100,405	52,697
Drop: Conviction date before age 18	100,318	52,675
Drop: Offense date before age 18	100,126	52,606
Drop: Start and end sentence in pre-trial detention	96,254	51,037
Drop: Uncertain Treatment (conviction pre reform, start is post)	87,563	47,446
Drop: life sentences	87,557	47,446
Drop: died in prison	87,486	47,385
Drop: sent to foreign prison	87,337	47,237
Trim: Share Time Served > 1.1	86,355	47,008
Trim: Share Time Served < 0.1	86,109	46,815

Note – This shows the sample restrictions in creating the baseline 0-48 month sentence bin sample. Most analyses are conducted on the 4-48 month bins.

Appendix Table 3. Relevance Across Sub-samples

	Coef. on Treated	Standard Error	F	N
Baseline Sample	46.209	(4.427)***	109	46815
# Current Charges				
4 or less curent crimes	46.28	(4.763)***	94	21146
more than 4 current crimes	45.684	(4.544)***	101	25669
Prison History				
no past prison	45.463	(5.807)***	61	8073
1 past prison	56.134	(5.900)***	91	9014
past prison more than 1	43.05	(4.485)***	92	29728
first 4-48 month sentence post 1992	41.729	(5.047)***	68	27165
last 4-48 month sentence pre 2002	53.2	(4.918)***	117	27180
Current Offense Broad Crime Category				
drugs alcohol	53.878	(3.990)***	182	7065
property	42.353	(4.235)***	100	20245
violent	48.06	(5.451)***	78	15484
Demographic And Socioeconomic Characteristics				
not born in sweden	46.629	(4.898)***	91	11069
born in sweden	46.388	(4.644)***	100	35746
younger 33	46.032	(5.151)***	80	22929
older 33	46.291	(4.219)***	120	23886
unemployed last 3 years	52.786	(4.353)***	147	31584
employed at least once in last 3 years	37.897	(5.318)***	51	15231
married	47.322	(4.130)***	131	9150
unmarried	45.801	(4.814)***	91	37665
male	46.286	(4.539)***	104	44499
female	40.845	(3.814)***	115	2316
no children	45.603	(4.933)***	85	21677
children	46.629	(4.225)***	122	25138
Pre Incarceration Hospitalization				
at least one psychiatric hospitalization last 5 years	48.486	(5.089)***	91	9995
general ward hospital hospitalization last 5 years	46.326	(4.237)***	120	19348
no hospitalization last five years	45.267	(5.099)***	79	22375
Reform SubSamples				
+/- 2 years of 1993 reform	22.924	(6.404)***	13	19130
+/- 2 years of 1999reform	73.316	(6.774)***	117	16930

Note – Each row presents estimates for different subsample of regressing prison days served on whether the individual's sentence is treated by the two-thirds reform law. The baseline specification, with the full set of controls, is used. Fstatistics on the instrument (share of time served law) are reported in the third columns. Standard errors clustered on sentence month bin.

Appendix Table 4. Do Observables Change with Exposure to the Reform?

Row	Variable	Coefficient	SE	Mean D.V.	R-squared
(1)	age_at_prison	-0.252	[0.227]	33.81	0.01
(2)	age_less_21?	0.003	[0.007]	0.081	0.01
(3)	number_crimes_past	-2.598*	[1.482]	59.51	0.07
(4)	number_prison_past	-0.235*	[0.138]	5.54	0.06
(5)	number_crimes_contemporaneous	-0.810***	[0.185]	7.17	0.07
(6)	property (current offense)?	-0.026	[0.016]	0.43	0.06
(7)	violent (current offense)?	0.015	[0.011]	0.33	0.05
(8)	drug_alc (current offense)?	0.003	[0.011]	0.15	0.02
(9)	hospital_alcohol_days_lag1to3	-0.106	[0.086]	0.4	0
(10)	hospital_narcotics_days_lag1to3	-0.182	[0.158]	0.84	0
(11)	hospital_psych_days_lag1to3	-0.387	[0.907]	5.38	0
(12)	hospital_other_days_lag1to3	-0.161	[0.313]	2.62	0
(13)	male?	0.003	[0.005]	0.95	0
(14)	swedish_citizen?	0.003	[0.010]	0.82	0
(15)	born_sweden?	0.005	[0.012]	0.76	0.01
(16)	education_lag1	-0.011	[0.038]	2.72	0.01
(17)	married_lag1?	0.005	[0.010]	0.2	0.01
(18)	number_children_at_sentence	0.047	[0.033]	1.07	0.01
(19)	number times employed in last 3 novembers	0.029	[0.022]	0.58	0.08
(20)	income_average_last 3 years	0.046	[0.035]	10.48	0.02

Note – This table presents the results of regressing each observable characteristic on: *treated* (exposure to the reform), prison sentence month bin fixed effects, conviction year fixed effects, and a conviction month trend. In other words, this is the baseline specification, without the controls. The education variable is 1-7 here, corresponding to different education categories (these are included as dummies in the baseline specification). Clustered on prison sentence month bin standard errors in brackets, *** 1%, **5%, *10%. N = 46,815 in all specifications

Appendix Table 5. Robustness of Mortality Results to Date to Measure At-Risk and Inclusion/Exclusion of the Straddle Sample

	Death (Any Cause)				Suicide				Violent Death				Circulatory			
	12	24	36	120	12	24	36	120	12	24	36	120	12	24	36	120
Panel A. Baseline (measures post release mortality *m* months from the release date, excludes straddle sample)																
	-0.0031	-0.0053*	-0.0045	-0.0055	-0.0017*	-0.0020**	-0.0027**	-0.0038*	-0.0007*	-0.0009	-0.0005	0.0012	0.0006	-0.0007	0	-0.0040*
	[0.0021]	[0.0027]	[0.0039]	[0.0060]	[0.0009]	[0.0010]	[0.0012]	[0.0019]	[0.0004]	[0.0007]	[0.0009]	[0.0013]	[0.0007]	[0.0010]	[0.0012]	[0.0020]
	46287	45934	45629	44560	46287	45934	45629	44560	46287	45934	45629	44560	46287	45934	45629	44560
Panel B. measures mortality *m* months from the start date of sentence (includes those who died in prison), excludes straddle sample																
	-0.0026	-0.0066***	-0.0074**	-0.0074	-0.0005	-0.0012	-0.0021**	-0.0042**	-0.0004	-0.0013*	-0.0016**	0.0007	0.0004	-0.0001	0.0002	-0.0044*
	[0.0016]	[0.0023]	[0.0031]	[0.0061]	[0.0004]	[0.0009]	[0.0010]	[0.0019]	[0.0003]	[0.0007]	[0.0008]	[0.0013]	[0.0006]	[0.0008]	[0.0011]	[0.0023]
	46616	46192	45840	44641	46616	46192	45840	44641	46616	46192	45840	44641	46616	46192	45840	44641
Panel C. measures mortality *m* months from the start date of sentence (includes those who died in prison), includes straddle sample																
	-0.002	-0.0059**	-0.0060*	-0.003	-0.0004	-0.001	-0.0018*	-0.0036*	-0.0003	-0.0012**	-0.0015**	0.0009	0.0003	-0.0001	0.0001	-0.0042*
	[0.0016]	[0.0026]	[0.0030]	[0.0051]	[0.0004]	[0.0008]	[0.0009]	[0.0018]	[0.0003]	[0.0006]	[0.0007]	[0.0012]	[0.0004]	[0.0008]	[0.0010]	[0.0021]
	50139	49685	49304	48008	50139	49685	49304	48008	50139	49685	49304	48008	50139	49685	49304	48008
Panel D. measures post release mortality *m* months from the release date, includes straddle sample																
	-0.003	-0.0047*	-0.0027	-0.0011	-0.0014*	-0.0017*	-0.0022**	-0.0031*	-0.0007**	-0.0009	-0.0005	0.0013	0.0007	-0.0007	-0.0001	-0.0034*
	[0.0021]	[0.0027]	[0.0036]	[0.0053]	[0.0008]	[0.0008]	[0.0010]	[0.0018]	[0.0004]	[0.0006]	[0.0009]	[0.0011]	[0.0006]	[0.0009]	[0.0011]	[0.0020]
	49792	49410	49080	47926	49792	49410	49080	47926	49792	49410	49080	47926	49792	49410	49080	47926

Note – Panel A presents the baseline specification in which the outcome is measured t months after the date of prison release and the straddle sample (those convicted before a reform but start sentence after) is excluded. This table considers the sensitivity of the results to these choices. Specifically, Panel B and C consider one to be at-risk as of the start date of the prison sentence (thereby including those who died in prison) while Panels C and D include the straddle sample individuals in the analysis. Clustered on prison sentence month bin standard errors in brackets, *** 1%, **5%, *10%.

Appendix Table 6. Heterogeneity for High-Risk Subsamples

	Death (Any Cause)				Suicide				Violent Death	circulatory	Alc/Narc Related (exclusive)			
	12	24	36	120	12	24	36	120	12	120	12	24	36	120
Baseline (N 12 months = 46287)														
	-0.0031	-0.0053*	-0.0045	-0.0055	-0.0017*	-0.0020**	-0.0027**	-0.0038*	-0.0007*	-0.0040*	-0.0013	-0.0005	0.0002	0.0043
	[0.0021]	[0.0027]	[0.0039]	[0.0060]	[0.0009]	[0.0010]	[0.0012]	[0.0019]	[0.0004]	[0.0020]	[0.0013]	[0.0019]	[0.0024]	[0.0034]
< 33 years old (N 12 months = 24417)														
	-0.006**	-0.009**	-0.009**	-0.001	-0.001	-0.001	-0.001	-0.002	-0.001	0.001	-0.002	-0.001	0	0.002
	[0.002]	[0.004]	[0.004]	[0.008]	[0.001]	[0.001]	[0.002]	[0.003]	[0.000]	[0.002]	[0.002]	[0.003]	[0.003]	[0.005]
>= 33 years old (N 12 months = 21870)														
	0	0	0.001	-0.012	-0.003**	-0.003*	-0.005**	-0.005*	-0.001	-0.011**	0	0.001	0.002	0.007
	[0.004]	[0.005]	[0.007]	[0.012]	[0.001]	[0.002]	[0.002]	[0.003]	[0.001]	[0.005]	[0.002]	[0.003]	[0.004]	[0.006]
property offender (N 12 months = 20084)														
	-0.006*	-0.006	-0.012**	-0.011	-0.001	0	-0.001	-0.001	0	-0.002	-0.001	0	-0.002	0
	[0.003]	[0.004]	[0.005]	[0.007]	[0.001]	[0.001]	[0.001]	[0.002]	[0.001]	[0.004]	[0.002]	[0.003]	[0.004]	[0.005]
violent offender (N 12 months = 15243)														
	-0.002	-0.009*	-0.002	-0.003	-0.002	-0.003	-0.004**	-0.006*	-0.002**	-0.008*	0.002	0.003	0.008*	0.019***
	[0.004]	[0.004]	[0.006]	[0.009]	[0.002]	[0.002]	[0.002]	[0.003]	[0.001]	[0.004]	[0.002]	[0.003]	[0.004]	[0.007]
drug and alcohol offender (N 12 months = 6968))														
	-0.004	-0.002	-0.004	-0.01	-0.002	-0.002	-0.004	-0.007*	0	0.001	-0.008**	-0.008	-0.012**	-0.016
	[0.006]	[0.007]	[0.009]	[0.021]	[0.002]	[0.002]	[0.003]	[0.004]	[0.000]	[0.007]	[0.004]	[0.006]	[0.006]	[0.011]
psychiatric hospitalization in the last five years (N 12 months =9897)														
	-0.004	-0.011	-0.007	-0.006	-0.006*	-0.009**	-0.007*	-0.015***	-0.002*	-0.008	0.004	0.006	0.007	0.021**
	[0.008]	[0.010]	[0.012]	[0.015]	[0.003]	[0.003]	[0.004]	[0.005]	[0.001]	[0.007]	[0.005]	[0.009]	[0.010]	[0.010]
general ward hospitalization in the last five years (N 12 months = 19176)														
	-0.004	-0.009	-0.006	-0.009	-0.002	-0.003	-0.003	-0.007**	-0.001	-0.009**	0	0.001	0.004	0.01
	[0.004]	[0.006]	[0.009]	[0.012]	[0.002]	[0.002]	[0.002]	[0.003]	[0.001]	[0.004]	[0.002]	[0.004]	[0.005]	[0.007]
no hospitalization in the last five years (N 12 months = 22073)														
	-0.004*	-0.004	-0.005	-0.004	-0.001	-0.001	-0.002	-0.001	-0.001	0.001	-0.004***	-0.004*	-0.005*	-0.003
	[0.002]	[0.004]	[0.004]	[0.007]	[0.001]	[0.001]	[0.002]	[0.003]	[0.001]	[0.002]	[0.001]	[0.002]	[0.002]	[0.005]

Note – This table presents the baseline cause of death results for each high-risk subsample. Clustered on prison sentence month bin standard errors in brackets, *** 1%, **5%, *10%.

Appendix Table 7. Heterogenity in Recidivism and Labor Market Results

	Dep. Var. = Any Prison at month:			Dep. Var. = Employed in November (near month)	
12	24	36	12	24	36
Baseline (N 12 months = 46287)					
-0.029**	-0.020*	-0.018	0.023***	0.001	-0.004
[0.012]	[0.011]	[0.012]	[0.008]	[0.009]	[0.008]
Heteroegeneity By Criminal History and Offense Characteristics					
No Prison (N 12 months = 7896)					
-0.009	0.012	0.007	0.016	0.011	0.002
[0.014]	[0.015]	[0.022]	[0.013]	[0.017]	[0.016]
One past prison sentence (N 12 months = 8893)					
-0.027*	-0.031**	-0.033*	0.033*	0.013	0.001
[0.015]	[0.012]	[0.017]	[0.019]	[0.026]	[0.025]
More than 1 Past Prison (N 12 months = 29498)					
-0.039**	-0.028*	-0.02	0.019*	-0.006	-0.008
[0.017]	[0.015]	[0.013]	[0.010]	[0.009]	[0.011]
property offender (N 12 months = 20084)					
-0.057***	-0.055***	-0.046***	0.021**	0.004	-0.011
[0.015]	[0.013]	[0.011]	[0.010]	[0.012]	[0.014]
violent offender (N 12 months = 15243)					
-0.015	-0.001	0.001	0.029***	0.008	0.01
[0.012]	[0.015]	[0.016]	[0.011]	[0.009]	[0.012]
Heteroegeneity By Demographic Characteristics					
< 33 years old (N 12 months = 24417)					
-0.02	-0.01	-0.014	0.014	-0.006	-0.008
[0.013]	[0.013]	[0.014]	[0.009]	[0.010]	[0.010]
>= 33 years old (N 12 months = 21870)					
-0.040***	-0.035**	-0.025	0.032***	0.009	0.001
[0.015]	[0.015]	[0.017]	[0.010]	[0.012]	[0.014]
Not Born in Sweden (N 12 months = 10699)					
-0.049***	-0.019	-0.025*	0.026**	-0.015	-0.005
[0.016]	[0.012]	[0.014]	[0.012]	[0.018]	[0.016]
Born in Sweden (N 12 months = 35588)					
-0.022*	-0.021	-0.016	0.022**	0.006	-0.006
[0.013]	[0.013]	[0.013]	[0.009]	[0.009]	[0.010]
Heteroegeneity By Pre-Incarceration SES and Health Characteristics					
Unemployed Last 3 years (N 12 months = 31209)					
-0.039***	-0.030**	-0.033*	0.015**	-0.001	0.007
[0.014]	[0.012]	[0.018]	[0.007]	[0.012]	[0.010]
Employed at Least Once (N 12 months = 15078)					
-0.012	-0.004	0.01	0.039***	0.007	-0.022
[0.016]	[0.018]	[0.014]	[0.011]	[0.017]	[0.013]
psychiatric hospitalization in the last five years (N 12 months =9897)					
-0.035	-0.021	-0.027	-0.002	0.006	-0.021
[0.026]	[0.022]	[0.025]	[0.013]	[0.015]	[0.018]
general ward hospitalization in the last five years (N 12 months = 19176)					
-0.027*	-0.01	-0.02	0.032***	0.01	-0.008
[0.016]	[0.018]	[0.017]	[0.009]	[0.010]	[0.012]
no hospitalization in the last five years (N 12 months = 22073)					
-0.016	-0.015	-0.009	0.018	-0.005	0
[0.012]	[0.012]	[0.012]	[0.012]	[0.012]	[0.010]

Note – This table presents the results of estimating the baseline specification for one recidivism outcome (prison) and one labor market outcome (employed in November) for various subsamples. Results on *treated* are presented for each specification.