The Health Effects of Prison[†]

By Randi Hjalmarsson and Matthew J. Lindquist*

This paper studies the health effects of Swedish prison reforms that held sentences constant but increased the share of time inmates had to serve. The increased time served did not harm post-release health and actually reduced mortality risk. We find especially large decreases in mortality for offenders not previously incarcerated, younger offenders, and those more attached to the labor market. Risk of suicide and circulatory death fell for inmates with mental health problems and older inmates, respectively. In-prison health care utilization and program participation increased with time served, suggesting health care treatment and services as the key mechanism for mortality declines. (*JEL* I12, I18, K42)

Driven by more and longer prison sentences, US incarceration rates have increased by more than 500 percent since 1980.[1] The unequal use of incarceration across society, especially by race, and the potentially negative consequences of prison on post-release crime, labor market, and familial outcomes has culminated in increased calls for justice reforms that de-emphasize the use of prison as a sanction.[2] At the same time, there is increasing evidence that—at least in some contexts and for some individuals—prison can improve post-release recidivism and labor market outcomes.[3] Thus, the answer to the question of more or less prison may not be a simple yes or no. Whether less prison is optimal may depend on what is in the black box of prison (e.g., prison conditions, treatment programs, or peers) and may

---

[1] See https://www.sentencingproject.org/criminal-justice-facts/ (last accessed March 8, 2021).

[2] For instance, at the extensive margin, Aizer and Doyle (2015) and Di Tella and Schargrodsky (2013) find harmful effects of prison in Chicago and Argentina, respectively. At the intensive margin, researchers find more time in prison yields worse recidivism and labor market outcomes in Texas (Mueller-Smith 2015).

[3] Using varying sources of identification, researchers have found that, at the intensive margin, more time in prison reduces the chance of recidivism in Georgia (Kuziemko 2013), Seattle (Roach and Schanzenbach 2015), and North Carolina (Rose and Shem-tov 2021) and reduces unemployment and increases earnings in Denmark (Landersø 2015). At the extensive margin, Bhuller et al. (2020) find beneficial effects in Norway, and Dobbie et al. (2018) find little effect of Swedish incarceration on recidivism (though the paper emphasizes child outcomes).

vary across subsamples of the population. This paper contributes to this debate by estimating the effect of more time in Swedish prisons on mortality, an important post-release outcome about which there is little causal evidence. Prison conditions in Sweden are among the best in the world, allowing us to answer whether spending more time in prison could potentially improve prisoner health.[4]

We take advantage of Sweden's 1993 and 1999 early release reforms, which held sentences constant but increased the share of time inmates were required to serve from 50 percent to 67 percent. 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. Moreover, we assess whether more time in prison differentially affects particularly vulnerable or high-risk subsamples, including those with pre-incarceration mental health problems. Such individuals are disproportionately represented in prisons worldwide: 50 percent of US prisoners have a known history of mental health problems, while 60 percent are classified as drug dependent or substance abusers (Bronson and Berzofsky 2017; Bronson et al. 2017).[5]

Despite the high costs of prison health care—more than $8 billion or about 20 percent of 2015 US prison expenditures (Pew Charitable Trusts 2017)—little is known about the individual and social returns to these expenditures. This knowledge gap is especially significant in light of growing evidence that health care access (especially for mental health care) outside of prison causally reduces crime (Jácome 2020).[6] Our paper contributes to this literature by studying the effect of extending exposure to high-quality prison conditions that include access to high-quality health care, mental health professionals, and a multitude of treatment programs.

More generally, 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 by the COVID-19 crisis), exposure to a violent and stressful environment, poor nutrition, and poor access to proper health care. 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.

---

[4] Prison health papers that do not study the effect of prison sentences on post-release health include 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; Campaniello, Diasakos, and Mastrobuoni (2017), who find that Italian collective pardons decreased prisoner suicide rates; Raphael and Stoll (2013), who find that the US deinstitutionalization of the mentally ill accounts for 4–7 percent of incarceration growth from 1980 to 2000; and Boylan and Mocan (2014), who find lower inmate mortality rates after court orders condemning state prison overcrowding.

[5] More than 60 percent of UK prisoners suffer from personality disorders and 50 percent from depression or anxiety (Burki 2017). More than 50 percent of Swedish prisoners had been previously diagnosed with a psychiatric disorder, most commonly a substance abuse disorder (Haglund et al. 2014).

[6] See Doleac (2018) for a popular science review. Bondurant, Lindo, and Swenson (2018) find that expanding access to substance abuse treatment facilities reduces local violent and financially motivated crimes. Using various expansions to Medicaid coverage, Wen, Hockenberry, and Cummings (2017); Vogler (2017); and Aslim et al. (2019) all find evidence of decreases in crime (or recidivism) behavior; some argue that the results are driven by increased access to substance abuse treatment.

Health screening upon intake can 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 affected directly 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.

There is ample evidence that incarceration is correlated with worse health outcomes and behaviors, including problems that prevent work (Schnittker and John 2007), depression (Turney, Wildeman, and Schnittker 2012), fast food consumption and smoking (Porter 2014), stress-related illnesses and infectious diseases (Massoglia 2008), and higher mortality due to both natural and unnatural causes (Sailas et al. 2005; Haglund et al. 2014; Binswanger et al. 2007; Skardhamar and Skirbekk 2013; and Jones et al. 2017).[7] Mortality risks are even larger with more time in prison: a study of New York state parolees finds that each additional prison year translates into a two-year decline in life expectancy (Patterson 2013). But just one contemporaneous paper attempts to estimate the causal effect of prison on mortality. Using administrative data from Ohio, Norris, Pecenco, and Weaver (2020) find a large protective (incapacitative) effect of incarceration: convicted felons not sent to prison have higher mortality rates, mainly from homicides and overdoses, than those who were incarcerated. Importantly, Norris, Pacenco, and Weaver (2020) can rule out the existence of large, detrimental post-release health effects.

Our analysis sample consists of nearly 47,000 prison sentences of 4 to 48 months. These prisoners are as negatively selected in terms of their health as prisoners around the world: more than 20 percent were admitted to a psychiatric ward in the five years prior to prison. The early release reforms provide multiple sources of exogenous variation in the amount of time served in prison: there are varying doses of the treatment affecting different sentence lengths across two different time periods. We combine these multiple sources of variation into a single treatment variable. Specifically, the baseline specification estimates the reduced-form 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 with the same sentence but different time served due to the timing of reform exposure (which varies across bins). Before presenting our results, we demonstrate that reform exposure resulted, on average, in a 46-day

---

[7] See the Fazel and Baillargeon (2011) review. Piquero et al. (2014) overview the offender mortality literature.

increase in time served and that conditional independence is satisfied, as a large set of observable controls are balanced around the reform.

In stark contrast to previous correlational studies, 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 years post release) are seen for positively selected subsamples, 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; the chance of suicide is still reduced by 38 percent ten years after release. These suicide results are driven by individuals with previously identified mental health issues and by 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-run 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 post release.

Why did exposure to the Swedish early release reforms improve general and mental health? We first provide qualitative and empirical evidence that there were no other changes to the prison experience (e.g., overcrowding, facility assignment, peers) beside the increase in days served that could drive our reduced-form effects. We consider two channels through which more time in prison could improve post-release health: the direct effect of in-prison health care and treatment and an indirect improved lifestyle effect post release.

With respect to the former, an out-of-sample analysis of the health care utilization of inmates sentenced from 2009 to 2013 (for whom we have detailed prison health care data) yields two important conclusions. First, health care in Swedish prisons is of high quality. Second, more time in prison is positively related to visits with medical professionals (doctors, nurses, and psychologists), medication, and starting and completing treatment programs. High-quality health care and treatment 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 takeaway in and of itself is that increased exposure to Swedish prisons generally does not lead to worse outcomes in either dimension. Recidivism is sometimes significantly lower, and there is a very short-term positive labor market effect. But, in contrast to the mortality analysis, the recidivism results are driven by negatively selected subsamples (in terms of criminal careers or societal connections); this suggests that the health-improving effects of the reform are unlikely to be driven by only the lifestyle improvement channel.

The remainder of the paper proceeds as follows. Section I describes the reform and the relevant Swedish institutions. Section II presents the research design, while Section III describes the data. Section IV studies the implementation of the reform and identifying assumptions. Section V presents the results. Mechanisms are discussed in Section VI, and Section VII concludes.

I. Institutional Background

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

In 1990, inmates serving sentences of two months or less were not eligible for early release. Inmates serving three months were released after two-thirds of their sentences, while those serving four months or more were released after one-half of their sentences. Only particularly dangerous criminals could be held longer—up to two-thirds of their sentences—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 between 4 and 24 months would be required to serve two-thirds of their sentences, 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 to 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 one month were required to serve two-thirds of their sentences (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 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

---

[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- to 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 percent).

[10]The first formal motion was filed in March 1998 and was passed by the Swedish Parliament on June 3, 1998.
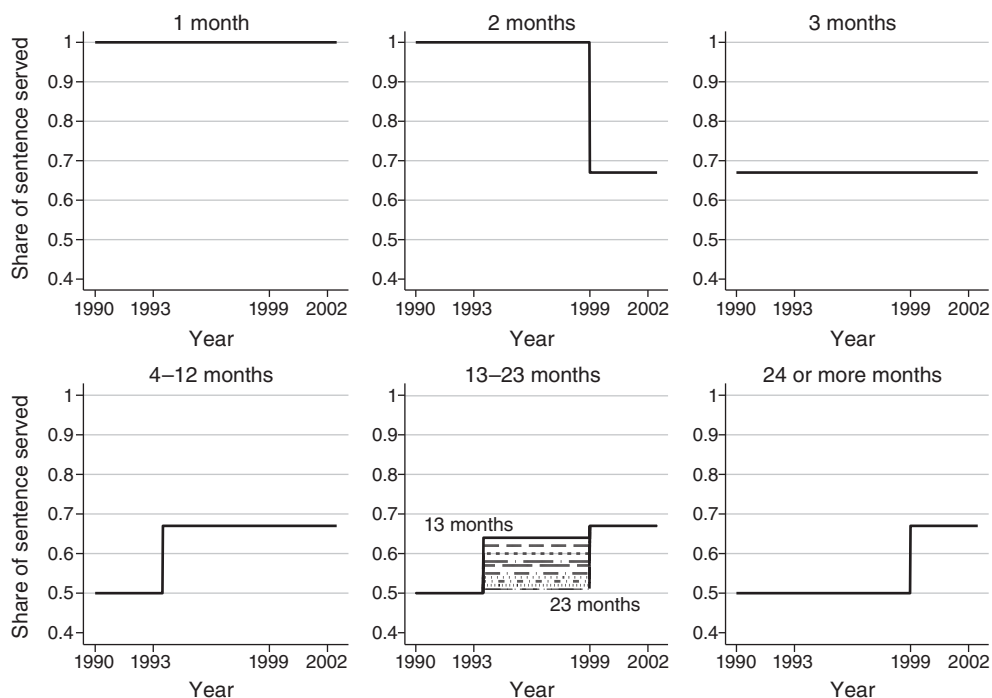
FIGURE 1. SWEDEN'S EARLY RELEASE POLICIES, 1990–2002

Notes: 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 from one-half to two-thirds for those with sentences of 4 to 12 months in prison. Those with sentences of 13 to 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 two or more months in prison to serve two-thirds of their sentences.

former inmates. Increasing the share of time served to two-thirds of sentences 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 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 online 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 IVD), our analysis focuses on sentences of 4 to 48 months in prison. Before the 1993 reform, individuals were required to serve 50 percent of such sentences, and after the 1999 reform the requirement became 67 percent (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.

B. *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 percent lower than the Western European average and ten times lower than the United States. (See panel A of online Appendix Figure 1). Sentences are also shorter in Sweden; 84 percent 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 30 months in US state prisons in 2000 (Durose and Langan 2003).[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.

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 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 online 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 details about working, education, substance abuse or psychological treatment, visitation rights, and 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

---

[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 percent to 67 percent for those sentenced to two months around the 1999 reform, whom we do not study here.

[12] Since the US estimates exclude short jail sentences, the difference between the United States and Europe may be exaggerated.

[13] Today there are 45 prisons: 12 open facilities, 4 mixed (open and medium-security facilities), 22 medium-security facilities, and 7 high-security facilities. Security class and prison assignment are based on crime severity, escape risk, gender, age, rehabilitative needs, and family ties.

to the health care system outside of prison.[14] Using detailed data about health care utilization for prisoners from more modern cohorts (2009–2013) than our reform sample, we will demonstrate in Section VIB that health care utilization in prison is high and that both utilization and treatment intensity increase with time served.

II. 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 outcomes such as mortality, recidivism, and labor market participation. We overcome this identification challenge by taking advantage of a key distinguishing feature of the early release reforms: they affected the share of time spent in prison without impacting the actual sentence length. The underlying intuition of our design is that we compare individuals with the same sentence (and offense characteristics), but who served different amounts of time in prison because they were convicted before or after one of the reforms. These reforms generate multiple sources of variation in prison exposure. All sentence month bins (from 4 to 48 months) are eventually fully treated by the reforms, which raises the share of time served from one-half to two-thirds. But as described earlier the timing of treatment varies across bins. Moreover, the intensity of treatment also varies across sentence bins and time: equal treatment in terms of the share of time served (i.e., an increase of about 17 percentage points) translates into variation across sentence lengths in the number of days served, and some bins are only partially treated at each reform. Finally, the reform can impact not just the number of days spent in prison but can also change how the existing days were spent (e.g., if more time in prison yielded eligibility for a particular treatment program).

Our empirical analysis combines these multiple sources of variation into a single treatment variable, *Treated*, which we use to estimate the reduced-form effect of an individual's full exposure to these reforms. 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 nonexposed sentences (i.e., the law prescribes one-half time served for that conviction date and sentence). As sentences of 13 to 23 months are partially treated by both reforms, *Treated* for these sentences equals zero prior to the 1993 reform, one after the 1999 reform, and the fraction of treatment for the in-between period.[15]

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

[14] This description of conditions and health care access in Swedish prisons contrasts with those of the US prisons. The analysis of prison conditions in the US by Katz, Levitt, and Shustorivich (2003) uses death rates in custody as a proxy and highlights this measure as an indication of inadequate health care, which was the subject of many lawsuits.

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

The baseline specification includes sentence month bin fixed effects, α_s, to compare individuals with the same sentence but who are exposed to different early release laws. X_i includes a full set of criminal justice controls (crime type dummies, age at incarceration, and number of contemporaneous crimes, past crimes, and prison sentences) that should affect sentence length (e.g., within-bin variation). To increase precision, X_i also includes court- and calendar-month-of-conviction dummies (which control for seasonal variations in convictions), 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 difference-in-difference 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. We demonstrate that our results are robust to a number of alternative specifications chosen in light of these two frameworks: difference-in-differences (e.g., sentence bin by year fixed effects, bin-specific trends, and parallel trends in prereform health) and regression discontinuity (e.g., nonlinear trends, split-linear trends allowed to differ on each side of the two reforms, and dropping donuts around the reforms).

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 affects 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 IV. 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 VI 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. In doing so, we exclude any potential incapacitation effect on mortality in order to obtain a clean measure of the post-release effect (even in the short run). However, 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. Since the time

[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.

spent in prison for 4- to 48-month sentences ranges from 2 to 32 months, estimates for the first three years post admission include a combination of incapacitation and post-release effects.[17] Third, 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).

III. Data

A. *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 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 assigning treatment status. We also use the conviction data to measure past offenses and prison spells, current offense characteristics, and post-release recidivism.

Mortality data 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 suicide, intentional violence, cancer, circulatory disease, and digestive disease. In each case, the coroner also notes whether the death was alcohol 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 date of death provided by Statistics Sweden.

Finally, we create measures of pre-incarceration health and health care utilization using data from the National Board of Health and Welfare's hospital inpatient registers, which are available from 1987 onward. These data include the dates of

---

[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 in, for instance, Rose and Shem-Tov (2021). Given our interest in post-release health, we use the date of release as our baseline but demonstrate robustness to using the (less endogenous) prison start date. Given the substantial variation in the amount of time between conviction and prison start dates—which is a function of many observable and unobservable factors—and our goal of studying the effect of treatment in prison, we do not use conviction dates.

[18] As data on days in pretrial 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.

admission and release as well as the admitting ward, which we categorize as psychiatric, alcohol, narcotics, and general (excluding maternity wards).

The remaining variables—including birth dates, immigration/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 us to create both contemporaneous variables and variables prior to a person's conviction.

B. *The Analysis Sample*

To treat both reforms in a reasonably symmetrical manner, we focus on sentences that start between 1992 and 2001. Online 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 subsample of sentences greater than three 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 percent of the sample, we can identify such a date. About half of the sample start their sentences within three months of conviction, 90 percent within 400 days, and 97 percent 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 more than a 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 zero- to three-month sentence bins, who face different sentencing laws and 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 percent have sentences of three 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 IVB demonstrates that at least a subset 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. Finally, we drop individuals for whom days served would be unaffected by the reform because they had a life sentence (N = 6), died in prison (N = 71), or were sent to a foreign prison (N = 149). We also trim the sample to exclude those who served more than 110 percent of their sentence (N = 982) or less than 10 percent (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 pretrial detention (for which we have no data). As these statistics include 0- to 3-month sentences, the number of individuals dropped from the 4- to 48-month analysis sample is less than that reported here. We present sensitivity analyses including both the straddle and trimmed samples, as well as those who died in prison (in a discussion of culling).

TABLE 1—SUMMARY STATISTICS

Variable	Main analysis sample 4–48 month bins N = 46,815		0–3 month bins N = 39,294	4–12 month bins N = 33,799	13–24 month bins N = 8,968	25–48 month bins N = 4,048
	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	1,069.38
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 or 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

Notes: 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 percent of the main analysis sample. A dummy indicating whether it is missing is included in regression specifications.

C. *Summary Statistics*

Table 1 provides summary statistics for the main analysis sample of 46,815 4- to 48-month sentences, for which the average number of months (days) sentenced is 11.7 (354), and for comparison purposes, for 0- to 3-month sentences. Different types of offenses lead to longer sentences; 37 percent of the 0–3 month sample are DUI (driving under the influence) offenders, primarily from the early 1990s, and 24 percent and 20 percent were charged with violent and property offenses, respectively. In contrast, just 3 percent of the analysis sample are charged with DUI, while 33 percent, 43 percent, and 15 percent 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. The only measure of pre-incarceration health available during this period is hospitalization, which likely captures a combination of health and health care utilization. The analysis sample has a similar average number of
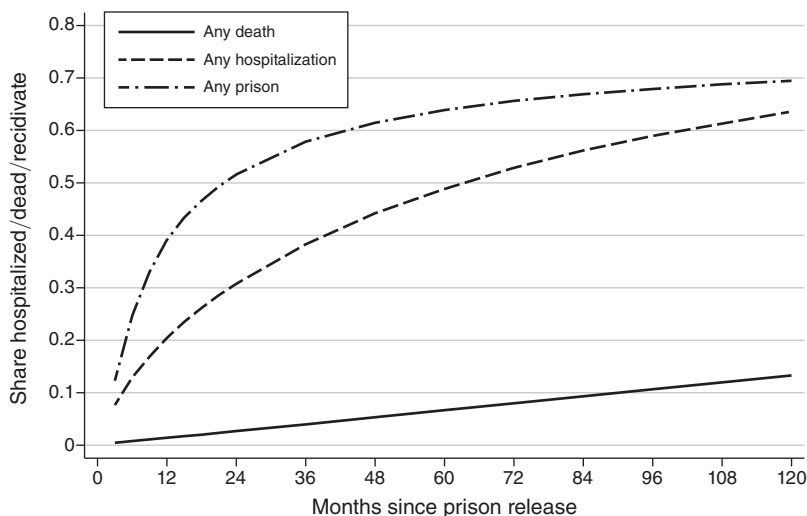
FIGURE 2. DESCRIPTIVE STATISTICS: DYNAMIC PATHS OF MORTALITY, RECIDIVISM, AND HOSPITALIZATION

Notes: For all 4-to 48-month sentences, this figure shows the dynamic paths for death due to any cause (solid line), any hospitalization (dashed line), and any prison (dash-dotted line), which are measured at *t* months post release and conditioned on the sample alive and in Sweden at that time.

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 versus 0.51 days) and especially psychiatric (5.38 versus 2.79 days) wards.

Of course, this negative selection for those with 4- to 48-month versus 0- to 3-month sentences is not the margin we study. 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 again highlight differences in observables but also highlight that observables are not monotonically worse as sentences get longer. Rather, they are likely driven in part 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 and alcohol, property, and violent offenses in each group is 13 percent, 49 percent, and 28 percent (4–12 months); 17 percent, 35 percent, and 43 percent (13–24 months); and 28 percent, 18 percent, and 52 percent (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 are seen for 4- to 12-month sentences. Psychiatric days are the most prominent for all subsamples.

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 percent of the analysis sample have died by eight years post release and 12 percent by age 50; this compares to just 3 percent in the full Swedish population (with similar demographic characteristics). Hospital utilization rates are also quite
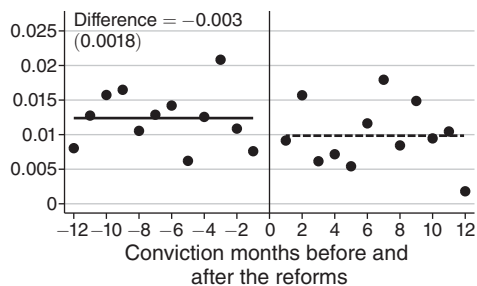
Panel A. Mortality for those affected by the reforms
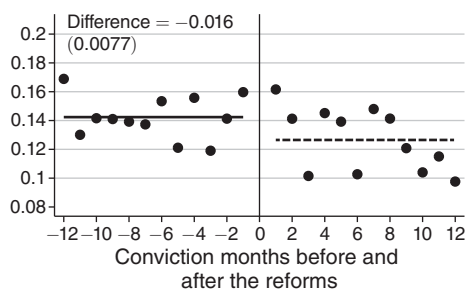
Panel B. Mortality for those not affected by the reforms
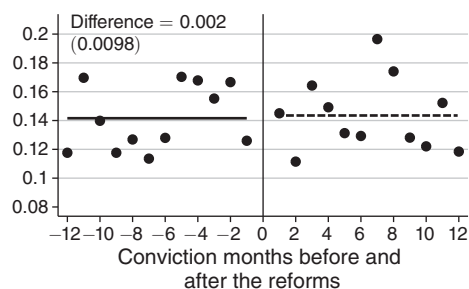
Panel C. Suicide for those affected by the reforms

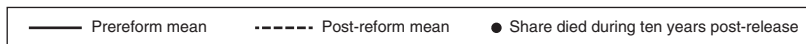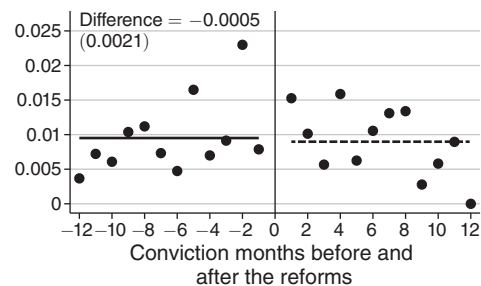Panel D. Suicide for those not affected by the reforms

FIGURE 3. COMPARISON OF MEAN TEN-YEAR MORTALITY (ALL-CAUSE AND SUICIDE) FOR INMATES AFFECTED AND UNAFFECTED BY THE REFORMS

Notes: This figure is created using a subsample of our data that includes those convicted within a 24-month window around each reform. The two reforms are pooled together and the reform dates are set to zero. Scatterplot dots represent conviction-month average mortality rates. Horizontal lines represent pre- and post-reform mean mortality rates. The left panels show results for those who are partially or fully treated by a reform. The right panels show results for those who are unaffected by the reforms (i.e., their treatment status does not change at the specific reform date). Differences and standard errors (in parentheses) are calculated in a simple pre- versus post-reform regression using the 24 scatterplot data points included in each panel of the figure.

high: by eight years post release, 60 percent have been hospitalized at least once (dashed line). This figure also shows that recidivism rates (dash-dotted line) are steep: more than 50 percent are reincarcerated within 12 months and 60 percent by 48 months.[19]

Figure 3 looks at the mean differences in two of our most important outcomes, all-cause mortality and suicide, measured ten years after release. Panels A and C show that those groups that are affected by the reforms have a clear decrease in both all-cause mortality and suicide during the post-reform period, while panels B and D demonstrate that there is no such drop in mortality for groups not exposed to the reforms. Using these means to calculate a simple difference-in-difference measure of the relative decrease in early deaths among those affected by the reforms

---

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

produces a 13 percent drop in the all-cause mortality rate (relative to the mean) and a 27 percent drop in the suicide rate. This is one indication that spending more time in Swedish prisons may in fact improve the health of former inmates.[20]

IV. Sentencing Reform Implementation

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

A. *No Impact on Sentence Length or Earlier Judicial Decisions*

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 pretrial 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.[21]

We begin with sentencing, which is perhaps the most salient dimension on which such manipulation could occur. Panels A and B of Figure 4 show the sentencing distributions (measured in prison sentence days) for those convicted within a four-year window around the 1993 and 1999 reforms, respectively. For each reform, we look at the distribution for three subsamples: those who are convicted and start their sentences prior to the reform (solid line), those who are convicted and start their sentences after the reform (dotted line), and those who are convicted before but start their sentences after the reform (dashed line). We refer to this last subgroup as the "straddle sample." These figures demonstrate that the sentence-length distributions do not change around the reforms. There is no evidence of a downshift in the treated sentence month bin regions. Rather, the sentence length distributions lie practically on top of each other for each subsample. Thus, there is no evidence of manipulation in sentencing. Furthermore, online Appendix Figure 2 provides evidence that the reforms did not systematically affect the share of cases that received a waiver of

---

[20] In Figure 3 we calculate the pre- and post-reform mortality rates of those who are affected by the two reforms and compare them to those who were convicted during the same time period but are not affected by the reforms. To do this, we take a subsample of our data, namely those who are convicted within a 24-month window around each reform. We pool the two reform periods together and set the reform date to zero. We then collapse our mortality variables by conviction month and treatment status. Conviction month runs from $-12, \ldots, 0, \ldots, 12$. Treatment is equal to one if an individual is either fully or partially affected by the reform and zero otherwise. The scatterplot dots in Figure 3 represent these conviction-month average mortality rates while the horizontal lines represent the pre- and post-reform mean mortality rates. Within-group differences and standard errors (in parentheses) are reported in each panel of Figure 3. These are calculated using a simple pre- versus post-reform regression. The difference in these differences (mentioned in the text) is calculated using a standard difference-in-difference regression.

[21] Bjerk (2005); Ulmer, Kurlychek, and Kramer (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.

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
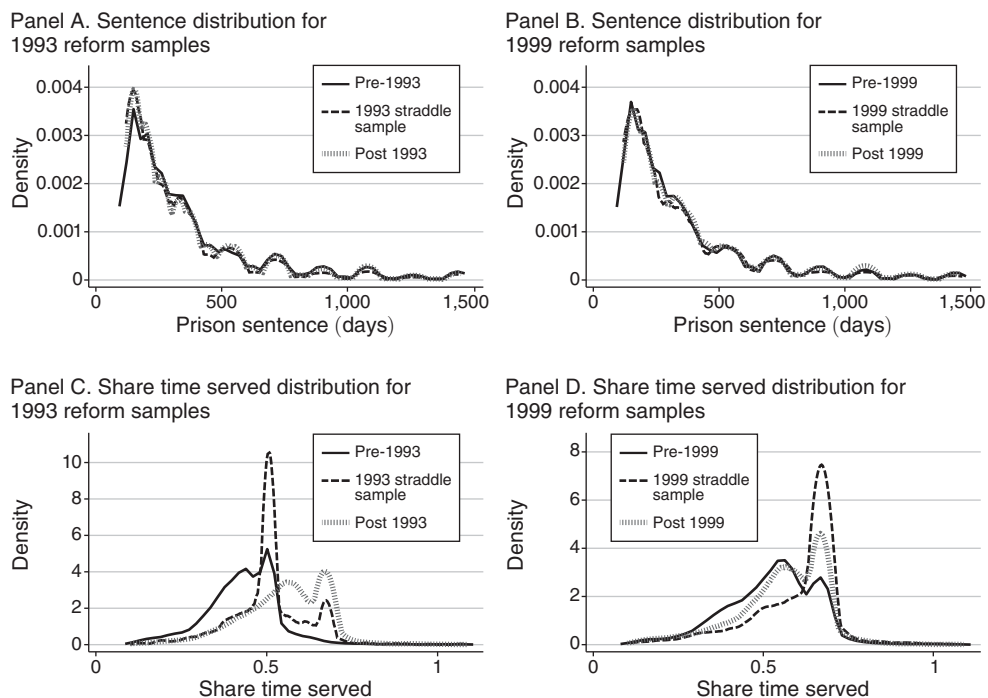1999 reform samples

FIGURE 4. KERNEL DENSITIES: SENTENCE AND SHARE TIME SERVED DISTRIBUTIONS FOR PREREFORM, POST-REFORM,
AND STRADDLE SAMPLES

Notes: 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 two years of the reforms on either side of the cutoff. We decompose these samples into three subsamples: pre-199X (solid line, conviction and sentence start dates prereform), post-199X (dotted line, conviction and sentence start dates post reform), and straddle sample (dashed line, conviction prereform and sentence start date post reform).

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 in agreement with the evidence, limiting the extent to which defendants could, in practice, be charged with a lesser crime.[22] Importantly, the prosecutor is not involved in sentencing, which is left to the judge and lay judges; discretionary sentencing decisions are limited by the sentencing window (the minimum and maximum sentence for each crime) being prespecified and relatively narrow.

B. *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

---

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

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 two months in 1999), the conviction date, rather than the start date, should in theory be the binding date for our analysis sample of 4- to 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 subsamples (pre-, post, and straddle) in panels C and D of Figure 4. These figures display the distribution of the share of time served for each subgroup. Though the distributions for the straddle sample (dashed line) should look like that of the prereform 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 started their sentence after the reform.

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

How did the share of time and 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 5 present the results of regressing the share of time and number of days served, respectively, on a dummy indicating whether the conviction occurred after July 1, 1993 for each 0- to 48-month sentence bin. Vertical bars are placed at 4, 12, and 24 months, as these are the treatment thresholds defined in the law. Share served is unaffected for 0- to 3-month sentences or sentences greater than 24 months. Share served significantly increases by 13 to 16 percentage points in each 4–12 month bin and by gradually lesser amounts for 13- to 24-month sentences. In other words, 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. 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; additional days served decrease with sentence lengths in the 13–24 month range.

Panel C of Figure 5 demonstrates the same pattern for the 1999 reform: share of time served only changes for bins that should be affected, and the changes are consistent in both sign and magnitude with that predicted by the reform. Panel D shows that the reform increases number of days served, with the largest effect in the 25 month bin (more than 130 days).

Panel A of Table 2 summarizes 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. For the full sample (column 1), being treated or fully exposed to the two-thirds reform increases the number of days served by 46, on average, with an associated F-statistic of 109. Each additional column of Table 2 corresponds to the portion of the sample who are alive and have never emigrated

Panel A. 1993 share time served

Panel B. 1993 prison days

Panel C. 1999 share time served

Panel D. 1999 prison days
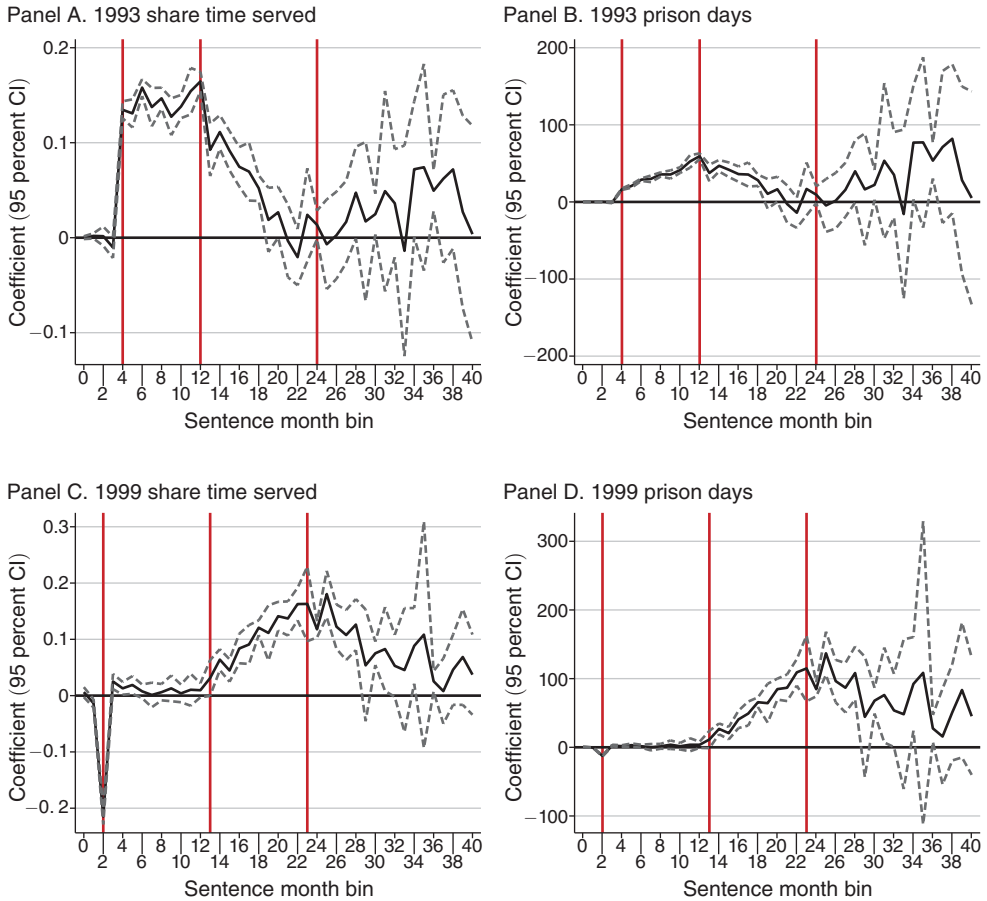
FIGURE 5. IMPLEMENTATION OF 1993 AND 1999 REFORMS: EFFECT ON SHARE OF TIME AND ACTUAL DAYS SERVED BY
SENTENCE LENGTH BINS

Notes: 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 four-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 percent confidence interval are plotted.

from Sweden until date t (one, two, three, and ten years) after release. The relationship between reform exposure and 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. Online Appendix Table 3 demonstrates that the reform is relevant across a wide range of subsamples characterizing the offender's current and past offense history, demographic and socioeconomic characteristics, and pre-incarceration health. These results point toward the validity of our heterogeneity analyses.

D. *Conditional Independence*

For the reduced-form estimates to be interpreted causally, exposure to the early release reform should be unrelated to defendant and case characteristics that could

TABLE 2—RELEVANCE: THE EFFECT OF THE TWO-THIRDS SENTENCING REFORM ON DAYS SERVED

	Dependent variable: Days served in prison				
	All sample	1 year post release	2 years post release	3 years post release	10 years post release
	(1)	(2)	(3)	(4)	(5)
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 (only bin fixed effects, year fixed effects, and conviction 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 number died	0	658	1,240	1,809	5,926
Cumulative number emigrated	0	528	881	1,186	2,255
Observations	46,815	45,629	44,694	43,820	38,634

Notes: Each column includes the portion of the sample who are living and never emigrated from Sweden since the date of release. Panel A (the baseline specification) includes controls for more than 60 crime type dummies, number of current offenses, number of past crimes, number of past prison admissions, age $<= 21$, and age at incarceration, as well as calendar-month dummies for conviction, court dummies, and demographic, 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 are in parentheses.

also affect the post-release outcomes. Such correlation could occur, for instance, if there were a systematic response on the part of the justice system (e.g., judges or prosecutors) to the reform; we have already shown in Section IVA that this is not the case. Alternatively, the assumption could be violated if other contemporaneous criminal justice reforms systematically affected the sentence that defendants with certain characteristics should receive. This is unlikely given that other reforms in this period only affected shorter sentences of 0–3 months, drunk driving offenses, and youths; i.e., case types that are almost completely excluded from our analysis sample.[23] Online Appendix Table 4 directly tests for balancing on observables (current and past offenses, demographics, socioeconomic characteristics, pre-incarceration health). Specifically, we regress each observable (for 20 such variables) on whether the individual was exposed to the reform and the trends controlled for in our baseline specification. Only one coefficient—the number of contemporaneous crimes—is significant at the 5 percent level, while most others are close to zero or far from significant. The lack of change in these observables is also

[23] Sweden passed a series of drunk driving law reforms in February 1994. Although the text of the law 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 led to fewer individuals spending more time in prison (BRÅ 1998:7). DUI offenses are concentrated in the 0–3 month bins and hence outside of our estimation sample. Electronic monitoring in the home for those with one- to two-month sentences was piloted in August 1994. It expanded to three-month sentences and to the whole country in 1997 and was made permanent in 1999. 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. Our 4–48 month estimation sample is unaffected by each of these reforms.

TABLE 3—THE EFFECT OF EXPOSURE TO THE TWO-THIRDS REFORM ON MORTALITY
OVERALL AND BY CAUSE

	Measured t months post release			
Dependent variable	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
Observations	46,287	45,934	45,629	44,560

Notes: 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 whether each was treated (one 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 are in parentheses.

supported by the robustness of the first stage to including controls (see panel B of Table 2) as well as the robustness of the main results (shown later).

V. The Effect of Reform Exposure on Mortality

A. *Baseline Results*

Table 3 presents the estimated coefficient on *Treated* in equation (1) for all-cause mortality and for mortality by cause of death measured at 12, 24, 36, and 120 months since date of release.[24] Exposure to the two-thirds reform reduces all-cause

---

[24] As highlighted in Section II, each regression conditions on the sample of individuals who have never emigrated t months since release. This explains the reduction in sample size across columns from 46,287 at 12 months after release to 45,934, 45,629, and 44,560 at 24, 36, and 120 months post release. Comparing summary statistics for the samples that leave and those that stay indicates that these two groups are very similar in many observable characteristics including gender, age, sentence lengths, and crime types. The main difference is that those who stay
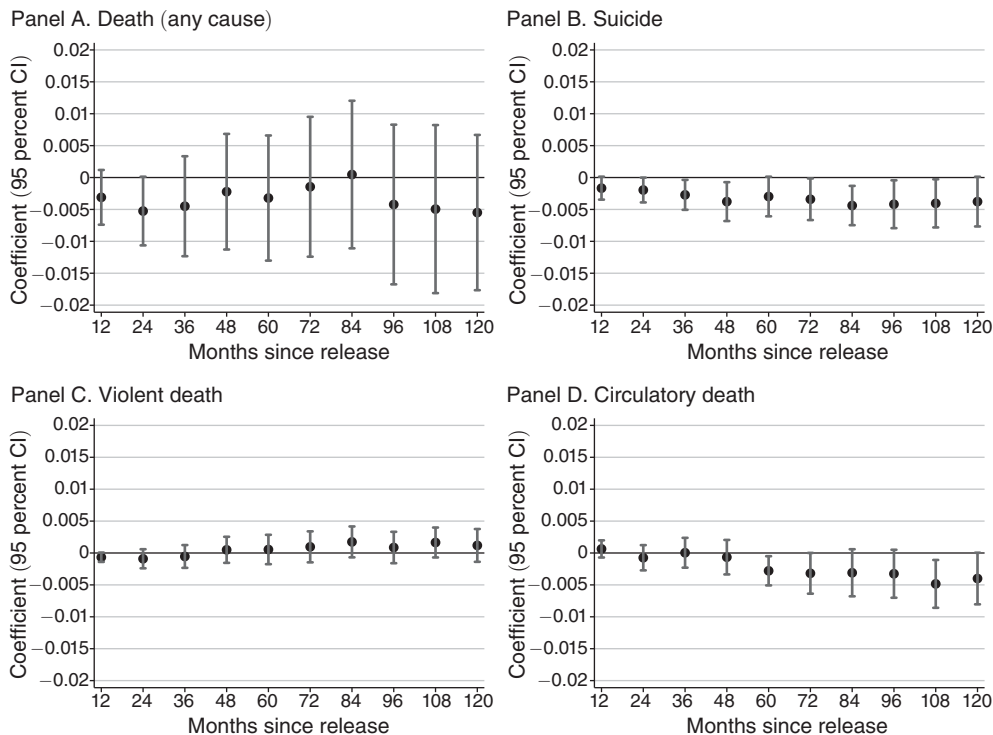
FIGURE 6. DYNAMICS OF MORTALITY EFFECTS

Notes: Panels A–D present the estimated coefficient and 95 percent 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.

mortality. However, these results are only significant (at the 10 percent level) in the 24-month window. Figure 6, panel A traces the dynamics of the effect at each of the first ten 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 and in fact some estimates become more precise in the robustness checks in Section VB.

These aggregate results mask important heterogeneity in the effect of prison on the cause of death and the chance of death for different subpopulations. Specifically, as highlighted earlier, this population has a disproportionate amount of mental health problems. Moreover, criminals lead generally risky lifestyles, which exposes them to violence. We therefore consider the effect of spending more time in prison on the chance of suicide and violent death. We also examine drug- and alcohol-related deaths, which could be affected by treatment programs while in prison and by being

---

are much more likely to be born in Sweden (78 percent versus 42 percent) and to be Swedish citizens (83 percent versus 48 percent). In short, immigrants tend to emigrate from Sweden more often than natives do. This is true in the nonprison population as well (see, e.g., *Utvandring från Sverige* [*Emigration from Sweden*] (SCB 2020)).

kept sober and drug free for an extended period of time. Finally, lifestyle changes in prison (e.g., controlled diets and exercise) and regular health care may have an effect upon natural causes of death such as 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; smoking outdoors is not banned in prison.

Table 3 presents the effect of being treated by the reform on each cause of death at 12, 24, 36, and 120 months post release. Dynamics are traced for suicide, violent, and circulatory deaths in Figure 6 and for cancer, digestive, and alcohol- and narcotic-related deaths in online 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); Figure 6, panel B confirms that this effect is seen in each of the first ten post-release years. Relative to the mean post-release suicide rates (at the bottom of the table), reform exposure reduces the chance of suicide by 79 percent in the first three years; a 38 percent reduction remains ten 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 ten. The immediate (12-month) short-run reduction in the chance of violent death is significant at the 10 percent 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 is quite large (a 100 percent reduction). Figure 6, panel C shows that the reduction in violent deaths is short term in nature, and by year four post release the estimates have swung (permanently) positive.

The next set of results in Table 3 is for circulatory, digestive, and cancer-related deaths. For the first three years in each category, there is an insignificant effect. However, in the long term, ten years post release, there is a significant reduction (10 percent level) in the chance of a circulatory death: reform exposure reduces the chance of circulatory death by 27 percent. Figure 6, panel D demonstrates that this general health improvement seen at year ten 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 are often attributed to lifestyle behaviors over an extended period of time, it is perhaps not surprising that this effect is only seen in the medium and long run. A similar (yet mostly insignificant) pattern is seen for digestive deaths. But longer stays in prison have no effect on cancer-related deaths (see panels B and C of online Appendix Figure 3).

The final panel of Table 3 shows the results for narcotics- and 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 deaths is traced out in panel A of online Appendix Figure 3. Though the point estimates swing positive at year four, they are never significant.

TABLE 4—ROBUSTNESS CHECKS OF THE EFFECT OF THE TWO-THIRDS REFORM ON MORTALITY

	Death (any cause)				Suicide		
12	24	36	120	12	24	36	120
Panel A. Baseline							
−0.0031	−0.0053	−0.0045	−0.0055	−0.0017	−0.0020	−0.0027	−0.0038
(0.0021)	(0.0027)	(0.0039)	(0.0060)	(0.0009)	(0.0010)	(0.0012)	(0.0019)
Panel B. Only criminal justice controls							
−0.0036	−0.0062	−0.0052	−0.0053	−0.0016	−0.0018	−0.0026	−0.0032
(0.0021)	(0.0028)	(0.0039)	(0.0060)	(0.0009)	(0.0009)	(0.0012)	(0.0019)
Panel C. Baseline without conviction month trend							
−0.0031	−0.0053	−0.0045	−0.0055	−0.0017	−0.0020	−0.0027	−0.0038
(0.0021)	(0.0027)	(0.0039)	(0.0060)	(0.0009)	(0.0010)	(0.0012)	(0.0019)
Panel D. With sentence month bin × year fixed effects							
−0.0027	0	−0.0092	−0.0152	−0.0035	−0.0031	−0.0066	−0.0043
(0.0029)	(0.0040)	(0.0047)	(0.0081)	(0.0013)	(0.0015)	(0.0024)	(0.0033)
Panel E. With sentence-month-bin-specific conviction month trend							
−0.0034	−0.0050	−0.0041	−0.005	−0.0017	−0.0020	−0.0028	−0.0039
(0.0022)	(0.0028)	(0.0040)	(0.0064)	(0.0009)	(0.0010)	(0.0012)	(0.0020)
Panel F. With split linear trend (different trends allowed between 1993 and 1999 and post 1999)							
−0.0043	−0.0078	−0.0035	0.0002	−0.0011	−0.002	−0.0016	−0.0028
(0.0026)	(0.0037)	(0.0048)	(0.0095)	(0.0010)	(0.0012)	(0.0012)	(0.0024)
Panel G. Quadratic conviction month trend							
−0.003	−0.0051	−0.0043	−0.0046	−0.0016	−0.0017	−0.0025	−0.0039
(0.0022)	(0.0028)	(0.0042)	(0.0066)	(0.0010)	(0.0010)	(0.0011)	(0.0020)
Panel H. Donut: Dropping four-month conviction interval around each reform							
−0.0060	−0.0088	−0.0072	−0.0065	−0.0017	−0.0023	−0.0029	−0.0047
(0.0025)	(0.0033)	(0.0048)	(0.0067)	(0.0010)	(0.0010)	(0.0013)	(0.0021)
Panel I. 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
(0.0020)	(0.0026)	(0.0039)	(0.0066)	(0.0009)	(0.0010)	(0.0012)	(0.0019)
Panel J. Baseline with probit							
−0.0028	−0.0058	−0.0055	−0.0083	−0.0010	−0.0017	−0.0028	−0.0032
(0.0018)	(0.0024)	(0.0035)	(0.0060)	(0.0004)	(0.0008)	(0.0009)	(0.0017)

Notes: 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 the others; 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 are in parentheses.

B. *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 for suicide measured 12, 24, 36, and 120 months post release.[25] Panel B demonstrates robustness to excluding all but the criminal justice

[25] The same robustness tests have been carried out for violent deaths (with close attention to the 12-month outcome) and circulatory death (with attention to the 120-month outcome). The results are available upon request.

observable controls. Panel C demonstrates robustness to excluding the linear conviction month trend. To the extent one is concerned about differential trends across sentence month bins (in the spirit of a difference-in-difference design), panels D and E present the results of including sentence month bin by year fixed effects and bin-specific conviction month trends, respectively.[26,27] Panels F–H demonstrate robustness to specification checks motivated by the parallels of our design to a regression discontinuity analysis. Panel F allows for a different linear trend for each period (pre-1993 reform, between reforms, and post-1999 reform). Though the same general pattern of results is seen, significance decreases due to a loss of precision (and sometimes smaller coefficients). Panel G demonstrates robustness to a quadratic conviction month trend, while panel H drops a four-month donut around each reform, demonstrating robustness to any potential manipulation (minimal or nonexistent though it may be). Finally, panel I replaces the baseline prison sentence month bin fixed effects with prison sentence day fixed effects, comparing individuals with exactly the same sentences, while panel J 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 (not dropped for other reasons) with sentences of 4 to 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 among suicides, but rather the largest share is circulatory, digestive, and cancer related (25 percent, combined). Moreover, estimating the baseline specification where the dependent variable is death in prison yields no evidence that reform exposure significantly affects the chance of death in prison (results available on request).

Our next analysis (presented in online Appendix Figure 5) replicates the baseline dynamic mortality figures allowing individuals to be at risk of death from the date of prison admission rather than the date of prison release. This analysis serves two purposes. First, we are again including those individuals who died in prison and controlling for possible culling effects. Second, to the extent one is concerned about the release date being partially endogenous, the prison start date is less subject to

---

[26] Moreover, online Appendix Figure 4 provides evidence that there are parallel preform trends in hospitalization rates among those who were treated and untreated by the reforms. We see parallel trends in the average number of nights spent in general hospital wards, psychiatric wards, alcohol wards, and narcotics wards. We also see parallel trends when looking at each reform separately.

[27] Goodman-Bacon (2020) highlights another concern in difference-in-difference designs in which the treatments are implemented at different times—namely, that earlier treated units (in our case, those with 4- to 12-month sentences treated by the 1993 reforms) end up acting as controls for later treated groups (e.g., 24- to 48-month sentences affected by the 1999 reform only). This can yield biased treatment effects if the initial treatment effect changes monotonically over time. Online Appendix Table 5 presents the results of estimating the baseline specification separately for the 1993 and 1999 reforms (using four-year windows around each). By definition, the 1993 reform results cannot be biased by time-varying treatment effects; yet they are the same as the pooled estimates and similar to the 1999 reform estimates. We also see parallel trends in pre-incarceration hospitalization rates when looking at the 1999 reform on its own. These observations speak against the existence of time-varying treatment effects that might bias our baseline estimates.

this concern.[28] The disadvantage of this specification is that the results during the first three years post admission can be a combination of incapacitation effects and post-release effects, which cannot be clearly disentangled. As seen in panel A of online Appendix Figure 5, there is a nearly significant reduction in overall mortality in the first 12 months (potentially partly incapacitation), which increases in size and remains significant (or nearly significant) through 48 months post admission. The significant suicide effect kicks in at 36 months and violent death, at 24 months. These somewhat delayed effects are consistent with the idea that our baseline findings were being driven by post-release effects.

Finally, online Appendix Table 5 assesses the sensitivity of the results to sample creation decisions. Including the straddle sample (i.e., those convicted before a reform but who entered prison after) in panel D yields the same pattern of results, with slightly smaller coefficients in most cases—which is consistent with the inclusion of the straddle sample inducing measurement error in the treatment variable. Panel E shows that the results are insensitive to including those for whom the measured share of time served was more (less) than 110 percent (10 percent).

C. *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: whether the cause of death results are driven by particular subsamples at highest risk of these types of deaths, and whether there are subsamples for whom the reform significantly decreased (or increased) the overall risk of death. Given the multiple hypotheses (in terms of subsamples and outcomes) being tested here, the conclusions of such a heterogeneity analysis should be taken as suggestive.

We first consider whether the reduced chance of suicide is driven by those with pre-identified mental health problems, which we proxy for 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 ten-year post-release suicide rate in each subsample is 2 percent (psych admission), 1.3 percent (general admission), and 0.6 percent (no admission). Figure 7, panel A presents the ten-year dynamics for each subsample. 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 before admission. Exposure to the two-thirds reform reduces the ten-year

---

[28] Though the release date is predetermined, it can be extended due to poor behavior. During the 1990s and early 2000s, a serious infraction of prison rules could lead to a delay in early release of at most 15 days (per infraction). However, such delays were used quite sparingly.

Panel A. Suicide effects by pre-incarceration hospitalization

Panel B. Violent death effects by current offense type

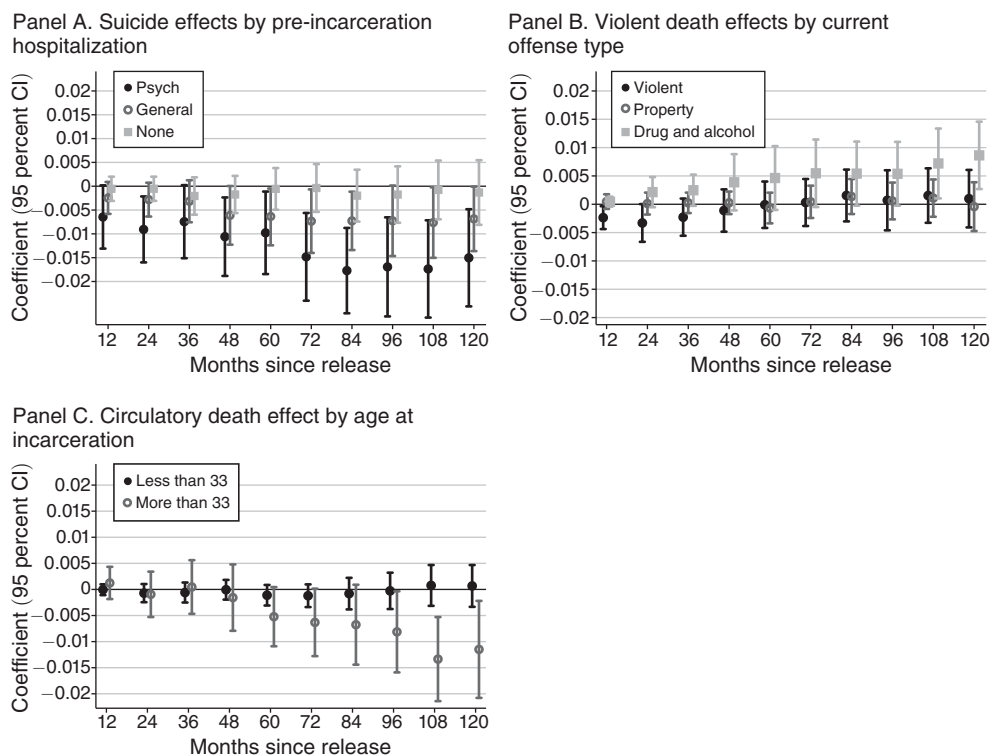Panel C. Circulatory death effect by age at incarceration

FIGURE 7. HETEROGENEITY ANALYSIS OF MORTALITY EFFECTS FOR HIGH-RISK SUBGROUPS

Notes: 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), a general ward (N = 19,176 at 12 months), or no hospitalization (N = 22,073 at 12 months). Panel B considers violent death and presents the results by current offense type: violent (N = 15,243), property-related (N = 20,084) and drug- and alcohol-related (N = 6,968). Panel C presents the circulatory death results separately by median age at prison admission ($<= 33$ (N = 23,410) and >33 (N = 21,150)). All specifications condition on being in Sweden at month t.

post-release chance of suicide by about 75 percent and 50 percent for the psychiatric and general ward samples, respectively.

Figure 7, panel B 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-related (open circle) and drug- or alcohol-related (square). Summary statistics show that the ten-year violent death rate is almost twice as large for violent offenders (0.7 percent) than for property or drug and alcohol offenders (both 0.4 percent). Figure 7 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 two years of release and persists over time. The latter could occur, for instance, if more time in prison reinforces or expands drug networks that result in more exposure to violence upon release (see Bayer et al. 2009). The former could

occur if violent offenders end up incapacitated by returning to prison (we 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. Figure 7, panel C splits the sample according to whether individuals were older or younger than 33 (the median age) at the prison start date. The share who died from circulatory-related deaths ten years post release is 2.7 percent and less than 0.4 percent for the older and younger samples, respectively. The medium- and long-run reductions in circulatory deaths are driven by the relatively older subsample.

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 subpopulations, these do not necessarily translate into a reduction in the overall risk of death. Second, there may be subpopulations for which increased prison exposure decreases the risk of death that 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 criminal history and offense characteristics, demographic characteristics, and 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).[29] 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 subsamples 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. Moreover, these effects are especially large given that mean mortality of these positively selected subsamples is markedly lower than the other subsamples (see column 3 of Table 5, which presents the mean mortality rate at 36 months post release).[30]

---

[29] For these same high-risk subsamples, online Appendix Table 6 presents results for death overall and by cause.

[30] Finally, we also assess whether the effects are heterogeneous across sentence month bins by interacting reform exposure (*Treated*) with dummy variables indicating if the sentence is 4–12, 13–23, or 24–48 months. Such heterogeneous effects could arise because of nonlinearities (reform exposure increases days served more in longer bins) or because the composition of offenders differs across bins. Online Appendix Table 7 shows that for death overall and for suicide, significant differences are not seen across bin groups. But the short-term violent death effects and longer-term circulatory effects do appear to be driven by specific bins: the former by the 4- to 12-month

TABLE 5—HETEROGENEOUS EFFECTS OF THE TWO−THIRDS REFORM ON OVERALL MORTALITY

			Dep. variable = Death (any cause) at month			
Sample	N 12 months	Mean DV 36 months	12	24	36	120
Baseline	46,287	0.04	−0.0031 (0.0021)	−0.0053 (0.0027)	−0.0045 (0.0039)	−0.0055 (0.0060)
Panel A. Heteroegeneity by criminal history and offense characteristics						
No past prison sentence	7,896	0.022	−0.012 (0.004)	−0.016 (0.005)	−0.009 (0.007)	−0.01 (0.011)
One past prison sentence	8,893	0.023	0.001 (0.003)	−0.004 (0.004)	0.003 (0.007)	0.001 (0.016)
More than one past prison sentence	29,498	0.049	−0.002 (0.003)	−0.003 (0.005)	−0.007 (0.006)	−0.007 (0.008)
Property offender	20,084	0.04	−0.006 (0.003)	−0.006 (0.004)	−0.012 (0.005)	−0.011 (0.007)
Violent offender	15,243	0.041	−0.002 (0.004)	−0.009 (0.004)	−0.002 (0.006)	−0.003 (0.009)
Drug/alcohol offender	6,968	0.034	−0.004 (0.006)	−0.002 (0.007)	−0.004 (0.009)	−0.01 (0.021)
Panel B. Heteroegeneity by demographic characteristics						
< 33 years old	24,417	0.032	−0.006 (0.002)	−0.009 (0.004)	−0.009 (0.004)	−0.001 (0.008)
>= 33 years old	21,870	0.048	0 (0.004)	0 (0.005)	0.001 (0.007)	−0.012 (0.012)
Not born in Sweden	10,699	0.034	−0.003 (0.004)	0 (0.005)	−0.002 (0.007)	0.028 (0.014)
Born in Sweden	35,588	0.041	−0.003 (0.003)	−0.007 (0.003)	−0.005 (0.004)	−0.014 (0.008)
Panel C. Heteroegeneity by pre-incarceration SES and health characteristics						
Unemployed last three years	31,209	0.047	−0.001 (0.003)	−0.003 (0.004)	−0.003 (0.006)	−0.01 (0.007)
Employed at least once	15,078	0.025	−0.007 (0.002)	−0.009 (0.004)	−0.007 (0.007)	0.001 (0.009)
Psychiatric hospitalization in the last five years	9,897	0.07	−0.004 (0.008)	−0.011 (0.010)	−0.007 (0.012)	−0.006 (0.015)
General ward hospitalization in the last five years	19,176	0.056	−0.004 (0.004)	−0.009 (0.006)	−0.006 (0.009)	−0.009 (0.012)
No hospitalization in the last five years	22,073	0.022	−0.004 (0.002)	−0.004 (0.004)	−0.005 (0.004)	−0.004 (0.007)

Notes: 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 one, two, three, and ten years post release. Standard errors clustered on sentence month bin are in parentheses.

VI. 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. This section assesses whether the reform entailed any significant changes to the prison

---

and the latter by the 24- to 48-month bins. It is certainly feasible that the latter represents a nonlinear 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.

experience other than increasing the number of days in prison. What is actually included in the reduced-form package we study? We then explore two potential mechanisms through which more time in prison could improve mental and physical health: the direct effect of health care provision in Swedish prisons and an indirect channel via the effect of more time in prison on recidivism and labor market outcomes.

A. *Prison Days versus Other Changes to the Prison Experience*

We have thus far focused on the increased number of days inmates spend in prison, i.e., the first-order impact of the reform. 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 reentering society with different release dates.[31] 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 online Appendix Figure 6). 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 online Appendix Figure 7, 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 security, 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 treatments to which they assigned inmates or the quality of care. Though we cannot

---

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

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 reestimating our baseline specification with facility type as the dependent variable.[32]

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 online Appendix Figure 7 plots the average sentence length of inmates. This does not change around (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.

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.[33]

B. *Health Care Provision and Treatment Programs in Swedish Prisons*

This section demonstrates that more time in a Swedish prison could directly improve an inmate's health. As discussed in Section IB, 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 health care utilization of Swedish inmates by studying all inmates with 4- to 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 all visits to doctors, nurses, and psychologists while in prison; the administration of medicines; and various treatment programs in which inmates are enrolled.

Health care variables by sentence month bin are shown in Figure 8. These exclude the initial health examination that all inmates receive upon intake. The extensive margin variables in panel A indicate that the take-up rate of health care 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 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. Furthermore, necessary

---

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

[33] 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 reestimate 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 that other aspects of prison experience do not change discontinuously), there is a substantial loss in precision, especially within a window of six months on either side of the reform.
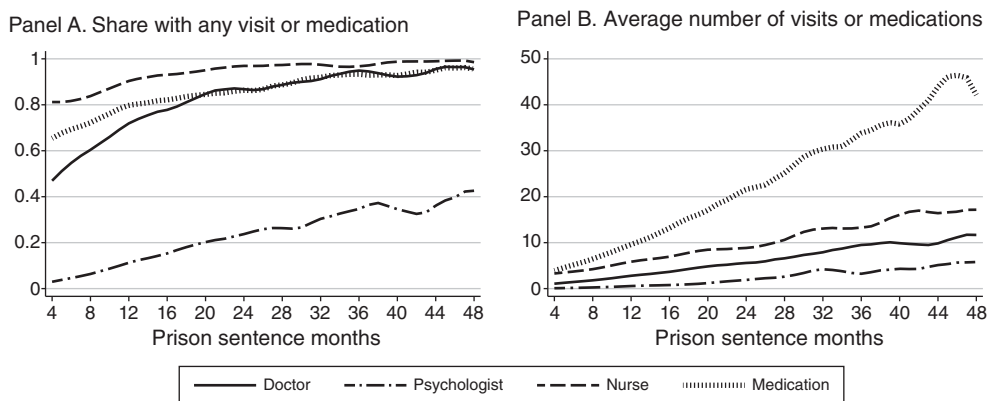
Panel A. Share with any visit or medication

Panel B. Average number of visits or medications

FIGURE 8. IN-PRISON MEDICAL DATA FOR ALL INMATES WITH SENTENCES OF 4–48 MONTHS ENTERING PRISON FROM 2009–2013

Notes: All lines are generated using a lowess smoother with a bandwidth of 0.2. 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.

medicines are administered by trained personnel on a daily basis, which may actually help some inmates properly follow medication regimens.[34]

Many inmates take part in professional treatment programs to help address mental health issues or alcohol and drug abuse. Figure 9, panel A 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): a motivational interview, Alcoholic/Addicts Anonymous's 12-step program, and the Correctional Service of Canada's Offender Substance Abuse Pre-Release Program (OSAPP). Similar programs were also available to inmates during the 1990s.

Figure 9 shows that more than 80 percent 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 US 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

---

[34] Nonadherence to prescribed medication regimens is a leading cause of preventable morbidity and mortality (Krueger, Berger, and Felkey 2005). US and Swedish prison officials are acutely aware of this issue. Though the Swedish Prison and Probation Service cannot force an inmate to take their medicine, they do provide additional service and information to those at risk of nonadherence. The US 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).
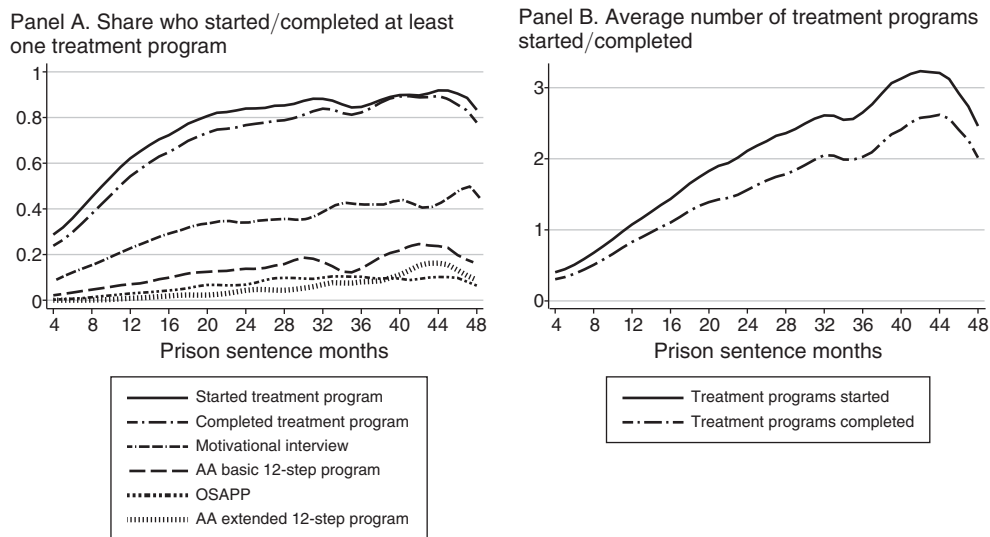
Panel A. Share who started/completed at least one treatment program

Panel B. Average number of treatment programs started/completed

FIGURE 9. IN-PRISON TREATMENT PROGRAM PARTICIPATION OF INMATES WITH 4- TO 48-MONTH SENTENCES ENTERING PRISON BETWEEN 2009 AND 2013

Note: All lines are generated using a lowess smoother with a bandwidth of 0.2.

than 90 days, with treatment completers achieving the greatest reductions in drug abuse and criminal behavior" (NIDA 2014, 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 Figure 9, panel A). 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, Figure 9, panel B also demonstrates that the quantity of treatment increases with time served, as inmates with longer sentences typically complete multiple programs.

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

C. *Recidivism and Labor Market Outcomes*

Table 6 presents the results of applying our identification strategy to outcomes measuring recidivism behavior and labor market performance for the first three

TABLE 6—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. Dependent variable = 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. Dependent variable = > 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. Dependent variable = 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. Dependent variable = November employment			
Treated	0.023	0.001	−0.004
	(0.008)	(0.009)	(0.008)
	0.144	0.162	0.179
Panel E. Dependent variable = earnings			
Treated	1,838.418	−432.25	−1,755.243
	(1,041.646)	(1,068.753)	(1,169.728)
	22,192	26,182	29,810
Observations	45,626	44,691	43,817

Notes: This table presents the effects of full exposure to the two-thirds reform on three mea-
sures of recidivism (any conviction, more than one conviction, and return to prison) measured
at 12, 24, and 36 months post release in panels A–C, respectively. 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 parentheses. The mean of the dependent variable is in italics.

years post release. Panels A–C consider three measures of recidivism: any con-
viction, 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. For example, exposure to the reform reduces
the chance of returning to prison within two years by almost 4 percent. Given
that these specifications necessitate conditioning on being alive and that we have
demonstrated that exposure to the reform reduces mortality, one should exercise
caution interpreting these estimates, since reform exposure can affect the compo-
sition of who survives.

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 (italicized) highlight the disadvantaged nature of this population: within
the first year of release, just 14 percent are employed in November and the average
labor market earnings in the first year after release are around 22,000 Swedish kro-
nor (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 percent) in the chance of being employed in the first post-release November.[35]

The first takeaway from these results is that reform exposure does not yield worse post-release behavior in terms of criminal activity or labor market participation. Moreover, there is evidence that individuals commit fewer crimes and are less likely to return to prison. This is consistent with the findings of Bhuller et al. (2020) that incarceration (at the extensive margin) in Norway reduces recidivism and with an increasing number of papers that reach similar conclusions.[36] While these results are important in their own right, the recidivism analysis can also speak to the mechanisms underlying the health effects of increased prison exposure.

One possible explanation is that the reduction in mortality is not driven by a direct improvement in health but indirectly, by the effect of more prison time on the post-release lifestyles and environments 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, neighborhoods, and living conditions. Yet the overall improvement in recidivism and short-term employment effects means that we cannot rule out this indirect channel yet.

To say something further about the importance of this mechanism, online Appendix Table 8 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 subsample. 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 with that for the mortality heterogeneity analysis (Table 5), suggesting that lifestyle improvement cannot be the only mechanism underlying the reform's health-improving effects.

VII. Conclusion

In stark contrast to previous correlational literature, we find that exposure to Sweden's early release reforms, 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 subsamples who are positively selected in terms of their criminal careers and connections to society. Moreover, even in the whole sample, increased exposure to prison reduces the chance of death due to causes that are particularly relevant for this high-risk population. Most prominently,

---

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

[36] See Kuziemko (2013) and Rose and Shem-Tov (2021), as well as Hjalmarsson (2009), who studies juveniles in Washington State. This is also consistent with Hinnerich, Pettersson-Lidbom, and Priks (2016), whose study of Swedish drunk driving sentences finds a reduction in post-release offenses 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- to 48-month sentences.

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 health-improving effects 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 health care and treatment in prison. We demonstrate that health care provision is high in Swedish prisons and, fundamental to its role as a mechanism, utilization and treatment increase with time served. Third, though there is some evidence that the reform improved post-release outcomes more generally (i.e., lower 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 among 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 US study to date can conclude that prison did not harm post-release health (Norris, Pecenco, and Weaver 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 health care and treatment program participation in prison play critical roles, 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 have even larger beneficial effects than those we find in the Swedish context.

REFERENCES

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey Wooldridge. 2017. "When Should You Adjust Standard Errors for Clustering?" NBER Working Paper No. 24003.

Aizer, Anna, and Joseph J. Doyle. 2015. "Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly-Assigned Judges." *Quarterly Journal of Economics* 130 (2): 759–803.

Aslim, Erkmen Giray, Murat Mungan, Carlos Navarro, and Han Yu. 2019. "The Effect of Public Health Insurance on Criminal Recidivism." Unpublished.

Bayer, Patrick, Randi Hjalmarsson, and David Pozen. 2009. "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections." *Quarterly Journal of Economics* 124 (1): 105–47.

Bhuller, Manudeep, Gordon B. Dahl, Katrine V. Løken, and Magne Mogstad. 2020. "Incarceration, Recidivism, and Employment." *Journal of Political Economy* 128 (4): 1269–1324.

Bjerk, David. 2005. "Making the Crime Fit the Penalty: The Role of Prosecutorial Discretion under Mandatory Minimum Sentencing." *Journal of Law and Economics* XLVIII: 591–625.

Bindler, Anna, and Randi Hjalmarsson. 2018. "How Punishment Severity Affects Jury Verdicts: Evidence from Two Natural Experiments." *American Economic Journal: Economic Policy* 10 (4): 36–78.

Binswanger, Ingrid A., Marc F. Stern, Richard A. Deyo, Patrick J. Heagerty, Allen Cheadle, Joann G. Elmore, and Thomas D. Koepsell. 2007. "Release from Prison—A High Risk of Death for Former Inmates." *New England Journal of Medicine* 356 (2): 157–65.

Bondurant, Samuel R., Jason M. Lindo, and Isaac D. Swensen. 2018. "Substance Abuse Treatment Centers and Local Crime." *Journal of Urban Economics* 104: 124–33.

Boylan, Richard T., and Naci Mocan. 2014. "Intended and Unintended Consequences of Prison Reform." *Journal of Law, Economics, and Organization* 30 (3): 558–86.

Bronson, Jennifer, and Marcus Berzofksky. 2017. *Indicators of Mental Health Problems Reported by Prisoners and Jail Inmates, 2011–12*. Washington, DC: US Department of Justice.

Bronson, Jennifer, Jessica Stroop, Stephanie Zimmer, and Marcus Berzofsky. 2017. *Drug Use, Dependence, and Abuse among State Prisoners and Jail Inmates*, 2007–2009. Washington, DC: US Department of Justice.

Burki, Talha. 2017. "Crisis in UK Prison Mental Health." *Lancet Psychiatry* 4 (12): 904.

Campaniello, Nadia, Theodoros M. Diasakos, and Giovanni Mastrobuoni. 2017. "Rationalizable Suicides: Evidence from Changes in Inmates' Expected Length of Sentence." *Journal of the European Economic Association* 15 (2): 388–428.

Di Tella, Rafael, and Ernesto Schargrodsky. 2013. "Criminal Recidivism after Prison and Electronic Monitoring." *Journal of Political Economy* 121 (1): 28–73.

Dobbie, Will, Hans Grönqvist, Susan Niknami, Mårten Palme, and Mikael Priks. 2018. "The Intergenerational Effects of Parental Incarceration." NBER Working Paper No. 24186.

Doleac, Jennifer L. 2018. "New Evidence that Access to Health Care Reduces Crime." Brookings. https://www.brookings.edu/blog/up-front/2018/01/03/new-evidence-that-access-to-health-care-reduces-crime/ (accessed August 14, 2019).

Durose, Matthew, and Patrick Langan. 2003. "Felony Sentences in State Courts, 2000." Bureau of Justice Statistics Bulletin NCJ 198821. U.S. Department of Justice.

Jácome, Elisa. 2020. "Mental Health and Criminal Involvement: Evidence from Losing Medicaid Eligibility." Unpublished.

Fazel, Seena, and Jacques Baillargeon. 2011. "The Health of Prisoners." *Lancet* 377 (9769): 956–65.

Goodman-Bacon, Andrew. 2020. "Difference-in-Differences with Variation in Treatment Timing." Unpublished.

Haglund, Axel, Dag Tidemalm, Jussi Jokinen, Niklas Långström, Paul Lichtenstein, Seena Fazel, and Bo Runeson. 2014. "Suicide after Release from Prison: A Population-Based Cohort Study from Sweden." *Journal of Clinical Psychiatry* 75 (10): 1047–53.

Hinnerich, Björn Tyrefors, Per Pettersson-Lidbom, and Mikael Priks. 2016. "Do Mild Sentences Deter Crime? Evidence Using a Regression-Discontinuity Design." Unpublished.

Hjalmarsson, Randi. 2009. "Juvenile Jails: A Path to the Straight and Narrow or Hardened Criminality." *Journal of Law and Economics* 52 (4): 779–809.

Hjalmarsson, Randi, and Matthew J. Lindquist. 2022. "Replication Data for: The Health Effects of Prison." American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. https://doi.org/10.3886/E141461V1.

Johnson, Rucker C., and Steven Raphael. 2009. "The Effects of Male Incarceration Dynamics on Acquired Immune Deficiency Syndrome Infection Rates among African American Women and Men." *Journal of Law and Economics* 52 (2): 251–93.

Jones, Mark, Gregory D. Kearney, Xiaohui Xu, Tammy Norwood, and Scott K. Proescholdbell. 2017. "Mortality Rates and Cause of Death among Former Prison Inmates in North Carolina." *North Carolina Medical Journal* 78 (4): 223–29.

Katz, Lawrence, Stephen D. Levitt, and Ellen Shustorovich. 2003. "Prison Conditions, Capital Punishment, and Deterrence." *American Law and Economic Review* 5 (2): 318–43.

Krueger, Kem P., Bruce A. Berger, and Bill Felkey. 2005. "Medication Adherence and Persistence: A Comprehensive Review." *Advances in Therapy* 22 (4): 313–56.

Kuziemko, Ilyana. 2013. "How Should Inmates be Released from Prison? An Assessment of Parole Versus Fixed-Sentence Regimes." *Quarterly Journal of Economics* 128 (1): 371–424.

Landersø, Rasmus. 2015. "Does Incarceration Length Affect Labor Market Outcomes?" *Journal of Law and Economics* 58 (1): 205–34.

Massoglia, Michael. 2008. "Incarceration as Exposure: The Prison, Infectious Disease, and Other Stress-Related Illnesses." *Journal of Health and Social Behavior* 49 (1): 56–71.

Mueller-Smith, Michael. 2015. "The Criminal and Labor Market Impacts of Incarceration." Unpublished.

National Institute on Drug Abuse. 2014. "Principles of Drug Abuse Treatment for Criminal Justice Populations: A Research-Based Guide." NIH Publication No. 11-5316, April 2014.

Norris, Samuel, Matthew Pecenco, and Jeffrey Weaver. 2020. "The Effect of Incarceration on Mortality." Unpublished.

Patterson, Evelyn. 2013. "The Dose-Response of Time Served in Prison on Mortality: New York State, 1989–2003." *American Journal of Public Health* 103 (3): 523–28.

Piquero, Alex R., David P. Farrington, Jonathan P. Shepherd, and Katherine Auty. 2014. "Offending and Early Death in the Cambridge Study in Delinquent Development." *Justice Quarterly* 31 (3): 445–72.

Pew Charitable Trusts. 2017. *Prison Health Care: Costs and Quality*. Philadelphia, PA: Pew Charitable Trusts.

Porter, Lauren C. 2014. "Incarceration and Post-Release Health Behavior." *Journal of Health and Social Behavior* 55 (2): 234–49.

Pratt, John. 2008. "Scandinavian Exceptionalism in an Era of Penal Excess. Part I: The Nature and Roots of Scandinavian Exceptionalism." *British Journal of Criminology* 48 (2): 119–37.

Raphael, Steven, and Michael A. Stoll. 2013. "Assessing the Contribution of the Deinstitutionalization of the Mentally Ill to Growth in the US Incarceration Rate." *Journal of Legal Studies* 42 (1): 187–222.

Roach, Michael A., and Max M. Schanzenbach. 2015. "The Effect of Prison Sentence Length on Recidivism: Evidence from Random Judicial Assignment." Unpublished.

Rose, Evan K., and Yotam Shem-Tov. 2021. "How Does Incarceration Affect Reoffending? Estimating the Dose-Response Function." *Journal of Political Economy* 129 (12): 3302–56.

Sailas, Eila S., Benjamin Feodoroff, Nina C. Lindberg, Matti E. Virkkunen, Reijo Sund, and Kristian Wahlbeck. 2005. "The Mortality of Young Offenders Sentenced to Prison and Its Association with Psychiatric Disorders: A Register Study." *European Journal of Public Health* 16 (2): 193–97.

Schnepel, Kevin T. 2018. "Good Jobs and Recidivism." *Economic Journal* 128 (608): 447–69.

Schnittker, Jason, and Andrea John. 2007. "Enduring Stigma: The Long-Term Effects of Incarceration on Health." *Journal of Health and Social Behavior* 48 (2): 115–30.

Skardhamar Torbjørn, and Vegard Skirbekk. 2013. "Relative Mortality among Criminals in Norway and the Relation to Drug and Alcohol Related Offenses." *PLoS ONE* 8 (11): e78893.

Starr, Sonja B., and M. Marit Rehavi. 2013. "Mandatory Sentencing and Racial Disparity: Assessing the Role of Prosecutors and the Effects of Booker." *Yale Law Journal* 123 (1): 2–80.

Statistics Sweden (Statistiska centralbyrån). 2017. Select Socioeconomic Variables from National Registers. https://www.scb.se/vara-tjanster/bestall-data-och-statistik/bestalla-mikrodata/.

Sweden's National Board of Health and Welfare (Socialstyrelsen). 2017. Cause of Death Register (Dödsorsakregisteret), 1992–2016. https://www.socialstyrelsen.se/statistik-och-data/register/dodsorsaksregistret/.

Sweden's National Board of Health and Welfare (Socialstyrelsen). 2017. Inpatient Hospitalization Register (Patientregisteret), 1987–2016. https://www.socialstyrelsen.se/statistik-och-data/register/patientregistret/.

Sweden's National Council for Crime Prevention (Brottsförebyggande rådet). 2018. National Convictions Register (Lagföringsregsitret), 1973–2016. https://bra.se/statistik/specialbestallningar.html.

Swedish Prison and Probation Service (Kriminalvården). 2018. Swedish Prison Admissions and Follow-Up Data, 1992–2013. https://www.kriminalvarden.se/forskning-och-statistik/statistik-och-fakta/.

Swedish Prison and Probation Service (Kriminalvården). 2019. "Ökat Klientflöde: Kriminalvårdens Samlade Bedömning och Förslag till Åtgärder." Kriminalvårdens Digitaltryck.

Turney, Kristen, Christopher Wildeman, and Jason Schnittker. 2012. "As Fathers and Felons: Explaining the Effects of Current and Recent Incarceration on Major Depression." *Journal of Health and Social Behavior* 53 (4): 465–81.

Ulmer, Jeffrey T., Megan C. Kurlychek, and John H. Kramer. 2007. "Prosecutorial Discretion and the Imposition of Mandatory Minimum Sentences." *Journal of Research in Crime and Delinquency* 44 (4): 427–58.

Ugelvik, Thomas, and Jane Dullum. 2012. *Penal Exceptionalism? Nordic Prison Policy and Practice.* New York: Routledge.

US Department of Justice. 2012. "Psychiatric Medication Adherence among People Who Are Incarcerated: What Do We Know?" *Corrections and Mental Health: An Update of the National Institute of Corrections* 1 (1): 1–4.

Vogler, Jacob. 2017. "Access to Health Care and Criminal Behavior: Short-Run Evidence from the ACA Medicaid Expansions." Unpublished.

Wen, Hefei, Jason M. Hockenberry, and Janet R. Cummings. 2017. "The Effect of Medicaid Expansion on Crime Reduction: Evidence from HIFA-Waiver Expansions." *Journal of Public Economics* 154: 67–94.

This article has been cited by:

1. Yuki Otsu, C.Y. Kelvin Yuen. 2022. Health, crime, and the labor market: Theory and policy analysis. *Journal of Economic Dynamics and Control* **144**, 104529. [Crossref]