The Health Effects of Prison*

Randi Hjalmarsson†
University of Gothenburg and CEPR

Matthew J. Lindquist††
SOFI, Stockholm University

March 19, 2020

Abstract

This paper analyzes the impact of longer prison exposure on post-release mortality by taking advantage of two Swedish early release reforms in 1993 and 1999 that held prison sentences constant but increased the share of time inmates were required to serve from one-half to two-thirds. Contrary to popular belief and previous correlational evidence, we find that more prison exposure reduces the overall risk of death, especially for property and young offenders. We also find (i) a significant and persistent reduction in the chance of suicide, (ii) a short-term reduction in violent death, and (iii) a long-term improvement in general health (circulatory death). These cause-specific effects are driven by particularly vulnerable populations – individuals with pre-incarceration mental health problems, violent offenders, and older offenders, respectively. We consider two channels through which prison may affect post-release mortality: direct via in-prison health treatment and services, and indirect via post-release lifestyle changes. With regards to the former, we demonstrate a high rate of healthcare provision and utilization in Swedish prisons. With regards to the latter, we find that more time in prison decreases recidivism. Heterogeneity analyses, however, suggest that this indirect channel cannot explain all of the observed health improvements.

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

* We would like to thank Anna Bindler, Andreea Mitrut, Analisa Packham, Dan-Olof Rooth, Jan Sauermann, Anders Stenberg, and seminar participants at Gothenburg University, Stockholm University, LSE/CEP, University of Dublin, the Texas Economics of Crime Workshop 2020, and the Swedish Prison and Probation Service for their helpful comments and suggestions. Hjalmarsson gratefully acknowledges funding support from Vetenskapsrådet (The Swedish Research Council), Grants for Distinguished Young Researchers. Lindquist would also like to gratefully acknowledge funding support from Vetenskapsrådet (The Swedish Research Council).
† University of Gothenburg, Department of Economics, Vasagatan 1, SE 405 30, Gothenburg, Sweden; randi.hjalmarsson@economics.gu.se
†† Swedish Institute for Social Research (SOFI), Stockholm University, Universitetsvägen 10F, 106 91 Stockholm, Sweden; matthew.lindquist@sofi.su.se

1. Introduction

Decades of research paint a very clear and consistently bleak picture of the health and well-being of current and former prison inmates.[1] Former prisoners around the world have much higher death rates from natural and unnatural causes than the general population (e.g. Binswanger et al, 2007; Skardhamar et al, 2013; and Jones et al, 2017). These mortality risks are even larger with more time in prison: a recent study of New York State parolees finds that each additional prison year translates into a 2-year decline in life expectancy (Patterson, 2013).[2]

The positive correlation between prison exposure and mortality risk is perhaps not surprising given that prisoners are negatively selected along many dimensions that are also related to mortality, including socioeconomic status and pre-incarceration health. For example, about 50% of U.S. prisoners have a known history of mental health problems, while 60% meet the criteria for drug dependence or abuse (Bronson and Berzofsky, 2017; Bronson et al., 2017).[3] Importantly, the correlational nature of this literature leaves a number of key policy relevant questions unanswered. What share of the prison-health correlation (if any) is due to the *causal* impact of prison on health? Must prison be harmful to one's health?

We address these causal questions by taking advantage of Sweden's 1993 and 1999 early release reforms, which held sentences constant, but changed the share of time inmates were required to serve from 50% to 67%. Exposure to these new policies 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. Our main analysis uses exogenous variation in time served generated by these reforms to estimate the effect of longer prison exposure on post-release mortality overall as well as by the main natural (circulatory, digestive, and cancer) and unnatural (suicide, violent death, and drugs and/or alcohol) causes of death. We also report results for recidivism.

Identifying whether prison has a causal impact on offender health is of first-order importance. First, there is extensive evidence documenting the relative disadvantage of the prison population. Does prison exacerbate or mitigate this relative disadvantage? The World

[1] See e.g. the review article by Fazel and Baillargeon (2011).

[2] See also Piquero et al. (2014) for an overview of the offender-mortality literature.

[3] Similarly, more than 60% of U.K. prisoners suffer from personality disorders and 50% from depression or anxiety (Burkhi, 2017); more than 50% of Swedish prisoners had been previously diagnosed with a psychiatric disorder, most commonly a substance abuse disorder (Haglund et al., 2014). Swedish prisoners also have much worse self-reported health when compared with an appropriate group of non-prisoners (Nilsson, 2002). For example, on a five point scale ranging from very bad to very good, Swedish inmates are much more likely to rate their overall health condition as very bad and much less likely to say that it is very good; 37% of inmates report having a long-term illness and 49% report having psychological problems, while only 11% and 8% of the non-incarcerated population report having these same health problems. Also see Fazel and Baillargeon's (2011) review article.

Health Organization's European health framework, Health 2020, highlights the particular needs of the prison population. They argue that increasing healthcare provision for this group is both an effective way of reducing health inequality and a means for improving public health overall, including the health of non-inmates (WHO 2014).

Second, there is a growing literature highlighting the challenges faced by former prisoners in desisting from crime and reintegrating into society upon release (DuRose et al., 2014; Doleac, 2019). Taken together with the theoretical role of human capital (in this case, health) in determining crime participation (Becker, 1968) and the increasing empirical evidence that health care access can causally reduce crime, this suggests that the ability of ex-prisoners to desist from crime could depend, in part, on the health effects of prison.[4]

Third, healthcare comprises a significant component of prison budgets: about 20% of U.S. prison expenditures – more than $8 billion – in 2015 were on healthcare (Pew Charitable Trusts, 2017). Yet, we know very little about the individual and social returns to these healthcare expenditures? Are there substantial gains to be made from spending more on prison healthcare?

Finally, as a substantial share of prisoners have minor children – more than 50% in the U.S. (Glaze and Maruschak, 2008) – parental incarceration can potentially have second-generation impacts via parental health.[5] In the extreme, if prison affects mortality, then the economics literature on early parental death suggests potentially important second-generation consequences (e.g. Gertler et al., (2004) and Lang and Zagorski (2001)).

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, exposure to a violent and stressful environment, poor nutrition, and low (or no) access to proper healthcare. But they could also be positive if inmates are kept sober and drug free or receive health care that they either could not afford or did not seek out when not in prison. Health screening upon intake can also identify previously unknown illnesses and lead to treatment.

Post-release health can be directly affected by the persistence of these contemporaneous effects or indirectly via the impact of prison on recidivism, labor market outcomes, and social connections. The financial and emotional strain associated with worse outcomes can translate into poor health outcomes and behaviors, including: high blood pressure, depression, anxiety,

[4] See Doleac (2018) for a popular science review. Bondurant et al. (2018) find that expanding access to substance abuse treatment facilities reduce local violent and financially motivated crimes. Using various expansions to Medicaid coverage, Wen et al. (2017), Vogler (2017), and Aslim et al. (2019) all find evidence of decreases crime (or recidivism) behavior; some argue that the results are driven by increased access to substance abuse treatment.
[5] Dobbie et al. (2018) study the effect of parental incarceration in Sweden on child outcomes, while Bhuller et al. (2018) and Norris et al. (2019) study the Norwegian and U.S. contexts, respectively.

poor eating, smoking, alcohol or substance abuse. Given the potential opposing channels, the effect of prison on offender health is clearly an empirical question.

Because individuals who receive harsher punishments tend to systematically differ on observable and unobservable dimensions that are correlated with post-release health, simply controlling for observables will not be sufficient to disentangle correlation from causation. Yet, this is the current state of the literature: incarceration is associated with worse health outcomes, including health problems that prevent work (Schnittker and John, 2007), depression (Turney et al., 2012), poor health behaviors like fast food consumption and smoking (Porter, 2014), stress-related illnesses and infectious diseases (Massoglia, 2008), and higher mortality rates (Sailas et al., 2005; Haglund et al., 2014).

In contrast, many papers study the causal effect of incarceration on non-health outcomes, like recidivism and employment, using exogenous variation from both random judge assignment and natural experiments based on sentencing guidelines or reforms.[6, 7] Even when using the same research design, the results are mixed. With random judges, longer prison sentences (and more time served) have been found to result in no effect on employment and earnings in Florida and California (Kling, 2006), less crime in Seattle (Roach and Schanzenbach, 2015), and worse recidivism and labor market outcomes in Texas (Mueller-Smith, 2015).[8] Three papers with alternative research designs find that longer prison sentences improve outcomes. Using discontinuities in Georgia's parole board guidelines, Kuziemko (2013) finds that one more month in prison reduces the 3-year chance of recidivism by 1.3 percentage points. Landerso (2015) finds that increasing violent offender incarceration lengths by one month (or about 50%) reduces unemployment and increases earnings. Using discontinuities in North Carolina's sentencing guidelines, Rose and Shem-Tov (2019) also find reduced recidivism, but that the effect diminishes as sentences get longer. The conflicting

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

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

[8] Most judge random assignment papers study the extensive margin of incarceration, as sentence intensity tends not to vary as much across judges. At the extensive margin, Aizer and Doyle (2015) and Di Tella and Schargrodsky (2013) find harmful effects of incarceration in Chicago (for juveniles) and Argentina respectively, while Bhuller et al. (forthcoming) find a reduction in recidivism and better labor market outcomes. Dobbie et al. (2018) use the harsh judge instrument in Swedish administrative data; though the emphasis of this paper is on child outcomes, they find little effect of incarceration on the incarcerated individual's recidivism.

findings of these studies are likely driven, in part, by institutional differences in prison conditions and experiences across countries; one such factor that may be relevant, but has not been discussed, is the role of prison healthcare and the post-release effect of prison on health.

Sweden's early release reforms provide a unique source of identification: they increase time spent in prison without impacting the actual sentence length. Moreover, there is variation across sentences of different lengths (measured in months) both in the timing of reform exposure and the size of the shocks. Thus, one can visualize our research design as taking advantage of a series of discontinuities of varying magnitudes, which occur at two dates and across sentencing bins (where each 'bin' contains all sentences that can be rounded down to the same number of months). The 'first-stage' impact of the reform is such that a 17 percentage point increase (from one-half to two-thirds) in the prescribed share of time served implies an average increase of 46 days served in prison.

Using matched Swedish register data, our baseline sample includes nearly 47,000 prison sentences of 4-48 months, for convictions between 1991 and 2001. These prisoners are as negatively selected in terms of their health as prisoners around the world: more than 20% had an overnight stay in a psychiatric ward in the five years prior to prison. And recidivism rates are comparably high: more than 50% are re-incarcerated within 12 months.

In stark contrast to previous correlational literature, our reduced form analysis finds that increases in the share of time served prescribed by the law within each sentence month bin improves prisoner post-release health along multiple dimensions. Though the reduction in mortality risk for the entire sample is not quite significant, we do see large and significant reductions for certain populations, including relatively young offenders and property offenders.

Significant effects are also seen when zooming in on specific causes of death that are particularly relevant for this population. First, there is a significant and immediate reduction in the chance of suicide, which persists in the long-run. Put simply, the improvement in mental health due to these reforms is large: increasing the share of time served by 17 percentage points reduces the chance of suicide by more than 70% in the three years following release, and almost 40% ten years after release. These suicide results are especially driven by the high-risk populations of individuals with previously identified mental health issues and violent offenders. Extended prison exposure does not just improve mental health. A significant reduction in violent death is seen in the months immediately following release; it is stronger and lasts until about three years out for the high risk population of violent offenders. Finally, there are significant improvements in medium and long-run general health, as measured by a reduction in the chance of death due to circulatory disease, digestive disease, and cancer. The typical

exposure to the reform reduces the 10-year post release chance of death due to these causes by 27%. These results are especially driven by circulatory diseases and the population of relatively older prisoners, for whom these diseases are more prevalent.

The bottom-line is that extended prison exposure in Sweden improves general and mental health. To help explain this important result, we consider two mechanisms: the direct mechanism of access to healthcare and treatment programs while in prison, and an indirect (healthy life-style) mechanism due to the potential effect of the reform on recidivism. With respect to the former, we conduct an out-of-sample analysis of the healthcare utilization of inmates sentenced to 4 to 48 months from 2009 to 2013 (cohorts for which we have detailed healthcare data while in the prison, but who are not directly affected by the reforms). We find that, indeed, Swedish prisoners have an extremely high in-prison rate of meeting doctors, nurses, and psychologists, and of receiving medication and participating in treatment programs. We also find that healthcare utilization in prison is at least as large, and in many dimensions larger, than that which the same individuals take-up in the two years prior to incarceration. Importantly, many of the treatment programs offered to inmates take time to complete and the probability of completing these programs rises dramatically with more time spent in prison.

With respect to the second mechanism, we assess whether extended prison exposure improved the post-release environment, including less participation in a criminal life-style. We do indeed find that the reform significantly decreases the likelihood of returning to prison: Serving an additional 46 days in prison (the average effect of the reform) would decrease the chance of returning to prison within two years by almost 4%. But, we argue that the indirect recidivism channel is unlikely to explain most of the observed health improvements because heterogeneity analyses for the two sets of outcomes are not aligned: the same sub-samples are not driving both the mortality and recidivism results. The single exception is older property offenders, who tend to both live longer and recidivate less after spending more time in prison.

The remainder of the paper proceeds as follows. Sections 2 and 3 describe Sweden's early release reforms and detail how we capitalize on the resulting exogenous variation in reduced form and instrumental variable estimation strategies. Section 4 describes the data. Section 5 studies the implementation of the reform. Section 6 presents both the reduced form and IV results. Mechanisms are discussed in Section 7 and Section 8 concludes.

2. Institutional Background

2.1. Sweden's Early Release Policy Reforms

The Swedish Prison and Probation Service (*Kriminalvården* in Swedish) has had an early release and probation system in place since 1906 (proposition 1906:49). It aims to help inmates reintegrate into life outside of prison and prevent recidivism by giving the prison authorities a period of time after the inmate's release during which they can make explicit demands (e.g. contacts with a probation officer, enrollment in substance abuse programs, or active job search).

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

The rules for early release changed on July 1, 1993 (proposition 1992/93:4).[9] The new rules stated that all prisoners sentenced to 4-24 months in prison would be required to serve two-thirds of their sentence. Those with more than 24-months could still be released after serving half of their sentences. To avoid threshold effects, a graduated scale was applied in practice for those serving 13 – 24 months (SOU 2005:54).[10]

On January 1, 1999 the early release and probation law changed again, such that all prisoners sentenced to more than 1-month were required to serve two-thirds of their sentence (proposition 1997/98:96).[11] Although the law still stated that early release was at the discretion of the parole board, in practice the Swedish Prison and Probation Service applied the two-thirds rule quite strictly with few and only minor deviations, regardless of inmate behavior and/or characteristics. At this time, a serious infraction of prison rules could lead to a delay in early release of at most 15 days (per infraction) and these delays were used quite sparingly.

The rules regarding the amount of time on post-release probation were held constant.[12] 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 on probation are "passive", with few or no demands placed on former inmates (given that the first year of probation was completed satisfactorily). Thus,

[9] 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.

[10] The graduated scale is stated explicitly in proposition 1992/93:4. Those with 13-24 month sentences should serve 8 months plus one-third of the time exceeding one year. For example, a 24 month sentence results in 8 + (12/3) = 12 months served, i.e. 50% of the full sentence, while an 18 month sentence results in 8 + (6/3) = 10 months served (56%).

[11] The first formal motion was filed in March 1998 and was passed by the Swedish Parliament on June 3, 1998.

[12] We use the term 'post-release probation' to refer to 'prövotid i samband med villkorlig frigivning' in Swedish.

an increase in the share of time served from one-half to two-thirds does not change the number of months on active probation to which former inmates are subject. Those sentenced to more than 24 months will, however, spend less time on passive probation after the 1999 reform.[13]

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 Appendix Table 1. Changes in these shares, over time and within different sentencing lengths, lie at the heart of our identification strategy.

The Swedish constitution prohibits the application of new sentencing laws retroactively unless they benefit the new inmate. Thus, the new early release policies, which increased the share of time served, were to be applied to people convicted (and entering prison) after the implementation date of each reform. The only exception is the reduction in share of time served from 100% to 67% for those sentenced to 2-months around the 1999 reform. We do not, however, study the impact of this reduction, given that there were other criminal justice reforms, described in Section 5.4, which simultaneously affected those sentenced to less than 4-months.

2.2. Prisons in Sweden

Over the last 30 years, Sweden's incarceration rate has fluctuated between a high of 79 (in 2006) and low of 53 (in 1985) inmates per 100,000 persons, which is roughly 25% lower than the Western European average and 10 times lower than the US. (See Panel A of Appendix Figure 1). Sentences are also shorter in Sweden; 84% of inmates convicted between 1991 and 2001 had sentences shorter than one year, with an average time spent in prison of 4.7 months. The average time served was about 8 months in Western Europe in 2001 (European Council 2002) and more than 30 months in U.S. state and federal prisons (BJS 2001a, 2001b).[14]

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

[13] In July 2020, a new, more active probation system will become law. Under the new regime, those with longer sentences will be required to remain on active probation until the entirety of their sentence is served.
[14] Since the U.S. estimates from the National Corrections Reporting Program Series and the Federal Criminal Case Processing Statistics exclude short jail sentences, the U.S. vs. Europe difference may be somewhat exaggerated.

maintained by building six new prisons and through the expansion of several existing prisons.[15] Average prison size across the entire sample period studied was 85 inmates. Swedish prisons, and prison healthcare in particular, will be discussed further in Sections 6.5 and 7.1.

3. Empirical Strategy

The main goal of this paper is to identify the effect of more time served in prison (measured in days) on individual *i's* post-release outcomes *Y* measured *m* months after release.

$$(1) \quad Y_i^m = \propto + \beta PrisonDays_i + \varepsilon_i$$

This section explains why simple OLS estimates of equation (1) will be biased, and even why biases will remain when controlling for a large set of observables as well as the actual assigned sentence. We then turn to our identification strategy, which relies on early release law reforms to isolate exogenous variation in prison days served.

3.1. OLS Analysis and Motivation for Identification Strategy

Table 1 presents the results of estimating equation (1) for three sub-samples of our data during which there is no variation in the early release laws (pre-1993 reform, between the 1993 and 1999 reforms, and post-1999 reform) and two outcomes (returning to prison and death) measured 3-years post-release. In columns (1) and (5), three of the raw associations are significantly positive, two are significantly negative, and one is zero. The pre-1993 sample echoes the correlational literature most clearly with a positive and significant association between the time spent in prison and the probability of recidivating and dying prematurely.

In columns (2) and (6), we re-estimate these associations using only those sentenced from 4 to 48 months (our estimation sample). Significant negative relationships are seen for almost every sample and outcome: more time spent in Swedish prisons is associated with a lower chance of recidivism and death. This negative raw relationship may be surprising. But, it could of course simply be an artefact of there being higher recidivism rates for those more minor drug/alcohol and property offenses that have shorter sentences. In other words, the number of days an individual serves is a function of many factors, including: early release laws, case and criminal history characteristics, demographics (e.g. age) and socioeconomic status (education

[15] Today, there are 45 prisons; 12 open facilities, 4 mixed (open and medium security), 22 medium security facilities, and 7 high security facilities. Security class and prison assignment are based on (i) crime severity, (ii) escape risk, (iii) gender, (iv) age 18 to 21, (v) rehabilitative needs, and (vi) family ties. As authorities strive for placements in lower security prisons when feasible and appropriate, it is common for minor offenders to wait up to 9 months before starting their sentences.

and employment), amount of time spent in pre-trial detention, and time-served extensions due to miss-behavior. Since many of these factors are also related to the post-release recidivism or health outcomes, these simple regressions are likely to yield biased estimates of the effect of prison days served.

Columns (3) and (7) of Table 1 therefore control for all criminal justice characteristics that are observable, as well as a large set of controls for demographic characteristics, pre-incarceration labor market exposure, hospitalization, income, education, and family status.[16] To account for crime trends over time, we control (as in our baseline specification) for year of conviction fixed effects and a conviction month trend. The relationship between prison days served and both recidivism and health is now at least an order of magnitude smaller (closer to zero) in almost every sample, and even begins to switch sign (for a second time in some cases).

Yet, there are still potentially unobservable determinants of time served that are related to recidivism and health. Thus, columns (4) and (8) take these regressions a step further, and control for prison sentence month bin fixed effects. This essentially controls for any unobservables that result in individuals getting the same sentence, and identifies the effect of time served off of a comparison of individuals with the same approximate sentence (e.g. 30-59 days) but different time served. Why do individuals with the same sentence serve different amounts of time, during a sample period with no early release reforms? Three potential explanations are time served in pre-trial detention, extensions due to poor behavior, and additional weeks added to the prison sentence due to a probation violation that must be served in full – three factors that are also likely to be associated with worse criminal behavior. Not surprisingly, a positive relationship is now seen between recidivism and prison days served.

3.2. The Reduced Form Impact of the Early Release Reforms

Our identification strategy aims to isolate that variation in time served that is exogenously determined by early release reform exposure rather than the endogenous sources of variation described above. Specifically, the main analysis estimates the reduced form impact of being 'treated' by the early release reform. The underlying intuition is that we compare individuals with the same sentence (and offense characteristics), but who serve different amounts of time in prison because they are convicted before or after the reform. Thus, our strategy takes advantage of one of the distinguishing features of the early release reforms: they affect the share of time spent in prison without impacting the actual sentence length.

[16] Criminal justice controls include detailed current offense dummies, criminal history characteristics, number of charges, court fixed effects, and conviction month of year fixed effects.

Equation (2) presents the reduced form specification, where *EarlyReleaseLaw* is the share of the sentence (between 0 and 1) an individual should serve if s/he is convicted at date *t* and sentenced to *s* months (ranging from 4 to 48). It varies with the conviction date (pre-1993, between 1993 and 1999, and post-1999 reforms) and length of sentence (months). Not all sentence length bins are affected by both reforms and the size of treatment varies across bins. Appendix Table 1 displays the values of *EarlyReleaseLaw* across all sentence bins and periods.

$$(2) \quad Y_{its}^{m} = \delta EarlyReleaseLaw_{ts} + \alpha_s + ConvMonth_{trend} + ConvYear_{fe} + X_i\theta + \varepsilon_{its}$$

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

To the extent that there are trends over time in criminal justice or prison related policies, we include year of conviction fixed effects, $ConvYear_{fe}$, and a linear time trend in the month of conviction (where January 1991 is 1) in our baseline, $ConvMonth_{trend}$. The former makes intuitive sense if one thinks of the reduced form as a differences-in-differences specification, where we want to control for any other shocks common to sentence month bins that are both treated and untreated by the reforms. The latter makes intuitive sense if one had decided to model the reduced form as a regression discontinuity design, with date of conviction as the running variable. To some extent, one can imagine our strategy as taking advantage of a series of discontinuities of varying magnitudes, which occur at two dates and across sentencing bins.

We return to the two necessary identifying assumptions underlying the reduced form analysis in Section 5. 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 assessing this, we also demonstrate the 'relevance' of the reform as an instrument for prison days served. The second is conditional independence, i.e. that exposure to the reform should be unrelated to individual characteristics.

3.3. Using the Reforms as an Instrument for Prison Days Served

We then use our *EarlyReleaseLaw* variable as an instrument for days spent in prison. Equations (3) and (4) present the first and second stage equations, respectively, of our two-stage least

squares specification, where $PrisonDays_{its}$ equals the actual number of days *served* in prison for individual *i* convicted on date *t* and sentenced to sentencing month bin *s*.

$$(3)\quad PrisonDays_{its} = \gamma EarlyReleaseLaw_{ts} + \alpha_s + ConvMonth_{trend} + ConvYear_{fe} + X_i\delta + \epsilon_{its}$$

$$(4)\quad Y_{its}^{m} = \beta \widehat{PrisonDays}_{its} + \alpha_s + ConvMonth_{trend} + ConvYear_{fe} + X_i\theta + \varepsilon_{its}$$

For β to represent the causal effect of an additional day in prison on outcome, *Y*, two additional identifying assumptions must be satisfied: monotonicity and the exclusion restriction. The latter implies that the reduced form impact of the reform on our outcomes of interest is only driven by the first-order impact of the early release reforms on prison days served, and not for instance by any other effects of the reform on prison experiences, e.g. overcrowding, peers, facility assignment, treatment plans, etc., to the extent that they occurred. We provide evidence in support of monotonicity in Section 5 and the exclusion restriction in Section 6.5.

3.4. Empirical Implementation

Finally, we note four features of the empirical implementation common to both our reduced form and IV analyses. First, in accordance with how the treatment is defined, we cluster standard errors at the sentence month bin level. Second, our analysis capitalizes on two policy reforms, which increase the share of time served for shorter and then longer sentences, respectively. Most of our analysis is conducted using the entire period (i.e. spanning both reforms), thereby estimating the effect of increasing the share of time served for all 4-48 month sentences. Heterogeneity analyses consider the 1993 and 1999 reforms separately to assess whether the effects vary for shorter and longer sentences. Third, we allow individuals to be *at risk* as of the release date; i.e., we measure all outcomes in terms of months since release.[17] Finally, our analysis traces 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).

[17] The appropriate date at which to measure 'at risk' is often debated in the literature, especially when studying recidivism: should the at-risk date be conviction (in which case one has to disentangle incapacitation from deterrence) or release (which leads to concerns about biases arising from the age-crime profile)? This issue is discussed extensively, for instance, in Rose and Shem-Tov (2019). Given our emphasis on health outcomes rather than recidivism, we are less subject to the latter concern; we thus use date of release as our baseline.

4. Data

4.1. Data Description

We begin constructing our sample using data from the Swedish Prison and Probation Service covering all individuals who entered the prison system 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] The prison data also include information on citizenship, transfers to and from foreign prisons, death in prison, and post-trial detention. Prison data can be matched to other national registers using the personal identification number that each Swedish resident has (including foreign inmates).

We match our prison data to the official convictions register (*lagföringsregistret* in Swedish) maintained by the Swedish National Council for Crime Prevention. The conviction data cover the years 1973 to 2016 and include information on offense and conviction dates, crime types, and sanctions. As we demonstrate below, knowing both the conviction date and start date of a prison sentence is crucial to correctly assign treatment status. We also use the conviction data to create measures of past criminal offenses and prison sentences, current offense characteristics, and measures of post-release recidivism and incarceration.

Our main health outcome of interest is mortality. Mortality data come from the Swedish National Board of Health and Welfare's cause of death register (*dödsorsaksregistret* in Swedish). We study all-cause mortality and cause-specific mortality. The main cause of death is classified using mutually exclusive ICD10 codes for: (i) suicide, (ii) intentional violence, (iii) cancer, (iv) circulatory disease, and (v) digestive disease. Much of our analysis combines these latter three categories together, and labels them CCD. In each case, the coroner also notes whether the death was alcohol and/or narcotics related. For example, a narcotics induced heart attack would be coded as both 'circulatory disease' and 'narcotics related'. Hence, the main cause of death is not mutually exclusive from the alcohol and/or narcotics classification. We therefore consider as an additional outcome an alcohol or narcotics flagged death that is not otherwise classified as one of our primary ICD10 codes. As the date of death variable is incomplete in this register, we use the death date provided by Statistics Sweden.

[18] Data on the number of days in pre-trial detention do not exist. So our measure of the share of a prison sentence actually served will almost always lie slightly below what the law prescribes, since time in pre-trial detention is subtracted from the amount of time an individual must serve. Although there is some seasonal variation, the use of pre-trial detention remains constant across years and reforms. This can be seen in Figure 4.

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

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

4.2. The Analysis Sample

To treat both reforms in a reasonably symmetric manner, we focus on sentences that start between 1992 and 2001. Appendix Table 2 shows the number of observations dropped due to each sample restriction for both the whole sample (N = 108,439) and the analysis sub-sample of sentences greater than 3 months (N = 57,310). We are left with 86,109 sentences of 0-48 months, and 46,815 in the main analysis sample of 4-48 months.

We first match prison sentences to conviction dates by searching for an individual's last conviction before the prison start date that included a prison sentence. For about 98% of the sample, we can identify such a date. About half of the sample starts their sentence within 3 months of conviction, 90% within 400 days, and 97% within two years. While large differences between conviction and start dates could theoretically exist (e.g. due to an extended appeal), such cases can also represent measurement error in our matching process. We therefore drop those with a more than two-year lag until starting prison (yielding N = 102,762).

We drop about 2,000 sentences longer than 48 months, which are too scarce to analyze. We also omit 467 juveniles (start, conviction, or offense occurred before age 18), mostly from 0-3 month sentence bins, who face different sentencing laws and/or facilities. Individuals who both start and end their sentence in post-trial detention (i.e. a temporary placement) are also excluded, as share time served laws would not apply (60% have sentences of 3 months or less).

We drop individuals for whom 'treatment' is uncertain (N=8,691). Though time-served should be determined by the pre-reform conviction date, we demonstrate in Section 5.2 that at least a sub-set of individuals who were convicted before but started their sentence after each reform were treated using the start date. Including individuals whose conviction and start dates 'straddle' the reforms would lead to measurement error in assigning treatment. We conduct sensitivity analyses to including these individuals as either treated or control sentences.

Finally, we drop individuals for whom days served would be unaffected by the reform because they: (i) had a life sentence (N=6), (ii) died in prison (N =71), or (iii) were sent to a foreign prison (N =149). We also exclude individuals who serve more than 110% of their sentence (N =982) or less than 10% (N =246). The former could occur, for instance, due to misbehavior related sentence extensions and to how strictly probation revocations were enforced. The latter are primarily due to time served in pre-trial detention (for which we have no data).[19] We provide sensitivity analyses to these sample restrictions in the appendix.

Appendix Table 3 shows the distribution of sentences across bins for the entire sample and those with conviction dates within two years of each reform. For the 1993 and 1999 reform samples, 'treated' bins are shaded grey. For the 1993 reform, there are 19,130 sentences of 4-48 months, and 16,829 are in the treated bins of 4-24 months. But, for the 1999 reform, which targets relative longer sentences, a smaller proportion of sentences are treated (4,850 of 16,930).

4.3. Summary Statistics

Table 2 provides summary statistics for the main analysis sample of 4-48 month sentences (N = 46,815), for which the average number of months (days) sentenced is 11.7 (354). For comparison purposes, Table 2 also provides summary statistics for 0-3 month sentences. Different types of offenses lead to longer sentences; 37% of the 0-3 month sample is driving under the influence (dui) offenders (primarily from the early 1990s) and 24% and 20% are charged with violent and property offenses, respectively. In contrast, just 3% of the analysis sample is charged with dui, while 33%, 43%, and 15% are convicted of violent, property, and drug and alcohol offenses, respectively. The 4-48 month sample appears to be negatively selected in terms of criminal history and some observables, including pre-incarceration employment, average income and health. Given our emphasis on post-release health, the pre-incarceration health of our sample is clearly of interest. The only variable available during this period is hospitalization, which likely captures a combination of both pre-incarceration health and health care utilization. The analysis sample has a similar average number of hospitalization days in alcohol (about 0.4) and general (about 2.5) wards over the last three years compared to the 0-3 month sample but significantly greater days in narcotics (0.84 vs. 0.51 days) and especially psychiatric (5.38 vs. 2.79 days) wards.

Of course, this negative 'selection' for those with 4-48 months versus 0-3 months is not the margin we are studying. But, it highlights that those serving relatively long sentences, who

[19] This does not imply that there are no individuals remaining in the sample with such extensions or revocations; rather, we have dropped the most extreme of these cases.

are treated in our analysis, are a particularly disadvantaged subset of the criminal population. Table 2 also presents summary statistics separately for sentences of 4-12 (N=33,799), 13-24 (N=8,968), and 25-48 months (N=4,048). These highlight again differences in observables, but also that observables are not monotonically 'worse' as sentences get longer. Rather, they are likely in part driven by the fact that inmates with longer sentences have relatively more violent and drug and alcohol offenses while those with shorter sentences have relatively more property crimes. The distribution of drug and alcohol, property, and violent offenses in each group is: 13%, 49%, and 28% (4-12 months), 17%, 35%, and 43% (13-24 months), and 28%, 18%, and 52% (25-48 months). In terms of hospitalization, those with the longest sentences have on average fewer hospitalization days in the last three years compared to the other two groups, and the most days is seen for those with the 4-12 month sentences. For all groups, psychiatric days are the most prominent. We return to the differences in offenders across sentences when considering the possibility of identifying non-linear effects of prison exposure.

Figure 2 presents information on the post-release health of our analysis sample. Panels A and B provide mortality statistics measured 10-years post release for each sentence month bin. Panel A shows the share who died according to ICD-10 codes for suicide (squares), violent death (solid circle) and circulatory disease, cancer, or digestive disease or CCD (open circle). Panel B shows the share who died due to any cause (bars) and coroner indicators of narcotics (open circle) and alcohol (square) related deaths; the alcohol and narcotics flags are not mutually exclusive from each other or the other categories. In the vast majority of sentence month bins, the share of the sample that died (for any reason) is between 12% and 20%. A substantial proportion of deaths are alcohol or narcotics related, especially for shorter sentences. The share who die from a CCD cause is 3-5% in most bins, while the share who die from suicide is greater than 1% in most bins. Violent deaths are the least common (less than 1%).

Finally, Panel C of Figure 2 plots the dynamic paths of mortality (solid line) over time. By 8-years post release, more than 10% of the analysis sample has died. Hospital utilization rates are also quite high: 60% have been hospitalized by this time (dashed line). This figure also shows that recidivism rates (dash-dot line) for the analysis sample are steep: more than 50% are re-incarcerated within 12 months and 60% by 48 months.[20]

[20] Hospitalization and recidivism statistics are based on the sample alive and never emigrated from Sweden *t* months since release. Mortality statistics condition on the sample still in Sweden. We do not use post-release hospitalization measures as an outcome given the many interpretation challenges: (i) to the extent that prison exposure impacts mortality, this results in a selection bias in the population alive to use hospitals, (ii) an increase in hospitalization can signal both worse health and increased utilization, and (iii) re-incarceration can directly impact hospitalization if there is a different threshold for prisoners to be admitted to in-patient hospitals.

5. Sentencing Reform Implementation

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

5.1. No Impact on Sentence Length or Earlier Judicial Decisions

Our identification strategy relies on the fact that the reforms should only affect the share of time served for a given sentence, but not the actual sentence: the former is determined by the prison authorities, while the latter is determined by a judge and three lay judges. Similarly, decisions at earlier stages of the judicial process, e.g. arrest, charge, or pre-trial detention, should not in theory be affected by the reforms. However, as previous research finds that criminal justice agents, including prosecutors and juries, may try to offset increases in sanction severity by charging or convicting defendants of lesser crimes, we assess the extent to which 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 3 show the sentencing distributions (measured in prison sentence days) for those convicted in a four-year window around the 1993 and 1999 reforms, respectively. For each reform, we look at the distribution of sentences for three sub-samples: those who are convicted and start their sentence prior to the reform (solid line), those convicted and start their sentence after the reform (dotted line), and those convicted before but start their sentence after the reform (dashed line). We refer to this last sub-group as the 'straddle sample'. These figures demonstrate that the sentence length distributions do not change around the reforms. There is no evidence of a 'shift down' in the treated sentence month bin regions. Rather, the sentence length distributions lie practically on top of each other for each sub-sample. Thus, there is no evidence of manipulation in sentencing. Furthermore, Appendix Figure 2 provides evidence that the reforms did not systematically affect the share of cases that received a waiver of prosecution, a summary sanction order, or a courtroom conviction. Nor did it affect the use of various sanction types: prison, fines, or other sanctions.

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

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

crime.[22] Importantly, the prosecutor is not involved in sentencing, which is left to the judge and lay judges; discretionary decisions in sentencing are limited by the sentencing 'window' (the minimum and maximum sentence for each crime) being pre-specified and relatively narrow.

5.2. Who Was Treated and Reform Timing

Exposure to the reform should, according to the law, be determined by a defendant's conviction date. A person convicted before the reform should serve the share of time prescribed under the earlier regime, unless the sentence starts after the reform *and* the post-reform regime is more lenient. As both the 1993 and 1999 reforms increased the share of time-served for all sentencing bins (besides 2-months in 1999), the conviction date rather than the start date should in theory be the binding date for our analysis sample of 4-48 month sentences. In practice, however, we find evidence that the prison authorities did not strictly adhere to this policy. This can be seen by returning to our three sub-samples (pre, post, and straddle) in Panels C and D of Figure 3. These figures display the distribution of the share of time served for each sub-group. Though the distributions for the straddle sample (dashed line) should look like that of the pre-reform sample (solid line) – i.e. the straddle sample should not be treated – we find clear evidence that these intermediate samples are partially treated: for both reforms, the share of time served for the straddle sample is markedly shifted to the right. Moreover, the share of time served shows concentrations of observations around the value prescribed after the reform – two-thirds; this implies that at least some of the straddle sample was treated by the prison authorities.[23]

Thus, naively using the conviction date to code treatment would lead to significant measurement error. Therefore, as highlighted previously, we exclude individuals at risk of such measurement error, i.e. those convicted before but who start their sentence after the reform.

Figure 4 demonstrates the immediate implementation of the reform. For sentence lengths of 3, 4-12, 13-18, 19-24, and >25 months, Figure 4 plots the average share of time served (dashed line) and that prescribed by the law (solid line) over time – month of conviction is on the x-axis. Vertical lines correspond to reform dates. For the most part, one sees a discrete change in the average monthly share of time served immediately after the reform.[24]

[22] In fact, prosecutors routinely drop charges for lesser crimes to focus on the more serious ones. We see no change around the two reforms in the probability of dropping a lesser charge, which can be seen in the suspects register (*misstänktsregistret* in Swedish) maintained by the Swedish National Council for Crime Prevention.

[23] This could have occurred unintentionally through a misunderstanding of the rules or intentionally in an attempt by the prison authorities to avoid within facility conflict or to be 'fair', i.e. two individuals arriving on the same day with the same sentence should be treated the same.

[24] The regularly spaced dips in Figure 4 correspond to each July, which is associated with a lower share of time served, especially in the 3-month bin. Fewer cases are decided in July due to this being the main vacation period in Sweden. Those convicted in July have longer waiting periods and stays in pre-trial detention, which are credited

5.3. The First Stage Impact of the Reform on Share of Time and Prison Days Served

How did the share of time served and actual number of days in prison change in each sentencing 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, respectively, actually served on a dummy indicating whether the conviction occurred after July 1, 1993. This is done separately for each sentence month bin from 0 to 48 months; thus, we present the unconditional effect (and associated 95% confidence interval) of the 1993 reform on the share of time and number of days served for each sentence month bin. Vertical bars are placed at 4, 12, and 24 months, as these are the treatment thresholds defined in the law. Consistent with the law, there is no effect for 0-3 month sentences. For 4-12 month sentences, share served significantly increases in each bin, with effects ranging from 13 to 16 percentage points. Thus, the reform had a large and significant impact on the intended sentence month bins, the magnitude of which was close to that prescribed by the law (17 percentage points). The effect on share of time served decreases as sentence length increases from 13 to 24 months, with no visible effect for sentences of 20 months or longer; this is consistent with the theoretical effect of the reform only being 3 percentage points for a 20-month sentence, and zero for sentences longer than 24 months. Despite the equal treatment with respect to the share of time served in the 4-12 month bins, there is an increasing effect on the number of days served, ranging from 17 days for 4-month sentences to 59 days for a 12-month sentence. The number of additional days served decreases with sentence lengths in the 13-24 month range.

Panels C and D of Figure 5 demonstrate the same general pattern for the 1999 reform: (i) Share of time served only changes for bins that should be affected, (ii) these changes are consistent with the reform, as the share of time served decreases in the 2-month bin but increases for sentences longer than 12 months (with larger effects for longer sentences) and (iii) the magnitudes are close to what the reform predicts. Panel D shows that the effect on the number of days served increases up to a maximum of more than 130 days for 25-month sentences, highlighting again that a homogenous treatment (increasing prescribed share served from 50 to 67%) has heterogeneous impacts on days served.

These bin specific results underlie the formal first stage impact of the reform. Table 3 presents the first stage results for the baseline specification (equation (3)) in Panel A. For the full sample (column (1)), we find a strong significant first stage, with an F-statistic of 111. The

to the offender as time already served; thus, they have a systematically lower share of time served in our data than those convicted in other months. We can adjust for seasonality but not for time served in pre-trial detention. Though our baseline controls for such seasonality, the results are robust to excluding these controls.

coefficient on the early release law variable (i.e. the instrument) is 271. As this variable is the share of the sentence an individual should serve, it can in theory range from 0 to 1 (though in practice the law never prescribed less than 0.5). Thus, for the analysis sample of 4-48 month sentences, the average days served would increase by 271 if the law changed from prescribing 0% to 100%. As the reform increased the prescribed share of time served by 17 percentage points (from 0.5 to 0.67), our first stage estimates imply an average increase of about 46 days.[25]

Each column of Table 3 corresponds to the sample that is alive and has never emigrated from Sweden up until date *t* (1, 2, 3, and 10 years) relative to release. The first stage 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.

5.4. Conditional Independence

For the reduced form estimates to be interpreted causally, the early release regime to which an individual is exposed should be unrelated to individual defendant and case characteristics that could also affect the defendant's post-release outcomes. Such correlation could occur, for instance, if there was a systematic response on the part of the criminal justice system (e.g. judges or prosecutors) to the reform; we have already shown in Section 5.1 that this is not the case.[26] Panel B of Table 3 tests conditional independence more directly by presenting the first stage when excluding all observable controls, *X*. If reform exposure is unrelated to these characteristics, then their exclusion should not change the estimates. This is what we find.

Finally, to the extent that identification is driven by temporal variation in exposure to the reform, one may be concerned about other contemporaneous changes in society. As Sweden experienced a financial crisis in the early 1990s, this is valid concern. However, any raw differences in the employment and income histories for those convicted after and before the reforms are accounted for with our identification strategy, which controls for year fixed effects in conviction and a conviction month trend. This is depicted in Figure 6 (for employment and income for both the 1993 and 1999 reforms): for each sentence month bin, we display the simple

[25] Appendix Table 4 demonstrates the robustness of the first stage to: dropping those with start dates more than 1 year from conviction, and keeping those with only post-trial detention, trimmed with very high or low shares served, juveniles, and the straddle samples when using either the conviction or start dates to define treatment.

[26] Manipulation could also (theoretically) occur in the timing of conviction: did agents 'push up' conviction dates so as to be treated by the more lenient regime? It is not trivial to rush a conviction, given existing caseloads and the relatively short time between arrest and conviction. For more than 50% of the analysis sample, there are less than 3 months between offense and conviction. Given the lack of manipulation in the more salient sentence length, manipulation in timing seems unlikely. Columns (9) and (10) of Appendix Table 4 demonstrate robustness to dropping the 4-6 month sentence bins, which is where manipulation is most feasible (for the 1993 reform), and a 4-month window around each reform, which is where a manipulation of conviction date is constrained to occur.

pre-post comparison of means (gray line) and the regression adjusted difference in means when including conviction year fixed effects and a conviction month trend (black line). Most notably, individuals convicted after the 1993 reform had worse pre-incarceration employment histories (measured in the last 3 years) compared to those convicted before: but these differences completely disappear when controlling for trends.[27]

Violation of the conditional independence assumption can also occur if other contemporaneous criminal justice reforms systematically affect the sentence that defendants with certain characteristics should receive. Our empirical analysis already suggests this is unlikely, since controls for defendant and case characteristics did not affect the first stage estimates. This finding is not surprising given that the few reforms that did occur around this time generally targeted shorter sentences and drunk driving offenses.

Specifically, though there were no other reforms in 1993, Sweden passed a series of reforms targeting drunk driving (lowered the blood alcohol content level, raised the maximum punishment from one to two years in prison, and increased substance abuse treatment programs) in February 1994 (BRÅ 1998:7). Our summary statistics table showed, however, that most driving under the influence offenses were outside our analysis sample in the 0-3 month sentence bins; nevertheless, we control for such offense types in our baseline specification. Also largely targeting drunk driving offenders, in August of 1994, the Swedish Prison and Probation Service began a pilot program for electronic monitoring in the home for those with 1-2 month sentences; this program was expanded to 3-month sentences and the whole country in 1997 and made permanent in 1999. Since the 1993 and 1999 reforms only affected sentences of at least 4-months, our estimation sample is unaffected by the introduction of electronic monitoring.

Two additional sanction types were introduced on January 1, 1999. Community service with probation could be used as an alternative to short prison sentences of three months or less and secure youth treatment centers could be used instead of prison for young offenders. Though these two sanctions lower (somewhat) the aggregate number of persons sentenced to prison after this date (e.g., Kriminalvården 2012 RV0401), they do not affect our identification strategy, since the 1999 early release reform only affected adults with long prison sentences.

[27] Moreover, there is little raw observable difference in offender characteristics around each reform (figures available upon request). These results also suggest that there was not a general deterrent effect of the reform (i.e. a change in behavior of potential offenders due to increased expected punishment), at least not one which changed observable offender characteristics. The robustness of the results to observables supports this conclusion.

5.5. Monotonicity

Though not necessary for a causal interpretation of the reduced form, monotonicity is required to assign a local average treatment effect interpretation to the two-stage least squares estimate if the effect of longer spells in prison is heterogeneous across individuals. One testable implication is that a non-negative first stage estimate should be seen for any sub-sample: i.e., exposure to the reforms should increase, or at least not decrease, the days served for any subsample.[28] Table 4 demonstrates that similar first stage estimates are seen for a wide range of sub-samples that characterize the number of current offenses and past prison sentences, whether it is the first or last offense observed for a defendant in our sample period, current offense category, defendant demographics and socioeconomic status (marriage, children, employment), and pre-incarceration health. These results also point towards the validity of heterogeneity analyses: each sub-sample is affected by the reform in the same way.

The last two rows of Table 4 show the first stage results for the sub-sample of convictions surrounding each reform (+/- 2 years) separately. The first stage estimates are 134 days for the 1993 reform and 429 days for the 1999 reform, with F-statistics of 13 and 123 respectively. For a 17 percentage point increase in the share of time served, the 1993 first stage estimates imply an average increase of 23 days served while the 1999 reform implies 73 more days.

6. The Effect of Prison Days Served on Mortality

6.1. Baseline Reduced Form and IV Mortality Results

We begin by presenting in Table 5 the reduced form effect of the share of time served law on the chance of death overall measured *t* (12, 24, 36, and 120) months since date of release. Overall, exposure to a longer share of time served *reduces* the chance of death; i.e. increased prison exposure improves health as measured by mortality. However, these results are only significant (at the 10% level) in the 24 month window in Table 5. Panel A of Figure 7 traces out the dynamics of the effect at each of the first 10 years post-release; while the estimates are never positive, they are generally not significant when considering the entire analysis sample.

These aggregate results may mask important heterogeneity in the effect of prison on (i) the *cause* of death and (ii) the chance of death for different sub-populations. Specifically, as highlighted earlier in the paper, this population of prisoners has a disproportionate share of individuals with mental health problems. Moreover, criminals lead generally risky lifestyles, which puts them into contact with other violent individuals or neighborhoods. For these reasons,

[28] Similar monotonicity tests are also included in, e.g., Bhuller et al. (forthcoming) and Dobbie et al. (2018).

we also consider the effect of extended prison exposure on the chance of suicide and violent death. In addition, the change in lifestyle while in prison – e.g. carefully controlled diets, the lack or limited access to drugs and alcohol, and increased health monitoring in general – could have impacts on other categories of death. Specifically, we consider drug and alcohol related deaths; while there is not a specific ICD10 code for this, we utilize the coroner flags, excluding the other causes of deaths studied. And finally, we look at deaths attributed to circulatory or digestive reasons, as well as cancer; these categories are presented combined (CCD) in both Table 5 and Figure 7, but separately in selected tables and figures in the Appendix.

The results in Table 5 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, which is observed in both the short (12-36 months) and long term (120 months); Panel B of Figure 7 confirms that this suicide effect is seen in each of the first 10 post-release years. How large is this improvement in mental health? To interpret these reduced form estimates, one needs to recall that they represent the effect of a hypothetical increase in the share of time served from zero to 100%. In reality, the reform increased the share of time served by 17 percentage points. Relative to the mean post-release suicide rates (listed at the bottom of the table), reform exposure reduces the chance of suicide by more than 75% in the first three years; there is still a 37% reduction in the chance of suicide 10-years post release (e.g. (0.17 * -0.022) / 0.010).

Turning to violent deaths, we find a negative coefficient for the first 3 post-release years, which switches in sign by year 10. The immediate (12 month) short-run reduction in the violent death chance is significant at the 10% level; while it does not change in magnitude over the next two years, precision decreases. Given that violent death is a relatively rare event, even for this population, the relative magnitude of these effects are quite large (a 100% reduction in the chance of violent death due to the reform). Tracing out the dynamics, Panel C of Figure 7 shows that the reduction in violent deaths is short-term in nature, and by year 4 post-release the estimates have swung (permanently) positive.

The next set of results in Table 5 is for circulatory, cancer and digestive related deaths. For the first three years, there is an insignificant effect. However, in the 10-year post release long-term, there is a large and significant reduction in the chance of such a death: exposure to the reform (i.e. increasing share of time served according to the law by 17 percentage points) significantly reduces the chance of death from a CCD related cause by 27%. Panel D of Figure 7 demonstrates that this general health improvement seen at year 10 is not an anomaly but rather shows up (significantly) about five years post release and then persists. Considering that these

causes of death are more common as individuals age and are often attributed to life-style behaviors over an extended period of time, it is perhaps not surprising that this effect is only seen in the medium and long-run. Appendix Figure 3 presents these results separately for circulatory, digestive and cancer deaths; the results are largely driven by circulatory deaths, but similar patterns (though generally insignificant) are seen for the other categories.

The final panel of Table 5 shows the results for narcotics and/or alcohol related deaths that are not attributed to the above ICD10 codes. There is no significant effect at any of the short and long-term periods shown in the table. The dynamic path for these alcohol/narcotic related deaths is traced out in Panel D of Appendix Figure 3. Though the point estimates swing positive at year 4, and remain positive over the next 6 years, they are never significant.

Finally, the right side of Table 5 translates these reduced form estimates into the causal effect of an additional prison day on mortality using the share of time served prescribed by the law as an instrument for the number of days served. A causal interpretation relies on four identifying assumptions. The first three – relevance, conditional independence, and monotonicity – have already been demonstrated in Section 5. We will argue and present evidence in support of the fourth assumption – the exclusion restriction – in Section 6.5. Our analysis implies that an additional prison day reduces the overall chance of death by less than 0.5%, while the chance of suicide decreases by 2.7% in the first year after prison. The suicide effects persists over time but gets gradually smaller relative to the mean suicide rate: 1.9% in year two, 1.7% in year 3, and 0.8% in year 10. The short-term negative effect of an extra day in prison on violent death is similarly large (2% at year one) but disappears much faster (0.7% by year 3, and insignificant). Finally, an additional day in prison decreases the chance of death due to circulatory, digestive or cancer by 0.57%.

6.2. Robustness and Sensitivity Analyses

This section presents a series of robustness and sensitivity checks for the baseline reduced form results. Appendix Table 5 includes only controls for criminal justice related case characteristics (as opposed to the full set of demographic and pre-incarceration socioeconomic and health controls). As we have already argued that the conditional independence assumption is satisfied, controlling for individual or case characteristics should not impact the results. This has little impact on the magnitude or significance of the results.

Appendix Table 6 considers whether the results are driven by a culling from the sample of the ‘least healthy’ individuals. Specifically, we consider the 53 individuals with sentences of 4-48 months who died in prison. Were such deaths in prison systematically more likely after

the reform? A simple look at the data suggests that this is unlikely, as these deaths are fairly evenly distributed across sentence bins and over time. They are also not concentrated amongst suicides, but rather the largest share is circulatory, digestive, and cancer related (25% combined). To assess this question more formally, Appendix Table 6 estimates the baseline specification where the dependent variable is death in prison: does exposure to the reform increase the chance of death in prison? Results are also presented for simple pre-post reform regressions in the years around each reform, controlling for sentence month bin fixed effects. We find no evidence of a significant effect of reform exposure on the chance of death in prison; in fact, most estimates are negative, opposite of what a culling story would imply.

Appendix Table 7 presents a series of robustness and sensitivity checks for any death, any suicide, and any violent death measured at 12, 24, and 36 months post-release. The first panel presents the baseline results. We first consider whether these results are affected by allowing repeat offenders to enter the sample more than once; we thus restrict the sample to the first and last sentence, respectively, in the sample period. We find the same general pattern of results, and though there is some loss of precision, significant effects on suicide remain. One factor to note is that focusing on the first and last sentences puts more weight on the 1993 and 1999 reform samples, respectively. To the extent that there are differential effects of each reform (which we return to shortly), this will be reflected in the first and last observation samples.

Appendix Table 7 also demonstrates the robustness of the results to how we deal with the straddle sample. The baseline excluded this sample rather than assigning a treatment status that we know will be measured with error for at least some of the straddle sample. Including the straddle sample, and assuming that they are either untreated (assign treatment using start date) or treated (assign treatment using conviction date), respectively, yields the same general pattern of results, but with somewhat attenuated estimates. Finally, Appendix Table 7 also demonstrates that the results are robust to controlling for sentence day (rather than sentence month) fixed effects, and controlling for differential trends for each sentence month bin.[29]

[29] Appendix Table 7 also confirms the robustness of the results to sample creation decisions – keeping individuals who served their entire sentence in post-trial detention, unusually high or low share of time served, and juveniles. And in further support of a lack of manipulation of charge/trial timing, we show that the results are robust to dropping a four-month window around the reform. Dropping this four-month donut actually also limits the possibility, especially in the 1993 reform where sentences are relatively short, that treated individuals in the analysis sample are actually exposed to control individuals while incarcerated, reducing any concerns about spill-over effects. Finally, the main results are robust to a probit specification. These results are not shown here, but are available upon request.

6.3. Heterogeneity in Mortality Results: High Risk Sub-samples

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 in the long run (circulatory, digestive, and cancer). This section assesses (i) whether the overall effect is perhaps more precisely estimated for selected subsets of the population, and (ii) whether these subsamples drive the cause of death specific results. In particular, we focus on sub-samples that one would expect to be at the highest risk of death for these specific causes. Thus, we examine three hypotheses.

First, are suicides driven by those with pre-identified mental health problems? We proxy for pre-incarceration mental health problems by looking at those who have ever been admitted to a psychiatric ward in the five years prior to the beginning of 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 the chance of suicide. The suicide rate in each of these sub-samples, measured 10 years post release is: 2% (psych admission), 1.3% (general admission), and 0.6% (no admission). Panel A of Figure 8 presents the 10-year dynamics for each sub-sample: psych (solid circle), general (open circle), and no hospitalization (square). The results clearly indicate persistent effects that are largest for those who have previously been admitted to psychiatric wards, while there is a smaller effect that is still significant for those in general wards; despite the difference in point estimates, the effects are not significantly different for the two groups of hospitalized individuals. In contrast, there is no significant effect for those who were 'healthy' prior to admission. The reduced form estimates imply that the typical exposure to the reform of increasing the share time served by 17 percentage points reduces the 10 year post-release chance of suicide by about 75% and 50% for the psychiatric and general ward samples, respectively.

Second, we assess whether the violent death effect is driven by (and even more prominent for) violent offenders, i.e. those most likely to be exposed to violence. Panel B of Figure 8 presents the results separately for those whose current offense is violent (solid circle), property (open circle) and drugs or alcohol (square). Summary statistics make it clear that violent offenders are at the highest risk of violent death: 10-year post release statistics indicate that for each of these groups, respectively, 0.7%, 0.4%, and 0.4% die a violent death. That is, the violent death rate is almost twice as large for violent offenders. Figure 8 makes it clear that increased prison exposure only reduces the chance 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 reduced form effect on violent death, while for drug and alcohol offenders, there is actually a significant increase in the chance of violent death that begins within 2 years of release, and persists over time. The latter could occur, for instance, if more time in prison reinforces and/or expands drug networks (i.e. criminal capital) that results in more exposure to violence upon release (see Bayer et al (2008)). The former could occur if violent offenders end up 'incapacitated' by returning to prison (we will return to this when looking at recidivism) or if they are removed from crime and criminal networks.

Finally, we turn to the long-run effect on CCD deaths; these are causes of death are more prevalent for older populations. Thus, we consider whether they are driven by the relatively older individuals in our sample. Panel C of Figure 8 splits the sample according to whether individuals were older or younger than 33 (the median age) at the prison start date. The share of the older than 33 sample who died from CCD related deaths 10 years post-release is 5.7%, compared to less than 1% of the younger sample. Consistent with this hypothesis, the medium and long-run reduction in CCD related deaths are driven by the relatively older sub-sample.

While the heterogeneity figures presented thus far test these explicit hypotheses, they do not paint the entire picture. Appendix Table 8 supplements these figures by providing the reduced form estimates for each of the studied sub-samples for: any death, suicide, and violent death measured at 12, 24, and 36 months and CCD deaths at 120 months. These results highlight some other interesting findings. Though the CCD results are driven by older individuals, we can now also see that older individuals are also significantly less likely to commit suicide. And younger individuals, who have much lower death rates in each category in general, are actually significantly less likely to die due to any cause (i.e. when aggregating all causes together). In fact for all sub-groups, there is a negative coefficient on the chance of death from any cause in the first 12-36 months, which is significant for both the young sample and property offenders. Though not always significant for the other sub-samples, the estimates tend to be comparably large. These results also suggest that there may be substantial overlap between these samples, especially violent offenders and those with psychiatric histories: for both of these groups, significant reductions are seen in the chance of violent death and suicide.

6.4. Heterogeneity in Mortality Results: 1993 versus 1999 Reforms

This section tests whether there is heterogeneity in the effects of the 1993 and 1999 reforms. This possibility is raised by the fact that each reform 'treats' offenders with different sentence lengths (shorter and longer, respectively). Differential effects could arise for two reasons. First, if the marginal treated offender differs across reforms in terms of (un)observable characteristics,

then heterogeneous effects (related to these characteristics) could generate differences across reforms. A second possibility is simply that the effects of longer prison exposure are non-linear.

Table 6 presents the baseline reduced form results separately for those within 4 years of the 1993 and 1999 reforms respectively. The effect of both reforms on the chance of any death is negative; though the point estimates are larger for the 1999 reform sample, they are generally not significantly different from each other. The overall negative effect of the 1999 reform (which is significant at 24 months) is driven by multiple types of death (especially alcohol and narcotics); but these effects are generally not significant. For the 1993 sample, we see a significant reduction in the chance of suicide, which is comparable to that for the whole sample. In general, the pattern of the point estimates for violent death and CCD deaths are similar for both reforms; i.e. both sub-samples contribute to the effects in the full sample. These results also demonstrate that a substantial amount of precision is gained by taking advantage of the full variation in the data set that combines the reforms.

Most of the mortality results are perhaps too imprecise to say whether the effects are different across reform. However, the results are suggestive that the 1993 reform had a stronger effect on mental health (suicide) while those affected by the 1999 seem to have large (albeit insignificant) reductions in alcohol and narcotics related deaths. What explains these potential differences? Appendix Table 9 shows that indeed the observable characteristics of those affected by the 1993 reform (serving 4 – 23 months) are clearly different than those affected by the 1999 reform (serving 13 – 48 months). In particular, half of the 1993 sample is in prison for a property crime and 29% for a violent crime, while the reverse is true for the 1999 sample (48% violent and 26% property). Especially relevant to the suicide perhaps is that the 1999 sample has fewer pre-prison days in psychiatric wards, despite similar health patterns otherwise.

Is it just observable differences that drive these differential results? Or are there potential non-linear health benefits to incarceration? To get at this, we re-estimate the suicide results for the 1993 and 1999 reform samples, separately by pre-incarceration hospitalization history. If the effects were driven by differences in sample characteristics, then making the samples more similar on this observable characteristic, which has already been shown to be important for suicide, should result in more similar results for the 1993 and 1999 reforms. Though slicing these reform specific samples further decreases precision, we do in fact see a significant negative effect for those with psychiatric ward histories in the 1999 reform. These results suggest that at least some of the differences in results (to the extent there are any) are driven by heterogeneous characteristics in the sample of offenders driving identification in the two reforms. This does not, however, rule out that non-linear effects of sentence length exist with

respect to health outcomes; unfortunately, we cannot provide a direct/clean test given the potential confounder of different (un)observables.

6.5. Evidence in Support of the Exclusion Restriction

The exclusion restriction requires us to ask whether the early release reforms could affect outcomes through channels other than an increase in the number of days in prison. One possibility is an effect on the date of release, and hence the economic conditions faced by individuals re-entering society.[30] But, our identification strategy, which controls for year of conviction dummies and conviction month trends, should control for any potential differences in such factors. One should also keep in mind that the early release reforms did not change the amount of time spent on active probation, since this is 12 months for all former inmates regardless of their initial sentence length.

Swedish prisons have an average occupancy rate of 90%. Appendix Figure 5 shows the number of available places and the average number of inmates each year in the Swedish prison system. One potential consequence of increasing the share of time an inmate must serve is that (all else equal) the stock of prisoners will grow. Though this could lead to prison overcrowding and conditions that may be detrimental to inmates' health and well-being, such overcrowding is not observed immediately after the 1993 or 1999 reforms (see Appendix Figure 5).

To further examine this potential violation of the exclusion restriction, we use data on all prison inmates during the period 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 in order to construct a monthly time series. In Panel A of Appendix Figure 6, 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 different measures of prison capacity utilization (see Panels B and C).[31] Since the majority of the inmates in our estimation sample reside in open facilities and medium security prisons, we also created the same types of figures for open, medium security, and high security prisons separately. We see no indications of overcrowding around (or just after) the early release reforms in these figures.[32]

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

[31] Panel D of Appendix Figure 6 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.

[32] These additional figures are available on request.

Another concern is that the prison authorities may have reacted to the expected increase in the number of prisoners by changing: the types of facilities, programs or treatment to which they assigned inmates and the quality of care. We test for this explicitly and find no evidence that inmates are being assigned to higher or lower security class prisons in response to the changes in the early release laws.[33]

It is important to keep in mind that time served changes immediately after the reform, while these other characteristics 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. Thus, the closer one is to the date of the reform, the more one can abstract from anything else potentially changing in the system. To assess the potential role played by such time-varying environmental factors, we re-estimate our baseline reduced form for smaller and smaller windows around each reform (see Appendix Table 10). If such factors are unimportant, then our estimates should not change.

In the previous section, we estimated the effects of each reform separately using a 4-year window around each reform (individuals convicted +/- 2 years from the reform date). For the 1993 reform, 88% of the individuals are in sentencing bins treated by the reform. But, for the 1999 reform, just 29% of the sample is in treated bins. Given that identification relies on within bin variation in exposure, our ability to look at smaller windows seems promising for the 1993 reform but likely to suffer from a small sample and precision problem for the 1999 reform. Though similar patterns of results are seen, there is a substantial loss in precision, especially as we get down to a window of 6-months on either side of the reform.

7. Mechanisms and Discussion

The analysis thus far indicates that increased exposure to Swedish prison generated by the early release law reforms improves health, as measured by mortality, especially for certain high risk populations. The first main finding is a reduction in the overall risk of death, which is significant for some sub-groups, including property and young offenders. Second, 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. Third, there is a long-term improvement in general health, primarily circulatory death, which is especially strong for older individuals. Finally,

[33] We know which prison each inmate is assigned to. Prisons are classified as low, medium, mixed (low and medium), and high security. We create a dichotomous variable for each security class and then use these as outcome variables in our baseline reduced form regression. We see no statistically significant effects of reform exposure on the probability of being assigned to a particular type of prison. The point estimates themselves are very small and display no regularities. These regression results are available upon request.

there is a short-term reduction in violent deaths that is especially prominent for violent offenders. What drives these health/mental health improvements? This section considers two mechanisms: a direct effect via healthcare 'treatment' within Swedish prisons and an indirect effect via the effect of increased prison exposure on recidivism.

7.1. Swedish Prisons and Prison Healthcare

A first-order question is whether more time in a Swedish prison could feasibly directly improve an inmate's health. We argue that it can.

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 Appendix Figure 1. One notable feature of Swedish prisons is that each inmate has his or her own private cell. While incarcerated, an inmate's time is governed by a treatment and activity plan designed during their first week. This plan includes: (i) details about working, education, and substance abuse or psychological treatment, (ii) visitation rights, and (iii) a clear end date for the inmate's sentence.

The treatment plan can also include health information and routines, based on the results of a health exam given to all new prisoners. All prisons have their own health clinics with nurses on call every day and doctors available one or two days a week. Larger prisons often have their own full-time psychologist. An acutely ill inmate will be transported to a local hospital, while specially trained custodial staff members are responsible for providing daily medication to inmates who need it. In most respects, the prison health care system is quite similar in quality and quantity to the health care system outside of prison.[34]

We provide further insight into the healthcare utilization of inmates by studying all inmates with 4-48 month sentences entering the Swedish 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 more modern cohorts, including: (i) all visits to doctors, nurses, and psychologists while in prison, (ii) the administration of medicines, and (iii) various treatment programs in which inmates are enrolled. We also create measures of healthcare use for the two years immediately preceding an inmate's time in prison. Specifically, we match on inpatient and outpatient hospitalization records, and

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

doctor prescribed medicines (95% of which are prescription medicines); outpatient and prescription records are only available for these more modern cohorts. *Pre-prison psychiatric* visits include both outpatient visits to a psychiatric clinic (or psychiatric hospital ward) and nights spent in a psychiatric ward. *Pre-prison non-psychiatric* include both outpatient and inpatient hospital visits. *Medicines* is the number of unique doctor prescribed medicines that a person picks up from the pharmacy on a single day, which is then summed across the 730 days preceding a prison sentence.

Pre-prison and in-prison health care variables are shown in Figure 9. The extensive margin variables in Panels A, C, and E show that the take-up rate of healthcare services during the two years before prison is quite high in our sample. But it is just as high (or even higher) when the same individuals are in prison (compare, e.g., the values of those sentenced to 36 months in prison – and thus serve two years– with their pre-prison values, which also cover a two-year period). Roughly 32% of this sample has had at least one outpatient or inpatient visit to a psychiatric clinic or psychiatric hospital ward; 93% have at least one outpatient or inpatient visit to a regular hospital; and 72% have used at least one doctor prescribed medicine in the two years prior to prison.

Intensive margin variables are depicted in Panels B, D, and F of Figure 9. In-prison doctor visits are twice as high as total outpatient and inpatient visits (pre-prison non-psychiatric) and in-prison nurse visits are even higher. We also see that those with longer sentences have a large average number of visits with the prison psychologist. But the true comparison may be even higher, since many of the visits included in our *pre-prison psychiatric* variable are "reactive" in nature and not part of a planned, proactive treatment program. Lastly, we see that prisoners are administered (on average) 20 medicines per year, which implies that inmates do receive needed medication.[35] Furthermore, necessary medicines are administered by trained personnel on a daily basis, which may actually help some inmates properly follow medication regiments.

Many inmates also take part in professional treatment programs to help address mental health issues and/or alcohol and drug abuse. Figure 10 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 focused on mental well-being and substance abuse): (i) motivational interview (*Bettende – Samtal – Förändring* in Swedish), (ii) Alcoholic/Addicts Anonymous' 12-step program, and (iii) the Correctional Service of Canada's offender

[35] The raw numbers for *in-prison medication* and *pre-prison medication* cannot be directly compared, since inmates are not allowed to keep medicines in their own room to self-medicate. Instead, trained personnel administer medicines daily. We observe these daily administrations of medicine in our prison healthcare data.

substance abuse pre-release program (OSAPP) (*Våga Välja* in Swedish). Similar programs were also available to inmates during the 1990's.[36]

Figure 10 shows that more than 80% of those serving longer sentences complete at least one such program (some complete more than one). Notably, the probability of completing a treatment program rises rapidly as we move from short to medium length sentences – but levels off for those serving long sentences. The U.S. National Institute on Drug Abuse argues that "one of the most reliable findings in treatment research is that lasting reductions in criminal activity and drug abuse are related to length of treatment. Generally, better outcomes are associated with treatment that lasts longer than 90 days, with treatment completers achieving the greatest reductions in drug abuse and criminal behavior." (NIDA 2014, p. 20) Thus, staying somewhat longer in prison may improve the health of inmates by increasing the efficacy of the treatment programs that they are engaged in.[37]

With this additional descriptive evidence in hand, we conclude that the Swedish Prison and Probation Authority supplies a large amount of healthcare services and treatment programs to inmates. We also see that in-prison take-up rates are high, and higher than pre-prison take-up rates. Together with the high quality living conditions observed in Swedish prisons and the prison system's focus on mental health and substance abuse program, these facts can potentially explain why longer prison exposure improves both mental and general health. 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.

7.2. Recidivism

This section presents the results of applying our identification strategy to an alternative outcome – recidivism. We first present the baseline results for the full sample and then a series of heterogeneity analyses that parallel those for mortality. Finally, we use these results to inform us on whether an indirect channel on health via recidivism is plausible.

[36] During the 1990's, there was a large number of locally initiated treatment activities. Starting in 1995 the central office initiated a program of support and professionalization of these activities. A national treatment program group was appointed in 1999 and in 2002 a national program accreditation system was put in place. Today, there are 13 accredited crime reduction and/or substance abuse treatment programs used by the Swedish Prison and Probation Service. (Kriminalvården 2014; Tallving 2018)

[37] Not all inmates spend time in only one prison. For those with longer sentences, it is common to first spend time in post-trial detention (waiting to be moved to a suitable facility). Then they spend most of their time in a middle or high security prison. Then they are moved to an open facility, before being released. One mechanical effect of spending more time in prison is that inmates do, in fact, spend more time in their main placement. We estimate that placement stability has increased by 25%, giving inmates more time in one place to complete a treatment program.

Table 7 presents the reduced form effect of the prescribed share of time served and IV estimates of an additional prison day on three measures of recidivism: any conviction, more than one conviction, and any prison by month *t* since the release date. The same (reduced form) results are traced out over 10 years in Appendix Figure 4. On average, more time in prison *reduces* post-release recidivism. Negative coefficients are seen for all outcomes in each of the first three years after release, and remain negative 10 years out for both having more than one conviction and returning to prison. This reduction in recidivism is significant for all outcomes in the first year after release; some loss of significance is seen as we get further from the release date. The IV estimates help evaluate the magnitude of these effects: one additional day in prison reduces the chance of recidivating (returning to prison) by 0.16% within 1 year, 0.08% in 2 years and 0.06% in 3 years. Serving an additional 46 days in prison (the average effect of the reform) would decrease the chance of returning to prison within two years by almost 4%.

However, these results for the whole sample are masking some heterogeneous effects: certain subsamples do appear to drive the recidivism results. Perhaps most striking, as seen in Table 8, which focuses on returning to prison, is that the reduction in recidivism is completely driven by the 1993 reform.[38] For the 1999 reform sample, there is even a positive (albeit insignificant) effect on recidivism. This again raises the possibility of non-linear effects of increased prison exposure: does more time in prison only benefit those with relatively short sentences? Or are these differential results driven by different (un)observable characteristics of individuals at the margin of treatment in both reforms? The remaining panels of Table 8 help get at this question; by focusing on a single characteristic, e.g. property offenders, we make the 1993 and 1999 reform sub-samples look more similar.

Indeed, when looking at property offenders, we see that the overall reduction in recidivism due to increased prison exposure is largely driven by property offenders. Moreover, this effect is seen for both reforms (though significance is lost when cutting the sample). These results would suggest that it is not necessarily non-linear effects of prison that drive the differential 1993 and 1999 results, but rather a different composition of offenders at the margin of being treated (more property or other minor offenders in the 1993 reform). A number of other relevant findings can be taken away from the rest of the heterogeneity analyses. First, there is little (to no) effect for violent offenders and young offenders. Second, a negative and significant effect is seen for both unhealthy and healthy pre-incarceration samples (for at least one of the reform samples).

[38] The same is true for any conviction or more than one conviction.

The main finding of these recidivism analyses is that the time-served reforms, on average, improved post-release crime behavior (and at the very least did not make it worse). This is consistent with Bhuller et al.'s (forthcoming) findings that incarceration (at the extensive margin) in Norway reduces recidivism, and an increasing number of papers that reach similar conclusions.[39] 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.

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

Second, it could be that prison does not directly improve health but rather indirectly affects it by improving the post-release lifestyle and environment of offenders; a reduction in recidivism would be one such improvement. For instance, individuals who are less likely to commit crime are likely also less likely to interact with other (potentially violent) offenders. If such a channel underlies our mortality effects, however, then one would expect the sub-sample analyses to yield parallel results, i.e. that the same sub-samples drive both sets of results. But this is, in fact, not completely the case. For instance, the overall reduction in mortality was driven by young and property offenders; while large reductions in recidivism are seen for property offenders, little effect is seen for young offenders. In addition, violent offenders were driving the short-run effects for violent death but not the recidivism effect. Taken together, these results suggest that the improved health due to increased prison exposure is not driven (only) by the indirect recidivism channel. Of course, a caveat to this analysis is that post-release health could also affect recidivism, and we cannot disentangle these simultaneous relationships.

8. Conclusion

Former prisoners around the world have worse mortality outcomes than others. Is any of this prison-health association causal? We gain some initial insight using exogenous variation in the length of prison exposure driven by Sweden's 1993 and 1999 early release policy reforms.

Our reduced form analysis of the effect of share of time served on post-release mortality yields the following main findings. More prison exposure reduces the overall risk of death,

[39] See Kuziemko (2013) and Rose and Shem-Tov (2019) as well as Hjalmarsson (2009), who studies juveniles in Washington state. This is also consistent with Hinnerich et al. (2016), whose study of Swedish drunk driving sentences finds a reduction in post release offending for those sentenced to a minimum-security institution or electronic monitoring rather than probation.

especially for property and young offenders. Second, 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. Third, there is a long-term improvement in general health (circulatory death) and short-term reduction in violent deaths, which are driven by relatively older offenders and violent offenders, respectively. The bottom line is that there is little evidence that increased prison exposure increases mortality risk, contrary to the existing correlational literature.

How do we explain the physical and mental health improvements caused by more time in prison? We consider two mechanisms. First, we demonstrate that it is plausible that Swedish prisons have a direct effect on health due to the fact that inmates receive medical and psychiatric care that they would not otherwise get (or seek out). Moreover, we demonstrate that health care utilization in Swedish prisons is incredibly high in terms of visits with medical staff, prescriptions, and program participation. Second, we consider the possibility of indirect effects due to an improved lifestyle more generally; we do in fact find there to be a significant reduction in recidivism, especially for certain sub-samples, such as property offenders. Though the overall effects are consistent with such a channel – i.e. both mortality risk and recidivism improve – there is not sufficient evidence to conclude that the results are explained by such an indirect channel. Specifically, the fact that it is not the same sub-samples that drive both sets of results suggests that this cannot be the only explanation.

This paper provides a first look at the important, policy relevant question of whether prison has a causal impact on the health of former inmates. At the same time, it raises many questions for future research. How low can prison quality go before the beneficial effects that we measure are pushed to zero, or turn negative? To what extent can improvements in inmates' health lower recidivism and re-incarceration? Answers to these and related questions are needed to aide in the design of socially efficient and humane prison policies.

References

Aizer, Anna and Joseph J. Doyle (2015) "Juvenile Incarceration, Human Capital and Future Crime: Evidence from Randomly-Assigned Judges," *The 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," George Mason Law & Economics Research Paper No. 19-19.

Barbarino, Alessandro and Giovanni Mastrobuoni (2014) "The Incapacitation Effect of Incarceration: Evidence from Several Italian Collective Pardons," *American Economic Journal: Economic Policy*, 6(1): 1-37

Becker, Gary (1968) "Crime and Punishment: An Economic Approach," *Journal of Political Economy*, 76: 169-217.

Bronson, Jennifer, and Marcus Berzofksky. 2017. "Indicators of Mental Health Problems Reported by Prisoners and Jail Inmates, 2011-12". Bureau of Justice Statistics special report, available at https://www.bjs.gov/content/pub/pdf/imhprpji1112.pdf.

Bronson, Jennifer, Jessica Stroop, Stephanie Zimmer, and Marcus Berzofsky. 2017. "Drug Use, Dependence, and Abuse Among State Prisoners and Jail Inmates, 2007-2009," Bureau of Justice Statistics report, available at https://www.bjs.gov/content/pub/pdf/dudaspji0709.pdf.

Bhuller, Manudeep, Gordon Dahl, Katrine Loken, and Magne Mogstad (2018) "Intergenerational Effects of Incarceration," *AEA Papers and Proceedings* 2018, 108: 234–240.

Bhuller, Manudeep, Gordon Dahl, Katrine Loken, and Magne Mogstad (forthcoming) "Incarceration, Recidivism and Employment," *Journal of Political Economy*.

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 IA, Stern MF, Deyo RA, et al. Release from prison -- a high risk of death for former inmates. *N Engl J Med* 2007;356:157-165

Bondurant, Samuel, Jason Lindo and Isaac Swensen (2018) "Substance Abuse Treatment Centers and Local Crime," *Journal of Urban Economics*, 104: 124-133.

Boylan, Richard and Naci Mocan (2014) "Intended and Unintended Consequences of Prison Reform," *Journal of Law, Economics, and Organization*, 30(3): 558-586.

Buonanno, Paolo and Steven Raphael (2013) "Incarceration and Incapacitation: Evidence from the 2006 Italian Collective Pardon," *American Economic Review*, 103(6): 2437-2465

Campaniello, Nadia, Giovanni Mastrobuoni, and Theodoros Diasakos (2017) "Rationalizable Suicides: Evidence from Changes in Inmates' Expected Length of Sentence," *Journal of the European Economic Association*, 152(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 24186.

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

Doleac, Jennifer (2019) "Encouraging Desistance from Crime" (Previously circulated as IZA Discussion Paper No. 11646.)

Drago, Francesco, Roberto Galbiati, and Pietro Vertova (2009) "The Deterrent Effects of Prison: Evidence from a natural Experiment," *Journal of Political Economy*, 117(2): 257-280

Drago, Francesco, and Roberto Galbiati (2012) "Indirect Effects of a Policy Altering Criminal Behavior: Evidence from the Italian Prison Experiment," *American Economic Journal: Applied Economics, 4(2): 199-218.*

DuRose, Matthew R., Alexia D. Cooper, and Howard N. Snyder. 2014. "Recidivism of Prisoners Released in 30 States in 2005: Patterns from 2005 to 2010." Bureau of Justice Statistics Special Report, NJS 244205.

Fazel, Seena and Jacques Baillargeon (2011) "The Health of Prisoners," *The Lancet*, 377(March), 956-965.

Gertler, Paul, David I. Levine, and Minnie Ames. 2004. "Schooling And Parental Death," *Review of Economics and Statistics*, 86(1):211-225.

Glaze, Lauren E. and Laura M. Maruschak, (2008) "Parents in Prison and Their Minor Children" Bureau of Justice Statistics Special Report, NCJ 222984 U.S. Department of Justice Office of Justice Programs.

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 – 1053.

Hinnerich, Björn Tyrefors, Per Pettersson-Lidbom, and Mikael Priks (2016) "Do Mild Sentences Deter Crime? Evidence using a Regression-Discontinuity Design," working paper.

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

Johnson, Rucker 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): 251-293.

Jones, Mark, Gregory Kearney, Xiaohui Xu, Tammy Norwood, Scott Proescholdbell (2017), "Mortality Rates and Cause of Death Among Former Prison Inmates in North Carolina", *North Carolina Medical Journal*, 78(4), 223-229.

Katz, Lawrence, Stephen D. Levitt, and Ellen Shustorovich (2003) "Prison conditions, capital punishment, and deterrence," *American Law and Economic Review*, 5(2):318—343.

Kling, Jeffrey (2006) "Incarceration Length, Employment and Earnings," *American Economic Review*, 96(3): 863-876.

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

Kriminalvården (The Swedish Prison and Probation Service) (2014) "Utvärdering av Kriminalvårdens Behandlingsprogram: Sammanfattningsrapport" (An Evaluation of The Swedish Prison and Probation Service's Treatment Programs: A Summary Report).

Landerso, Rasmus (2015) “Does Incarceration length Affect Labor market Outcomes,” *Journal of Law and Economics*, 58(February): 205-234.

Lang, Kevin and Jay L. Zagorsky. 2001. “Does Growing up with a Parent Absent Really Hurt,” *Journal of Human Resources*, 36(2):253-273.

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.

Maurin, Eric and Aurelie Ouss (2009) “Sentence Reductions and Recidivism: lessons from the Bastille Day Quasi-Experiment,” IZA Discussion paper No. 3990.

Mueller-Smith, Michael (2015) “The criminal and labor market impacts of incarceration,” working paper.

NIDA (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.

Nilsson, Anders (2002) *Fånge i marginalen: uppväxtvillkor, levnadsförhållanden, och återfall i brott bland fångar* (Prisoners on the margin: childhood background, adult living conditions, and recidivism among Swedish prisoners), Ph.D. dissertation series nr. 8, Department of Criminology, Stockholm University.

Norris, Samuel, Matthew Pecenco and Jeffrey Weaver (2019) “The Effects of Parental and Sibling Incarceration: Evidence from Ohio” Working Paper.

Patterson, Evelyn (2013) “The Dose-Response of Time Served in Prison on Mortality: New York State, 1989-2003,” *American Journal of Public Health*, 103: 523-528.

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,” accessed Aug 14, 2019, https://www.pewtrusts.org/-media/assets/2017/10/sfh_prison_health_care_costs_and_quality_final.pdf

Porter, Lauren (2014) “Incarceration and Post-release Health Behavior,” *Journal of Health and Social Behavior*, 55(2): 234-249.

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): 119-137.

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

Roach, Michael and Max Schanzenbach (2015) “The Effect of Prison Sentence Length on Recividivism: Evidence from Random Judicial Assignment,” *Northwester Law and Econ Research Paper No. 16-08*.

Rose, Evan and Yotam Shem-Tov (2019) “Does Incarceration Increase Crime?” Working paper.

Sailas, Eila, Benjamin Feodoroff, Nina Lindberg, Matti Virkkunen, Reijo Sund, 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): 193-197.

Schnepel, Kevin (2018) “Good Jobs and Recidivism,” *The Economic Journal*, 126: 447-469.

Schnittker, Jason and Andrea John (2007) “Enduring Stigma: The Long-Term Effects of Incarceration on Health,” *Journal of Health and Social Behavior*, 48(2): 115-130.

Schnittker, Jason, Michael Massoglia, and Christopher Uggen (2012) “Out and Down: Incarceration and Psychiatric Disorders,” *Journal of Health and Social Behavior*, 53(4): 448-464.

Skardhamar T, Skirbekk V (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(2): 2- 80.

Swedish Prison and Probation Service (*Kriminalvården*) (2019) “Ökat Klientflöde: Kriminalvårdens Samlade Bedömning och Förslag till Åtgärder”, Kriminalvårdens Digitaltryck, www.kriminalvarden.se.

Tallving, Gustav (2018) ”Research and Effects of Treatment,” Chapter 8 in Tallving, Gustav (editor) *Getting Here: An Anthology on the Swedish Approach to Offenders*, 128-147.

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): 465-481.

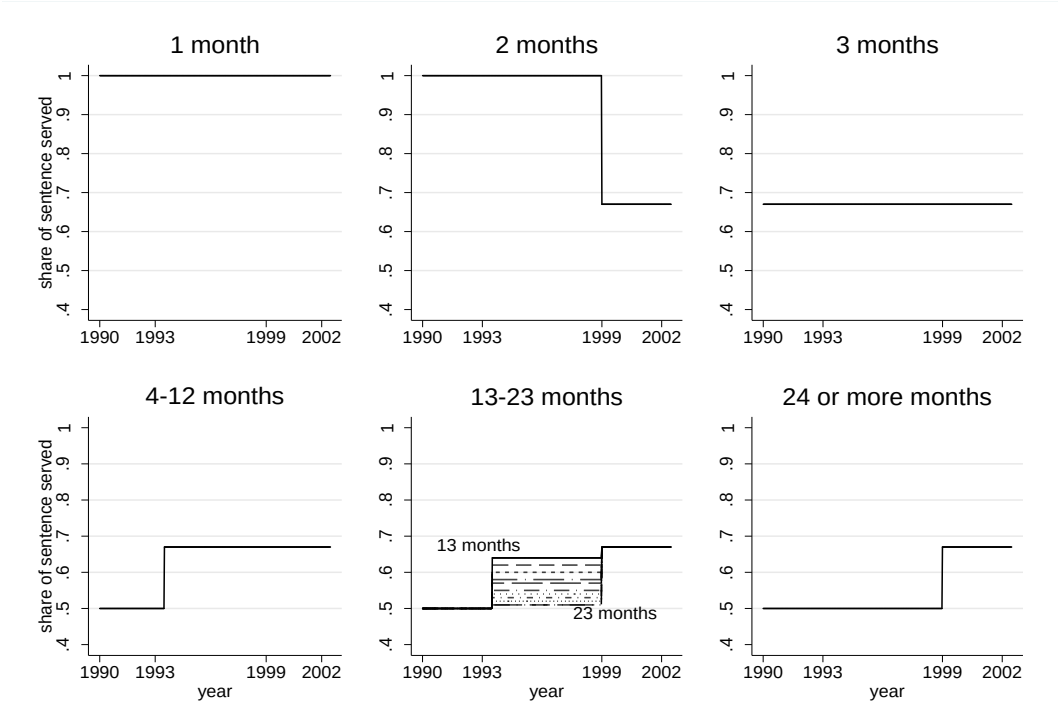
Ulmer, Jeffrey, Megan Kurlychek and John Kramer (2007) “Prosecutorial Discretion and the Imposition of Mandatory Minimum Sentences,” *Journal of Research in Crime and Delinquency*, 44(4): 427-458.

Ugelvik, Thomas and Jane Dullum (Eds.) (2012) *Penal Exceptionalism? Nordic Prison Policy and Practice*, Routledge, Taylor & Francis Group, London and New York.

Vogler, Jacob (2017) “Access to Health Care and Criminal Behavior: Short-Run Evidence from the ACA Medicaid Expansions,” working paper. See : https://ssrn.com/abstract=3042267

Wen, Hefei, Jason Hockenberry, and Janet Cummings (2017) “The Effect of Medicaid Expansion on Crime Reduction: Evidence from HIFA-waiver Expansions,” *Journal of Public Economics*, 154: 67-94.

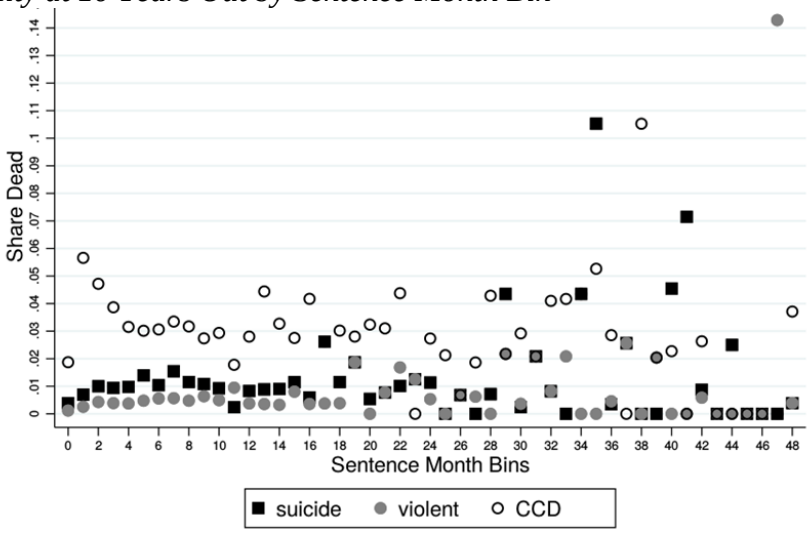
Figure 1. Sweden's Early Release Policies 1990-2002



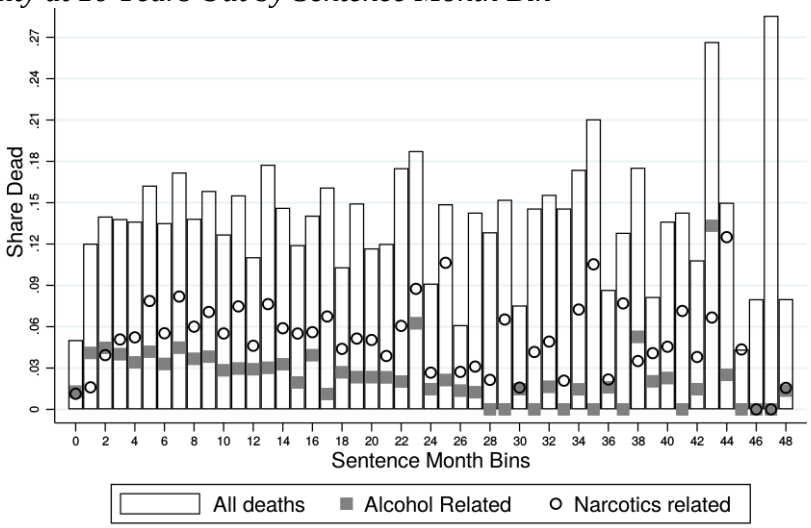
Note – This figure depicts the share of a prison sentence that must be served as stated in Sweden's early release law by sentence length and conviction year. In July 1993, share served was increased for those with sentences of 4 – 12 months in prison from one-half to two-thirds. Those with sentences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

Figure 2. Descriptive Statistics: Post Release Health by Sentence Bin

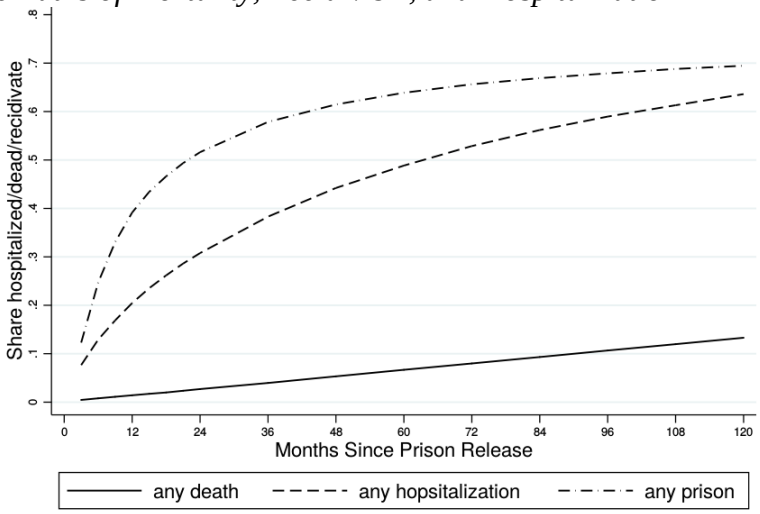
Panel A. Mortality at 10 Years Out by Sentence Month Bin



Panel B. Mortality at 10 Years Out by Sentence Month Bin



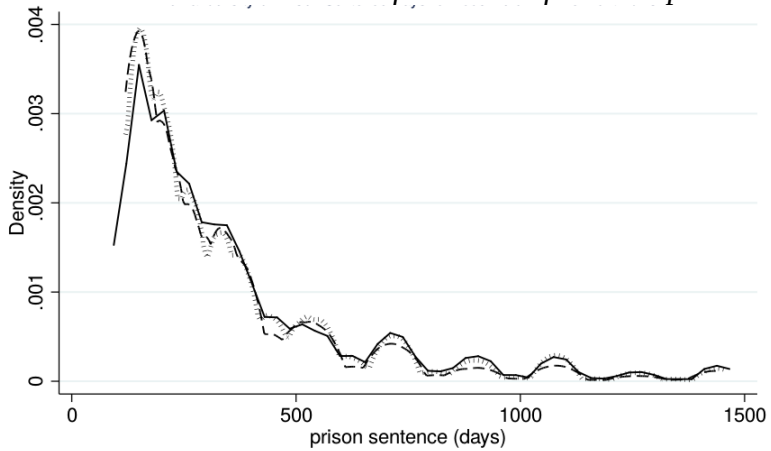
Panel C. Dynamic Paths of Mortality, Recidivism, and Hospitalization



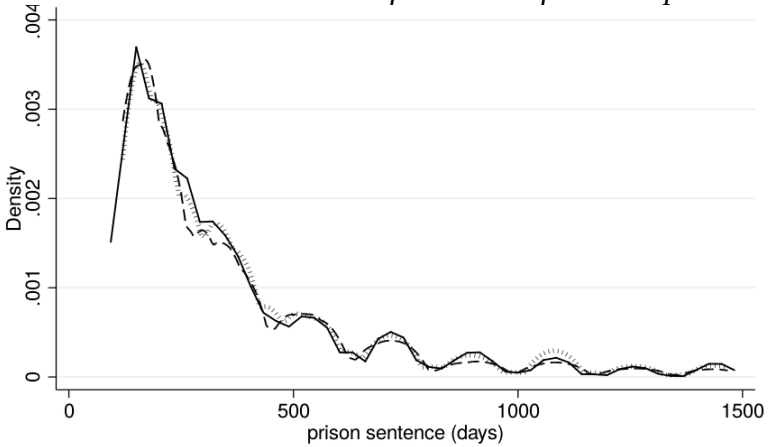
Note –Panels A and B shows the share of individuals in each bin that have died 10 years post release due to specific ICD-10 causes and overall and by coroner flags for alcohol and narcotics on the right. Note that alcohol and narcotics related deaths are not mutually exclusive from each other or other causes of death in this figure. For the all 4-48 month sentences, Panel C shows the dynamic paths for death due to any cause (solid line), any hospitalization (dashed), and any prison (dash-dot), which are measured at *t* months post release, and condition on the sample alive and in Sweden at that time.

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

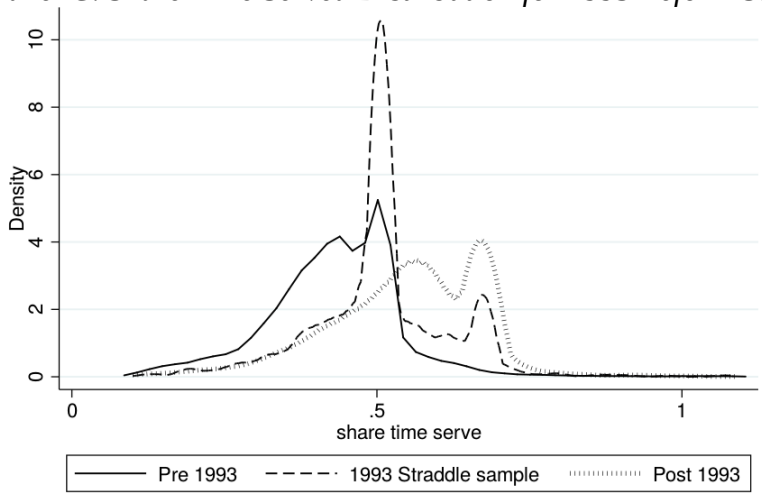
Panel A. Sentence Distribution for 1993 Reform Samples



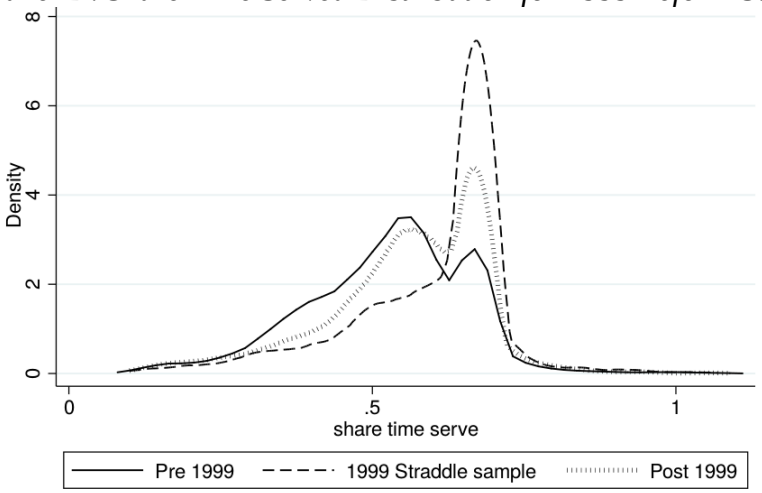
Panel B. Sentence Distribution for 1999 Reform Samples



Panel C. Share Time Served Distribution for 1993 Reform Samples

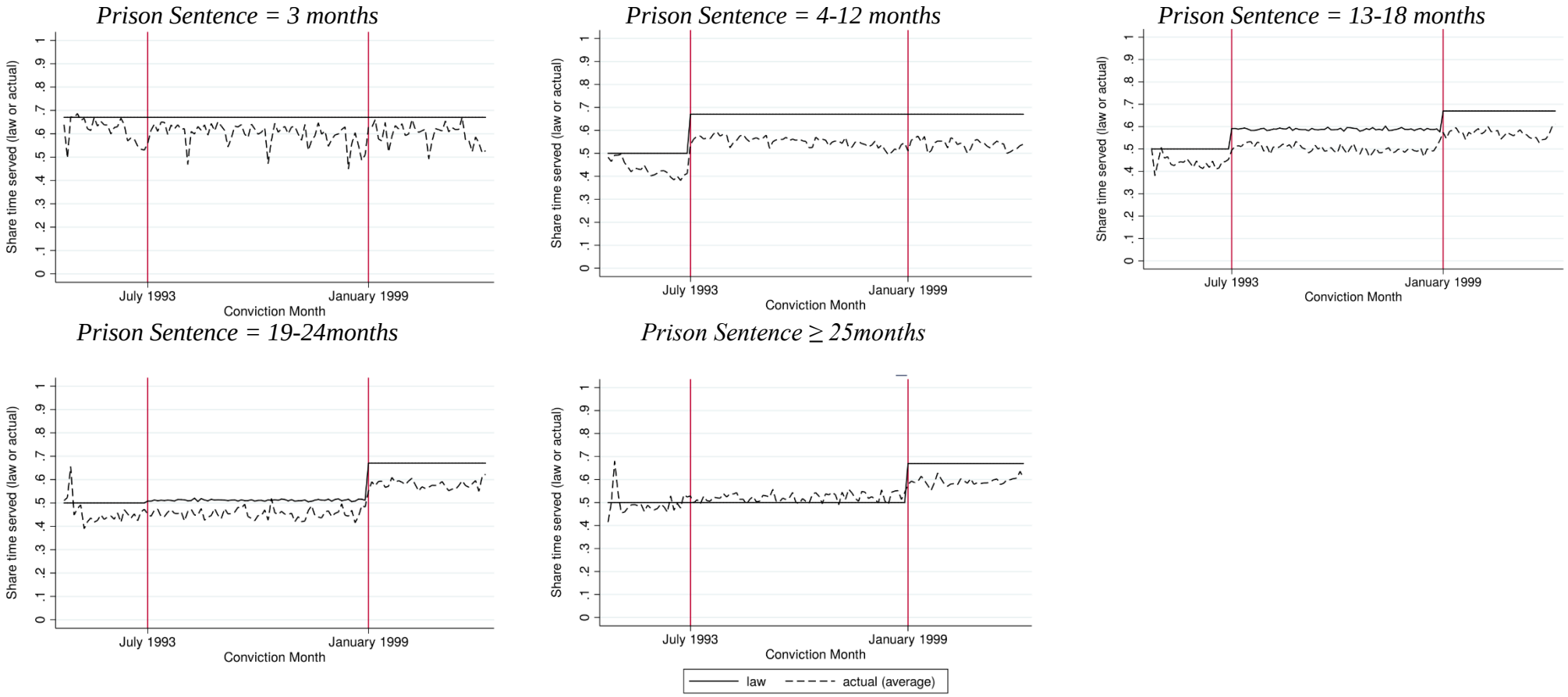


Panel D. Share Time Served Distribution for 1999 Reform Samples



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

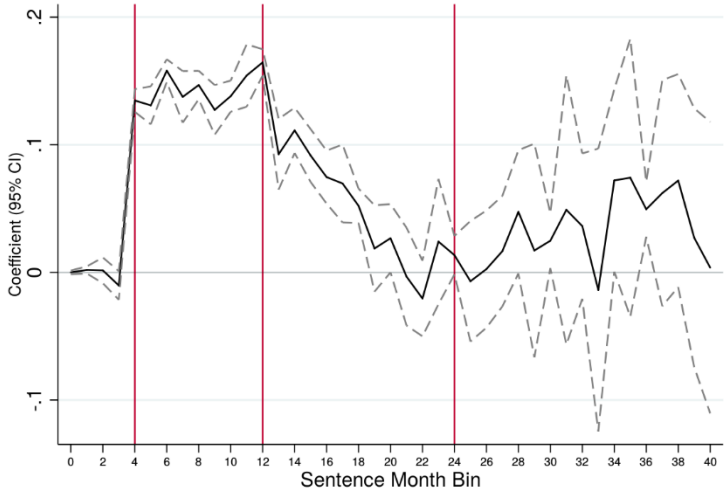
Figure 4. Reform Timing. Is the Reform Implemented Immediately?



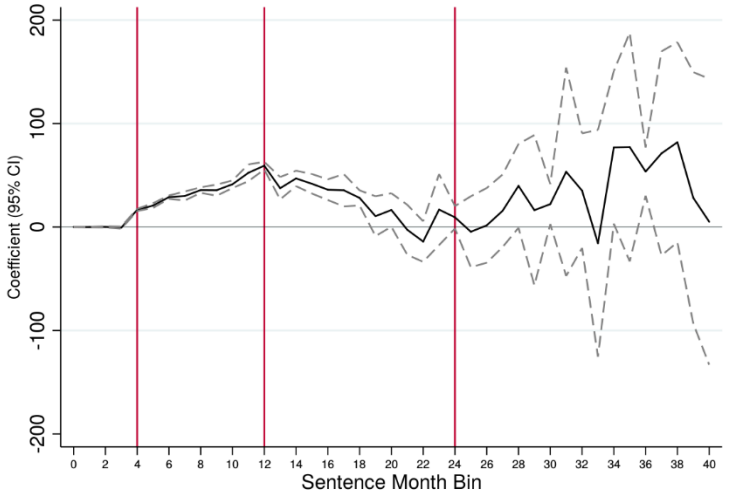
Note – This figure shows the implementation of both reforms for the analysis sample. Sentence bins are grouped according to when they should be treated, and the size of the treatment. Vertical lines correspond to July 1993 and January 1999

Figure 5. Implementation of 1993 and 1999 Reforms: Effect on Share of Time and Actual Days Served.

Panel A. 1993 Share Time Served

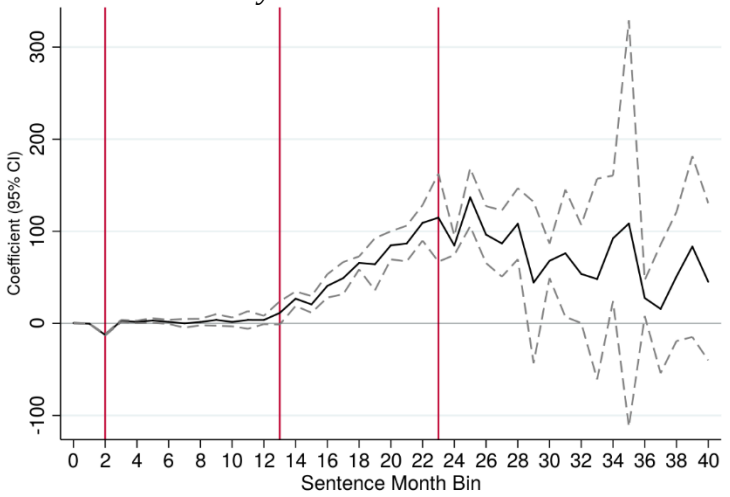


Panel B. 1993: Prison Days



Panel C. 1999 Share Time Served

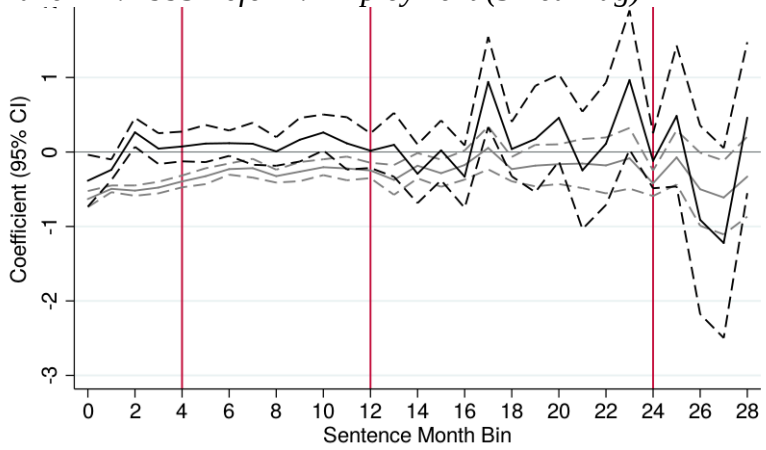
Panel D.1999 Prison Days



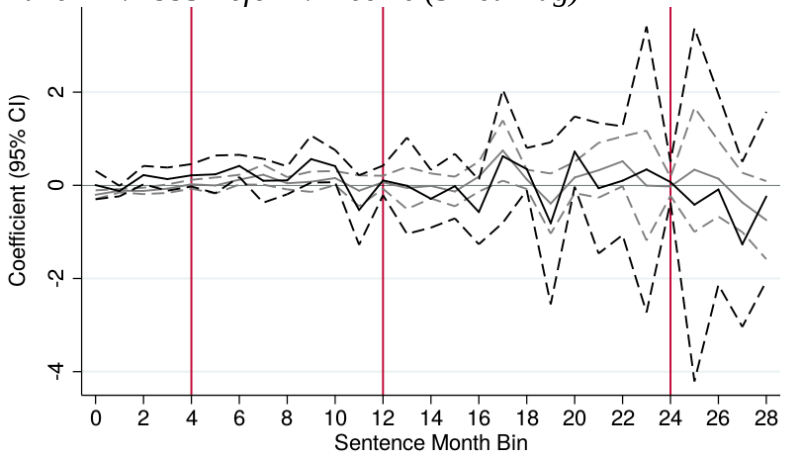
Note – Panels A and C present regressions of the share of time served on post reform dummies for the 1993 and 1999 reforms separately; Panels B and D show the same thing for prison days served. Results are estimated separately for each sentence month bin; the coefficients and 95% confidence interval are plotted.

Figure 6. Changing Observables: Employment and Income

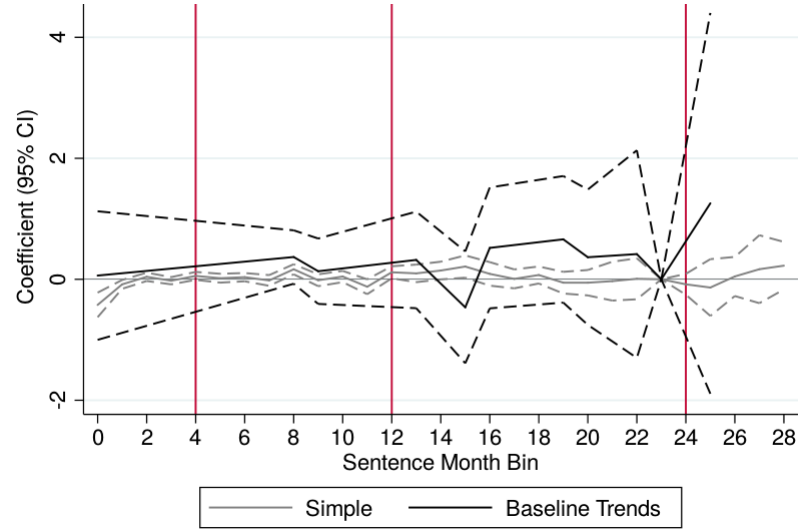
Panel A1. 1993 Reform: Employment (3 Year lag)



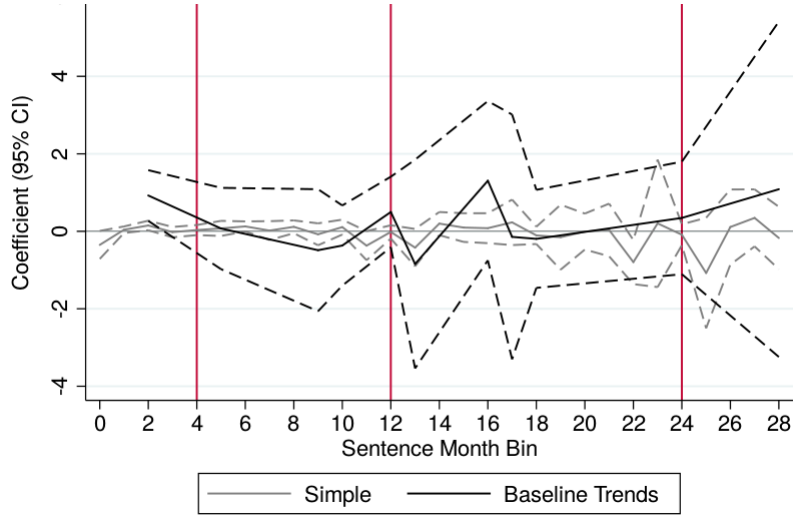
Panel A2. 1993 Reform: Income (3 Year lag)



Panel B1. 1999 Reform: Employment (3 Year lag)



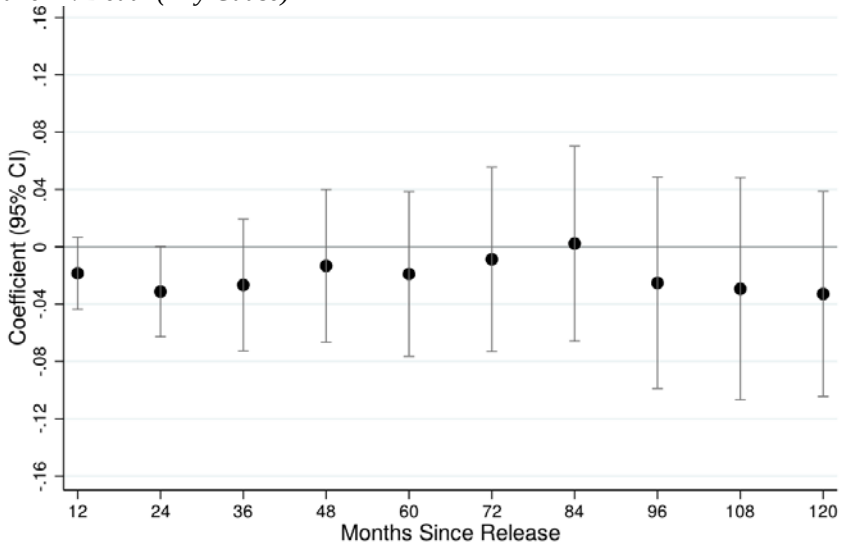
Panel B2. 1999 Reform: Income (3 Year lag)



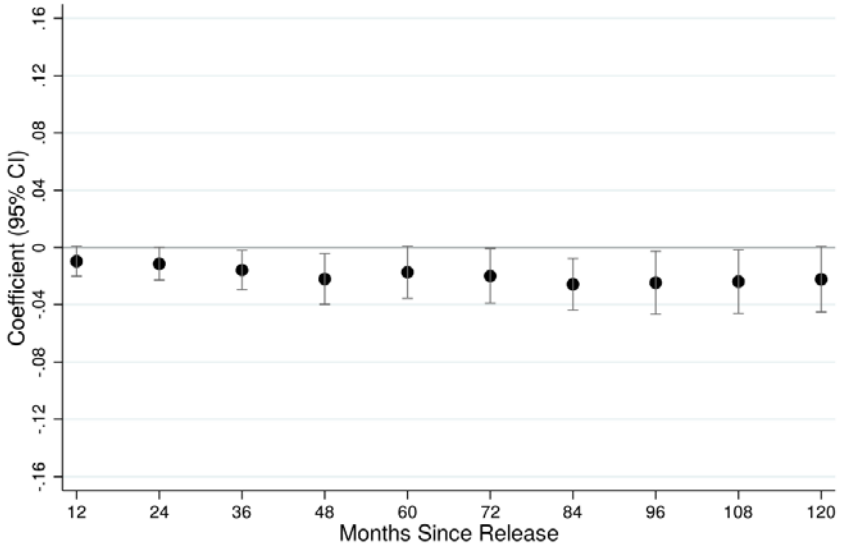
Note – These figures plot the results of regressing observable pre-incarceration employment and income on whether individuals are treated by each reform (the raw difference in gray) and the results (in black) when controlling for trends (using year fixed effects and conviction month trend, as in the baseline specification).

Figure 7. Dynamics of Reduced Form Mortality Effects

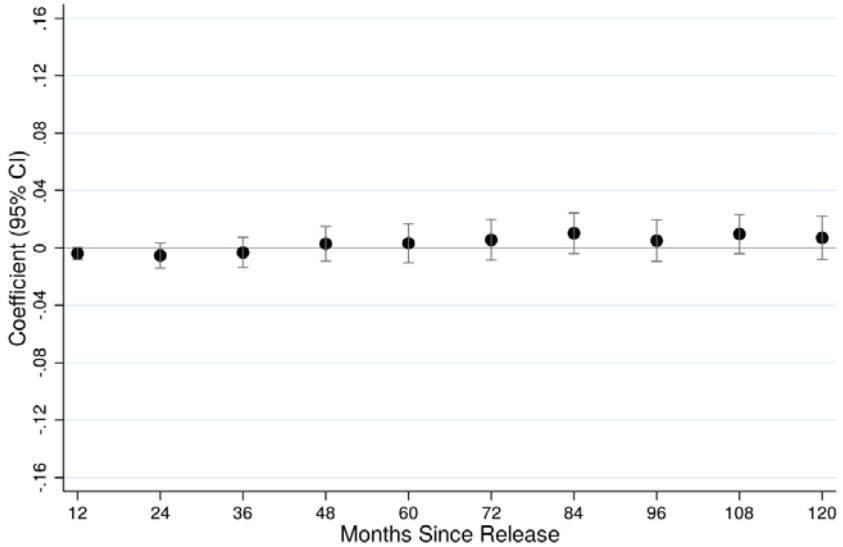
Panel A. Death (Any Cause)



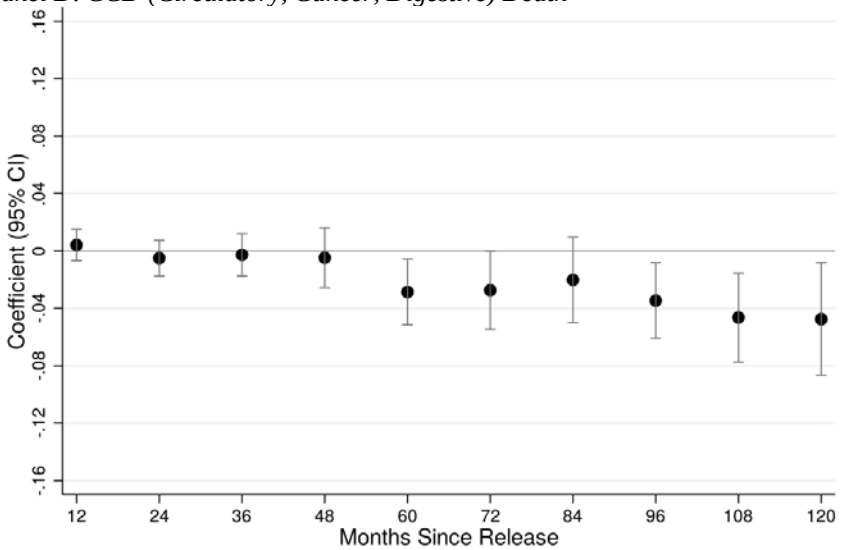
Panel B. Suicide



Panel C. Violent Death



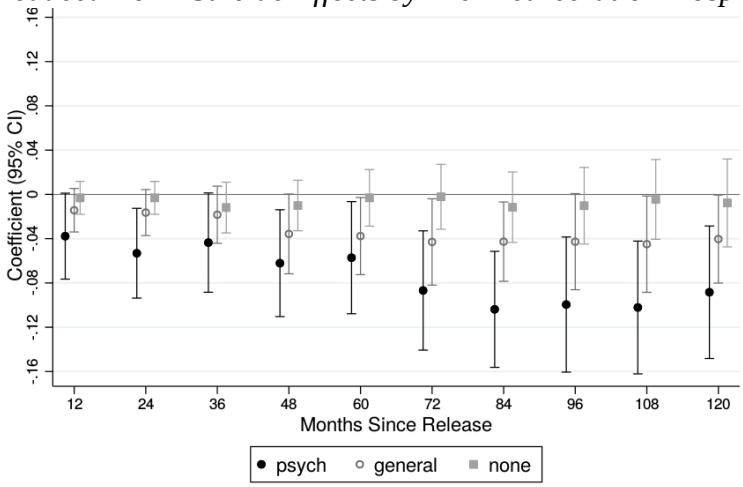
Panel D. CCD (Circulatory, Cancer, Digestive) Death



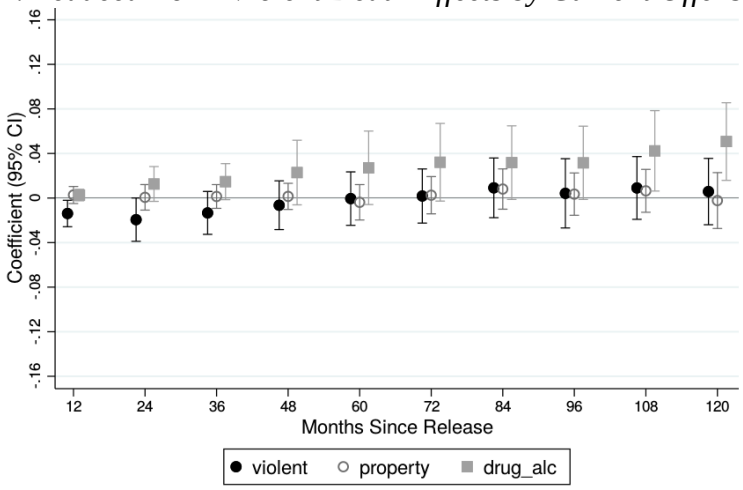
Note – Panels A-D present the reduced form effect and 95% confidence interval of the share of time served law on the outcome listed measured at *t* months since release. All specifications condition on not having migrated from Sweden by month *t*.

Figure 8. Heterogeneity Analysis of Reduced Form Mortality Results

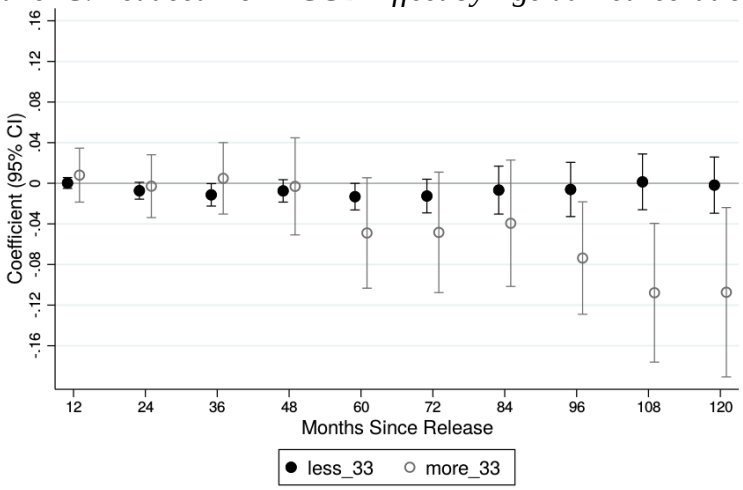
Panel A. Reduced Form Suicide Effects by Pre-Incarceration Hospitalization



Panel B. Reduced Form Violent Death Effects by Current Offense Type

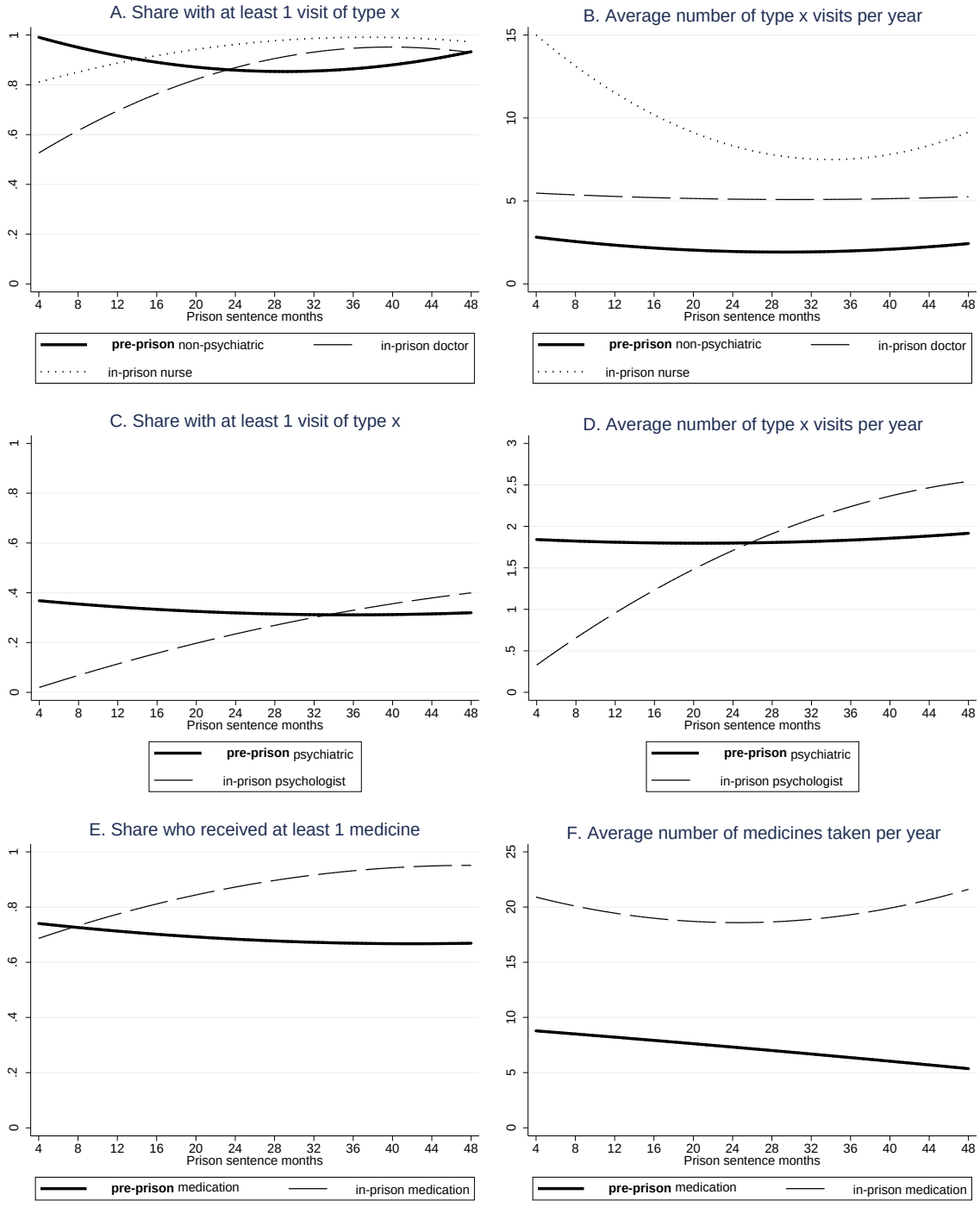


Panel C. Reduced Form CCD Effect by Age at Incarceration



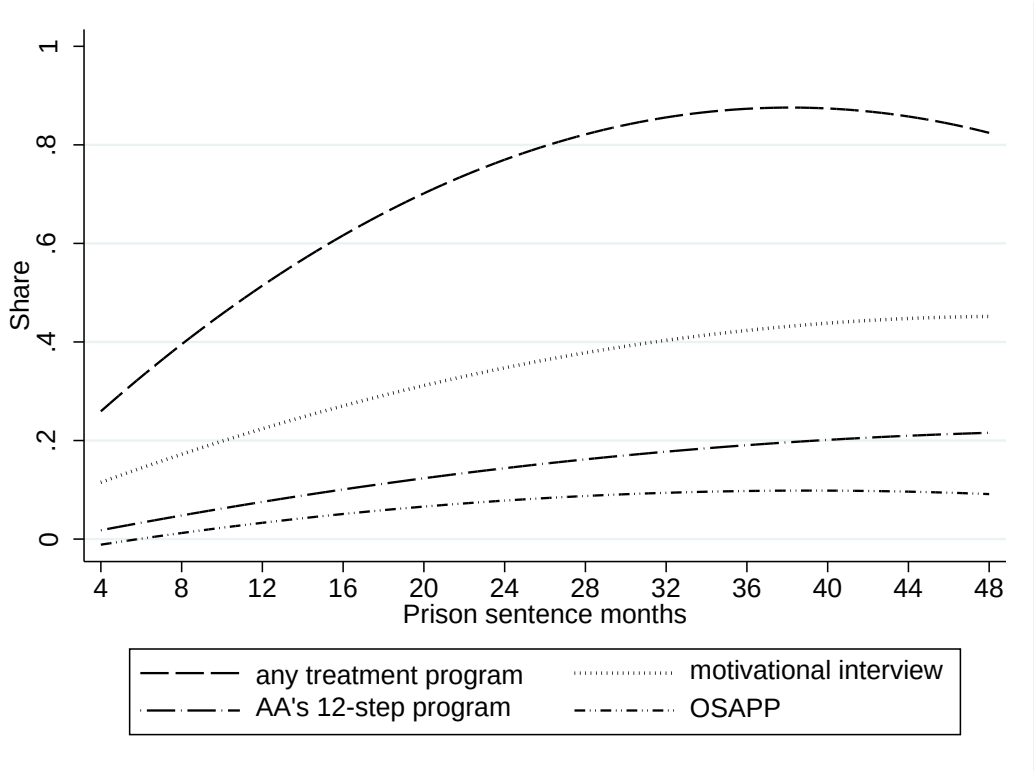
Note – These figures present the reduced form effect of the share of time served law on mortality measured at *t* months since release. Panel A considers suicide and presents the results separately by pre-incarceration hospitalization in a psychiatric ward (N = 9,897 at 12 months), general ward (N = 19,176 at 12 months), or no hospitalization (N = 22073 at 12 months). Panel B considers violent death and presents the results by current offense type: violent (N = 15243), property (N = 20084) and drug and alcohol (N=6968). Panel C presents the CCD (circulatory, cancer, and digestive) results separately by median age at prison admission (<= 30 (N = 23410) and >33 (N = 21150)). All specifications condition on being in Sweden at month *t*.

Figure 9. Pre-Prison and In-Prison Medical Data for All Inmates Entering Prison between 2009 – 2013 and Sentenced to 4 – 48 Months.



Note – All lines are generated by fitting quadratic trends to the data. Pre-prison non-psychiatric visits and pre-prison psychiatric visits include both inpatient and outpatient visits during the two years preceding a prison spell. Pre-prison medication counts the number of unique medicines a person picks up at the pharmacy each day. These numbers are then summed across the 730 days preceding a prison spell. In-prison medication is a count of each time the medical staff administers a single medicine to an inmate. These counts can be quite high for those who take medicine on a regular basis, since inmates are not allowed to self-medicate.

Figure 10. In-Prison Treatment Program Completion Rates for All Inmates Entering Prison between 2009 – 2013 and Sentenced to 4 – 48 Months.



Note – All lines are generated by fitting quadratic trends to the data.

Table 1. OLS Relationships between Prison Days Served and Post-Release Death and Recidivism

	any prison (month 36)				any death (month 36)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sentences:	0-48 months	4-48 months	4-48 months	4-48 months	0-48 months	4-48 months	4-48 months	4-48 months
	no controls	no controls	all controls	plus sentence month bin FE	no controls	no controls	all controls	plus sentence month bin FE
Pre-1993 Sample								
Prison days	0.000492***	-0.000393***	0.000013	0.000276***	0.000023**	-0.000009	0.000012	0.000006
	[0.000033]	[0.000038]	[0.000043]	[0.000104]	[0.000011]	[0.000014]	[0.000019]	[0.000047]
Observations	15908	7911	7911	7911	16406	8214	8214	8214
Between 1993 and 1999								
Prison days	0.000295***	-0.000519***	-0.000059**	0.000023	-0,000001	-0.000024***	-0.000003	-0.000006
	[0.000018]	[0.000022]	[0.000024]	[0.000060]	[0.000007]	[0.000009]	[0.000011]	[0.000028]
Observations	46912	24085	24085	24085	48741	25094	25094	25094
Post 1999								
Prison days	-0.000256***	-0.000471***	-0.000035	0.000046	-0.000029***	-0.000033***	-0.000001	-0.000046
	[0.000027]	[0.000031]	[0.000036]	[0.000108]	[0.000011]	[0.000013]	[0.000017]	[0.000051]
Observations	12921	8430	8430	8430	13513	8808	8808	8808

Note -- This table presents OLS regressions of recidivism and mortality measured at 36 months from release on the number of days served in prison, with varying sets of controls. Column (1) of each panel presents the raw correlation for all sentences less than 48 months, while Columns (2) -(4) are restricted to the 4-48 month analysis sample. Column (3) includes the full set of observable controls to measure criminal justice characteristics, as well as prior pre incarceration criminal history, labor market experience, family status, and general trends in crime. Column (4) adds in controls for sentence month bin fixed effects. Each panel represents a different subsample during which there is *no variation* in the share time served laws, i.e. before the 1993 reform, between the 1993 and 1999 reforms and after the 1993 reform. Thus, all the variation in prison days served is due to differences in offense and case characteristics, differences in sentencing trends over time, credit for time served in häkte, extensions due to miss-behavior, or defaulting on probation, and finally the actual sentence (much of which is captured by case characteristics). Adding fixed effects in column (3) captures much of this variation, but leaves that variation which is potentially very important in terms of outcomes, e.g. reasons for extended sentences. The fact that there is still significant relationships in non-reforms samples suggests that there is a lot of potentially important unexplained variation in time served.

Table 2. Summary Statistics

	Main Analysis Sample									
	0-3 Month Bins N=39294		**4-48 Month Bins N=46815**		4-12 Month Bins N=33799		13-24 Month Bins N=8968		25-48 Month Bins N=4048	
variable	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
prison sentence months	1.54	0.88	11.71	9.15	7.16	2.65	18.09	3.62	35.59	6.91
prison sentence days	48.15	25.39	353.92	274.22	217.49	79.58	545.19	107.75	1069.38	206.62
Early release law (share time served law)	0.92	0.14	0.62	0.07	0.64	0.07	0.58	0.07	0.55	0.08
share time served	0.88	0.2	0.52	0.13	0.53	0.14	0.5	0.11	0.53	0.1
prison days	38.89	15.92	184.55	154.38	114.64	53.17	271.68	76.49	575.23	169.78
assumed conviction year	1995.01	2.71	1995.98	2.94	1995.92	2.91	1996.09	2.99	1996.18	3.04
dui	0.37	0.48	0.03	0.17	0.04	0.2	0.01	0.08	0	0.04
drugs alcohol	0.06	0.24	0.15	0.36	0.13	0.34	0.17	0.37	0.28	0.45
traffic	0.06	0.24	0.01	0.12	0.02	0.14	0.01	0.07	0	0.02
property	0.2	0.4	0.43	0.5	0.49	0.5	0.35	0.48	0.18	0.38
violent	0.24	0.43	0.33	0.47	0.28	0.45	0.43	0.5	0.52	0.5
other	0.07	0.25	0.04	0.2	0.04	0.2	0.04	0.19	0.02	0.16
number crimes contemporaneous	3.25	5.04	7.17	7.02	7.55	7.1	6.77	7.03	4.91	5.71
number crimes past	29.98	47.76	59.51	68.61	63.3	69.18	56.15	69.9	35.27	54.16
number prison past	3.03	4.77	5.54	6.86	5.9	7.02	5.17	6.71	3.29	5.12
any post trial detention	0.24	0.43	0.72	0.45	0.68	0.47	0.79	0.41	0.87	0.34
male	0.94	0.23	0.95	0.22	0.95	0.21	0.95	0.22	0.95	0.22
Swedish citizen	0.84	0.37	0.82	0.39	0.82	0.38	0.81	0.39	0.77	0.42
born Sweden	0.78	0.41	0.76	0.42	0.78	0.42	0.75	0.44	0.68	0.47
age at prison start	35.82	11.45	33.81	9.57	33.94	9.44	33.28	9.79	33.82	10.1
primary school_lag1	0.12	0.33	0.1	0.3	0.1	0.3	0.1	0.3	0.1	0.29
short high school_lag1	0.33	0.47	0.41	0.49	0.41	0.49	0.41	0.49	0.36	0.48
long high school_lag1	0.45	0.5	0.39	0.49	0.39	0.49	0.38	0.49	0.42	0.49
married_lag1	0.23	0.42	0.2	0.4	0.19	0.39	0.21	0.41	0.24	0.43
number children at conviction	1.18	1.4	1.07	1.34	1.06	1.32	1.07	1.34	1.18	1.46
employed in november_lag1to3	1.09	1.22	0.58	0.96	0.55	0.94	0.61	0.98	0.78	1.08
log average income_lag1to3	10.95	1.17	10.44	1.39	10.42	1.38	10.4	1.44	10.63	1.34
hospital days alcohol_lag1to3	0.42	3.79	0.4	3.48	0.44	3.7	0.32	2.97	0.21	2.48
hospital days narcotics_lag1to3	0.51	5.66	0.84	7.93	0.96	8.67	0.53	5.62	0.46	5.33
hospital days psychiatric_lag1to3	2.79	22.57	5.38	39.26	5.56	38.23	5.34	43.23	3.94	38.5
hospital days other_lag1to3	2.47	9.34	2.62	10.76	2.68	9.83	2.52	10.56	2.4	16.86

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

Table 3. Main First Stage Table

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Prison Sentence Days				
	Drop Individuals Dead or Emigrated by *x* years after End Date				
	All sample	1 year post release	2 years post release	3 years post release	10 years post release
Panel A: Baseline Specification (Includes Full Set of Controls)					
law_share_sent	271.037	274.163	274.163	275.342	274.481
	(25.771)***	(26.102)***	(25.785)***	(25.524)***	(25.462)***
F-stat	111	110	113	116	116
Panel B: Baseline Minus All Controls (Just Bin Fixed Effects, Year FE, and Conv Month trend)					
law_share_sent	272.825	275.57	275.874	276.91	277.272
	(25.616)***	(25.984)***	(25.686)***	(25.511)***	(25.432)***
F-stat	113	112	115	118	119
Cumulative # died	0	659	1242	1812	5996
Cumulative # emigrated	0	527	879	1183	2185
Observations	46815	45629	44694	43820	38634

Each column includes sample alive and NEVER Emigrated from Sweden X years since date of release. Panel A (the baseline spec) includes controls fo 60+ crime type dummies, # current offense, # past crimes, # past prison admissions, age <=21, and age at prison, as well as calendar month dummies of conviction and court dummies, and demographics and socioeconomic and health characteristics at time of incarceration, including: male swedish_citizen born_sweden i.education_lag1 married_lag1 number_children_at_sentence employed_november_lag1to3 income_average_lag1to3_REG Dincome_lag1_3, hospital_alcohol_days_lag1to3 hospital_narcotics_days_lag1to3 hospital_psych_days_lag1to3 hospital_other_days_lag1to3. Panel B drops all observable controls. Standard errors clustered on sentence month bin.

Table 4. First stage heterogeneity table: monotonicity

	Coefficient	Std Error	F	N
Baseline Sample	271.037	(25.771)***	111	46815
# Current Charges				
4 or less curent crimes	271.560	(27.722)***	96	21146
more than 4 current crimes	267.803	(26.476)***	102	25669
Prison History				
no past prison	266.822	(33.781)***	62	8073
1 past prison	328.963	(34.303)***	92	9014
past prison more than 1	252.472	(26.174)***	93	29728
first 4-48 month sentence post 1992	244.678	(29.394)***	69	27165
last 4-48 month sentence pre 2002	312.132	(28.530)***	129	27180
Current Offense Broad Crime Category				
drugs alcohol	316.011	(23.274)***	184	7065
property	248.162	(24.693)***	101	20245
violent	282.147	(31.696)***	79	15484
Demographic And Socioeconomic Characteristics				
not born in sweden	273.588	(28.496)***	92	11069
born in sweden	272.062	(27.044)***	101	35746
younger 33	270.025	(30.007)***	81	22929
older 33	271.488	(24.590)***	122	23886
unemployed last 3 years	309.500	(25.307)***	150	31584
employed at least once in last 3 years	222.301	(31.043)***	51	15231
married	277.273	(24.079)***	133	9150
unmarried	268.728	(28.032)***	92	37665
male	271.519	(26.439)***	105	44499
female	239.143	(22.178)***	116	2316
no children	267.521	(28.741)***	87	21677
children	273.461	(24.594)***	124	25138
Pre Incarceration Hospitalization				
at least one psychiatric hospitalization last 5 years	284.216	(29.729)***	91	9995
general ward hospital hospitalization last 5 years	271.701	(24.672)***	121	19348
no hospitalization last five years	265.555	(29.674)***	80	22375
Reform SubSamples				
+/- 2 years of 1993 reform	134.006	(37.527)***	13	19130
+/- 2 years of 1999reform	428.866	(38.701)***	123	16930

Each row presents first stage estimates for different subsample; i.e. regressions of prison days served on share time that should be served according to the law. The baseline specification, with the full set of controls, is used. F-statistics on the instrument (share of time served law) are reported in the third columns. Standard errors clustered on sentence month bin.

Table 5. Reduced Form and IV Estimates of Prison Days Served on Mortality Overall and by Cause

	Reduced Form Estimates at month				IV Estimates at month			
Dep Variable	12	24	36	120	12	24	36	120
Death (Any Cause)	-0.019	-0.031*	-0.027	-0.033	-0.000068	-0.000114**	-0.000097	-0.000118
(all controls)	[0.012]	[0.016]	[0.023]	[0.035]	[0.000044]	[0.000055]	[0.000080]	[0.000122]
Death by Type (ICD Codes):								
Suicide	-0.010*	-0.011**	-0.016**	-0.022*	-0.000035*	-0.000042**	-0.000058**	-0.000080**
	[0.005]	[0.006]	[0.007]	[0.011]	[0.000019]	[0.000021]	[0.000025]	[0.000041]
Violent	-0.004*	-0.005	-0.003	0.007	-0.000014*	-0.00002	-0.000012	0.000025
	[0.002]	[0.004]	[0.005]	[0.007]	[0.000008]	[0.000016]	[0.000019]	[0.000027]
Cancer/Circ/Digestive	0.004	-0.005	-0.003	-0.048**	0.000015	-0.000019	-0.00001	-0.000172***
	[0.005]	[0.006]	[0.007]	[0.019]	[0.000020]	[0.000022]	[0.000026]	[0.000062]
Death by Type (Coroner Flags):								
Alcohol/Narcotics Exclusive	-0.008	-0.003	0.001	0.025	-0.000028	-0.000012	0.000003	0.00009
	[0.008]	[0.011]	[0.014]	[0.020]	[0.000027]	[0.000040]	[0.000051]	[0.000071]
Share dead	0.014	0.027	0.040	0.13	0.014	0.027	0.040	0.13
Share suicide	0.0013	0.0022	0.0034	0.010	0.0013	0.0022	0.0034	0.010
Share Violent death	0.00069	0.0011	0.0018	0.0050	0.00069	0.0011	0.0018	0.0050
Share CCD Death	0.0019	0.0039	0.0061	0.030	0.0019	0.0039	0.0061	0.030
Share Alc/Narc Only Death	0.0072	0.0136	0.0200	0.0601	0.0072	0.0136	0.0200	0.0601
N	46287	45934	45629	44560	46287	45934	45629	44560

Note – The panel on the left presents the reduced form effect of share of time served prescribed by the law on each mortality outcome listed in column (1), for the baseline specification. The panel on the right presents the instrumental variable estimates of the effect of an additional day served in prison on each mortality outcome. The means of the dependent variables are presented at the bottom of the table. All specifications include the full set of controls. Standard errors clustered on sentence month bin in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6. Reduced Form Mortality Results: Heterogeneity by Reform

	1993 Reform Sample at Month:				1999 Reform Sample at Month:			
Dep Variable	12	24	36	120	12	24	36	120
Death (Any Cause)	-0.015	-0.008	-0.017	-0.006	-0.032	-0.079**	-0.057	-0.071
(all controls)	[0.014]	[0.019]	[0.022]	[0.046]	[0.024]	[0.034]	[0.043]	[0.089]
Death by Type:								
Suicide	-0.014**	-0.011*	-0.019**	-0.026*	0.004	-0.007	-0.009	-0.018
	[0.006]	[0.006]	[0.008]	[0.016]	[0.010]	[0.010]	[0.011]	[0.023]
Violent	-0.006*	-0.006	-0.007	0.015	-0.005	-0.005	-0.002	-0.003
	[0.003]	[0.006]	[0.007]	[0.011]	[0.007]	[0.008]	[0.009]	[0.017]
Cancer/Circ/Digestive	0.006	-0.004	-0.001	-0.029	-0.003	-0.015	-0.012	-0.068
	[0.010]	[0.014]	[0.016]	[0.035]	[0.013]	[0.018]	[0.017]	[0.051]
Alcohol/Narc Exclusive	0.005	0.023	0.031	0.058	-0.025	-0.04	-0.041	0
	[0.009]	[0.015]	[0.022]	[0.043]	[0.019]	[0.034]	[0.039]	[0.059]
N	18891	18731	18596	17927	16768	16660	16559	16369

Note –The panel on the left presents the reduced form effect of share of time served prescribed by the law on each mortality outcome for the 1993 reform sample, while the panel on the right presents analogous results for the 1999 reform sample. All specifications include the full set of controls. Standard errors clustered on sentence month bin in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7. Reduced Form and IV Estimates for Recidivism

Reduced Form Estimates			**IV Estimates**		
12	24	36	12	24	36
		Dep Var = Any Conviction at Month t			
-0.089*	-0.103	-0.034	-0.000324*	-0.000374	-0.000123
[0.046]	[0.074]	[0.062]	[0.000166]	[0.000262]	[0.000220]
0.563	*0.700*	*0.758*	*0.563*	*0.700*	*0.758*
		Dep Var = > 1 Conviction at Month t			
-0.098**	-0.101*	-0.145**	-0.000356**	-0.000366*	-0.000526**
[0.039]	[0.051]	[0.061]	[0.000157]	[0.000193]	[0.000224]
0.314	*0.514*	*0.607*	*0.314*	*0.514*	*0.607*
		Dep Var = Any Prison at Month t			
-0.170**	-0.117*	-0.103	-0.000619**	-0.000428*	-0.000374
[0.068]	[0.065]	[0.070]	[0.000273]	[0.000246]	[0.000259]
0.391	*0.516*	*0.579*	*0.391*	*0.516*	*0.579*
45626	44691	43817	18662	18294	17928

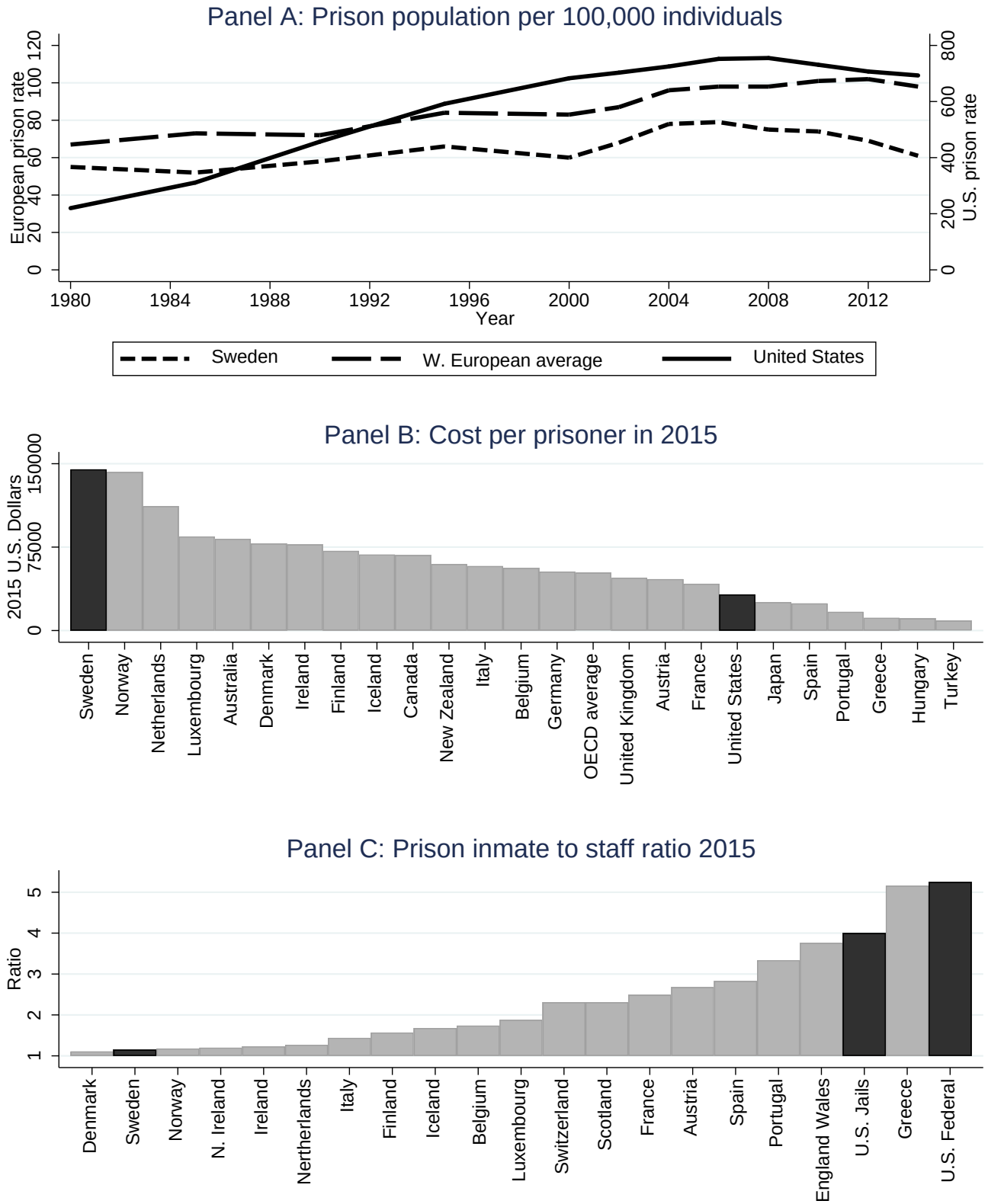
Note -- The left hand side presents the reduced form effects of the prescribed share of time served on three measures of recidivism (any conviction, more than 1 conviction, and return to prison) measured at 12, 24, and 36 months post-release. All specifications condition on the sample that is alive and in Sweden at time t. Standard errors, clustered at the prison sentence month bin level, are in brackets. The mean of the dependent variable is in italics. The right-hand side presents the corresponding instrumental variable estimates of the effect of an additional prison day served on recidivism. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8. Heterogeneity in the Effect of Prison Exposure on Recidivism: By Reform and Case Characteristics

Full Sample: Both reforms at month			1993 Reform Sample at month			1999 Reform Sample at month		
12	24	36	12	24	36	12	24	36
Full Sample (Baseline)								
-0.170**	-0.117*	-0.103	-0.208**	-0.179**	-0.176***	0.059	0.161	0.199
[0.068]	[0.065]	[0.070]	[0.080]	[0.074]	[0.058]	[0.100]	[0.125]	[0.137]
0.391	*0.516*	*0.579*	*0.398*	*0.515*	*0.576*	*0.386*	*0.518*	*0.582*
45626	44691	43817	18662	18294	17928	16489	16153	15832
Current Offense = Property								
-0.331***	-0.321***	-0.267***	-0.196	-0.176	-0.159*	-0.174	-0.153	-0.166
[0.088]	[0.078]	[0.065]	[0.118]	[0.115]	[0.091]	[0.204]	[0.183]	[0.152]
19794	19394	19022	8649	8487	8315	6694	6555	6433
Current Offense = Violent								
-0.088	-0.005	0.007	-0.12	-0.08	-0.055	0.01	0.253	0.3
[0.071]	[0.090]	[0.096]	[0.118]	[0.135]	[0.116]	[0.105]	[0.186]	[0.216]
15015	14701	14397	5930	5804	5683	5616	5511	5392
Young (Age <= 33)								
-0.113	-0.055	-0.078	-0.057	-0.044	-0.066	0.077	0.263	0.239
[0.079]	[0.077]	[0.080]	[0.087]	[0.085]	[0.075]	[0.136]	[0.178]	[0.210]
24118	23679	23267	10512	10331	10155	8207	8055	7919
Old (Age > 33)								
-0.233***	-0.201**	-0.147	-0.412***	-0.346***	-0.315***	0.031	0.025	0.108
[0.086]	[0.089]	[0.098]	[0.109]	[0.099]	[0.116]	[0.127]	[0.143]	[0.123]
21508	21012	20550	8150	7963	7773	8282	8098	7913
Pre-Incarceration Psych Hosptial Admission								
-0.203	-0.123	-0.155	-0.012	0.058	0.12	-0.494*	-0.138	-0.187
[0.150]	[0.126]	[0.145]	[0.240]	[0.220]	[0.245]	[0.246]	[0.337]	[0.303]
9637	9363	9106	3759	3653	3552	3620	3526	3431
Pre-Incarceration General Hospital Admission								
-0.161*	-0.061	-0.119	-0.253**	-0.177	-0.239*	0.123	0.317	0.185
[0.095]	[0.104]	[0.099]	[0.119]	[0.132]	[0.138]	[0.172]	[0.193]	[0.239]
18803	18364	17916	7523	7345	7158	6944	6784	6611
Pre-Incarceration No Hospital Admission								
-0.093	-0.087	-0.051	-0.162*	-0.175*	-0.183**	0.176	0.169	0.298***
[0.070]	[0.071]	[0.071]	[0.093]	[0.098]	[0.081]	[0.108]	[0.109]	[0.109]
21887	21514	21203	9145	8994	8865	7735	7604	7495

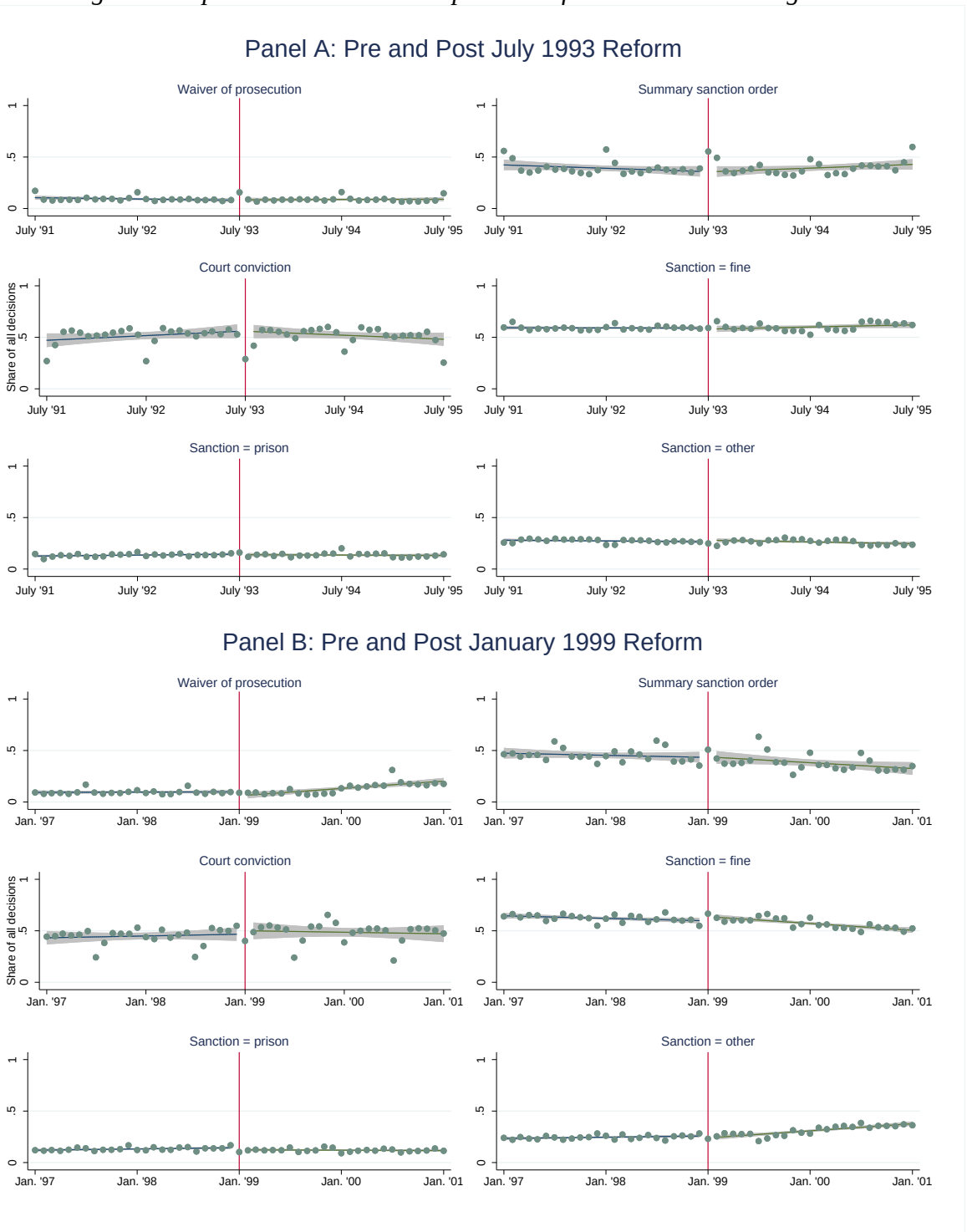
This table presents the reduced form effects of the prescribed share of time served on recidivism, measured by a return to prison, at 12, 24, and 36 months post-release. All specifications condition on the sample that is alive and in Sweden at time *t*. Standard errors, clustered at the prison sentence month bin level, are in brackets; below are sample sizes for each specification. Columns (1)-(3) present the results for the whole sample, while the 4th-6th and 7th - 9th columns present the results for the 1993 and 1999 reform subsamples, respectively. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Figure 1. Swedish Prisons in International Perspective



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

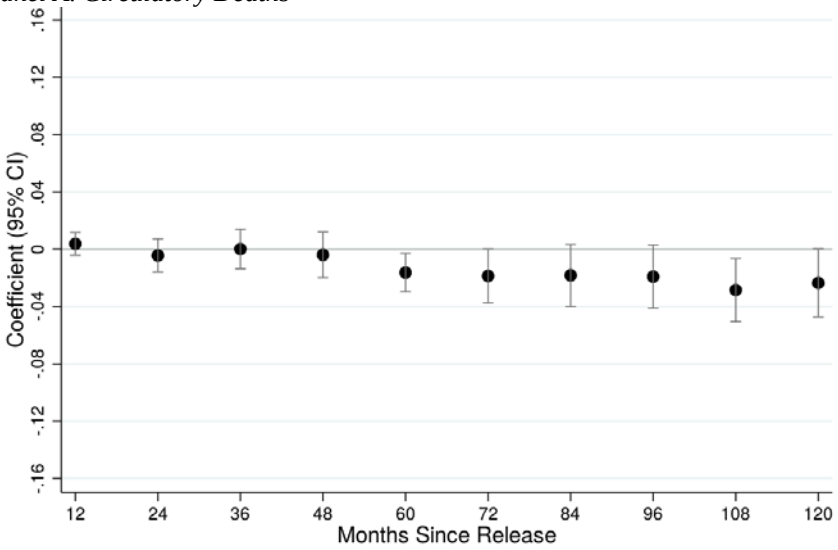
Appendix Figure 2. Implementation: No Manipulation of Prosecutor and Judge Decisions



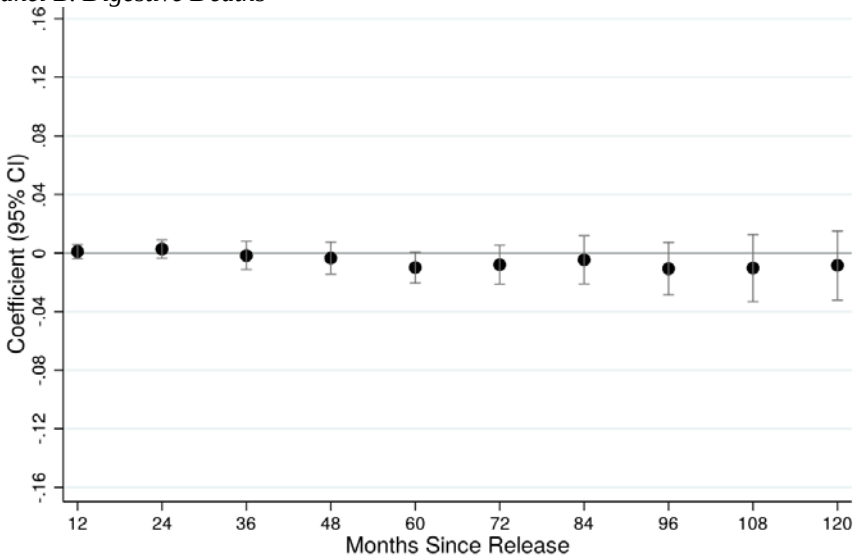
Note – This figure was created using the universe of all decision types and convictions included in the official convictions register for those aged 18 or older in the four years around each reform. We then collapsed these data by the month that each decision or conviction was made. We plot these data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

Appendix Figure 3. Dynamics of Reduced Form Mortality Effects

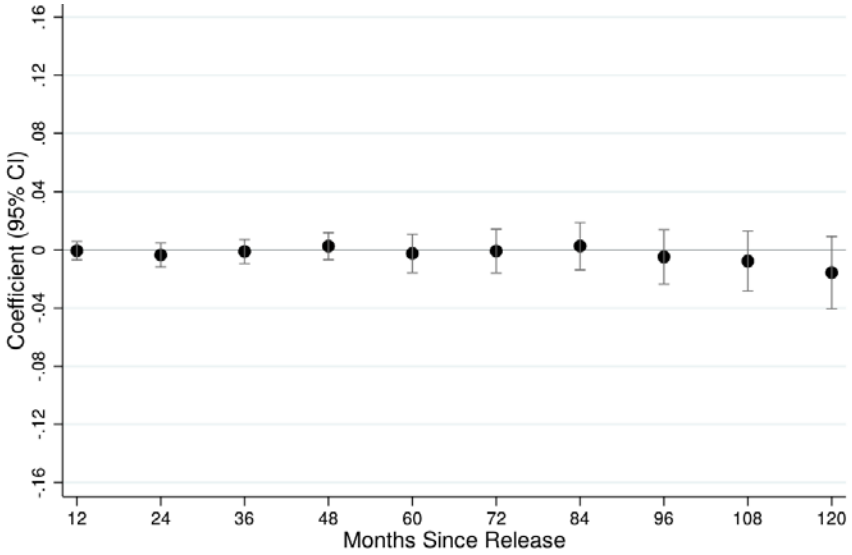
Panel A. Circulatory Deaths



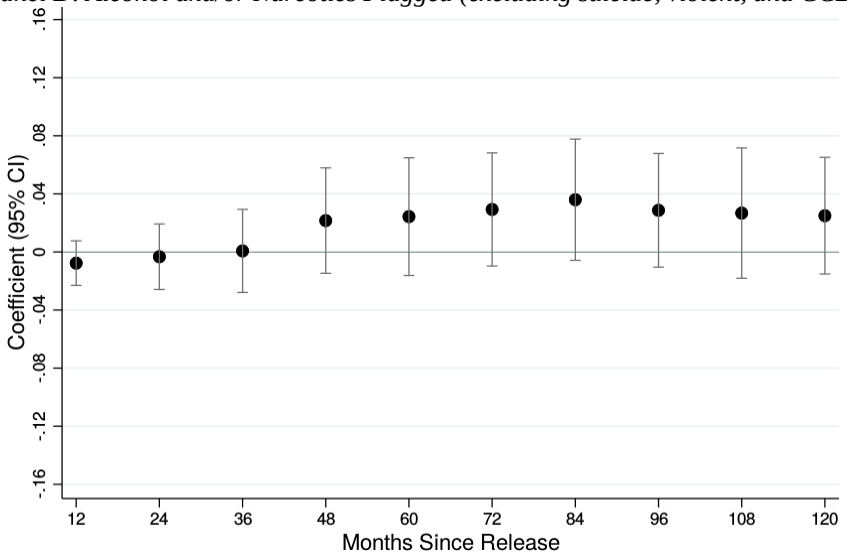
Panel B. Digestive Deaths



Panel C. Cancer



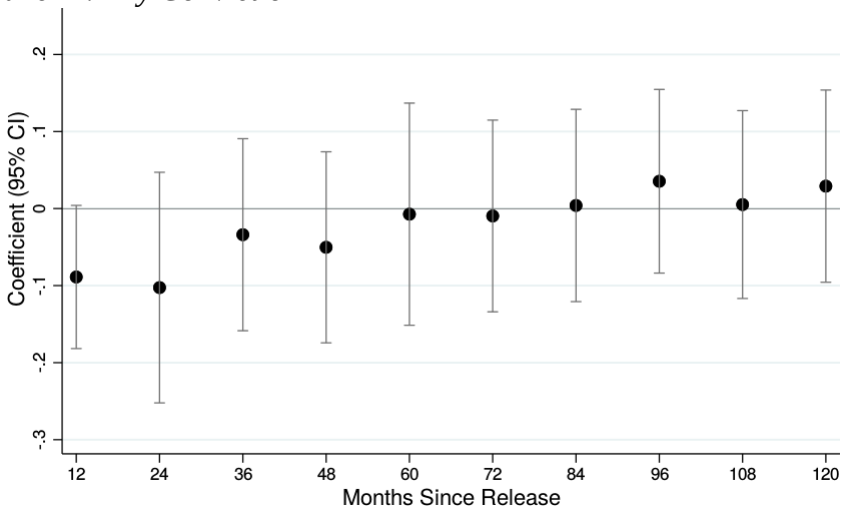
Panel D. Alcohol and/or Narcotics Flagged (excluding suicide, violent, and CCD)



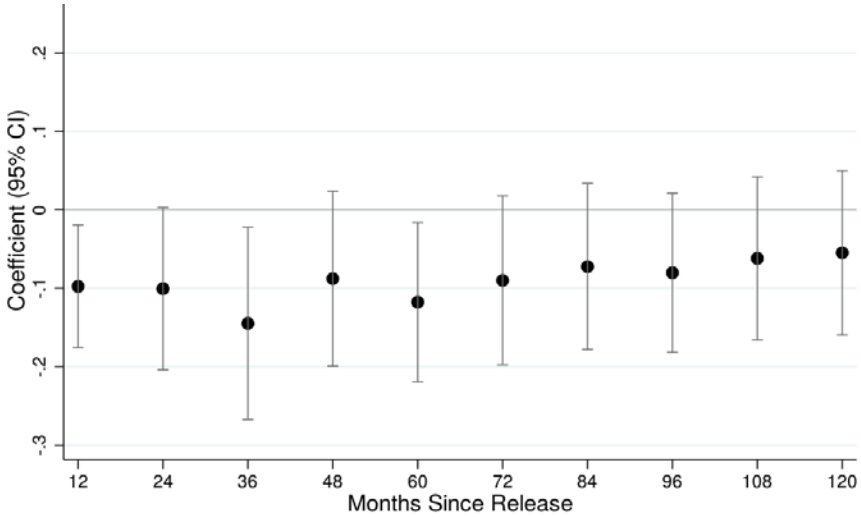
Note – Panels A-D present the reduced form effect and 95% confidence interval of the share of time served law on the outcome listed measured at *t* months since release. All specifications condition on not having migrated from Sweden by month *t*.

Appendix Figure 4. Dynamics of Reduced Form Recidivism Effects

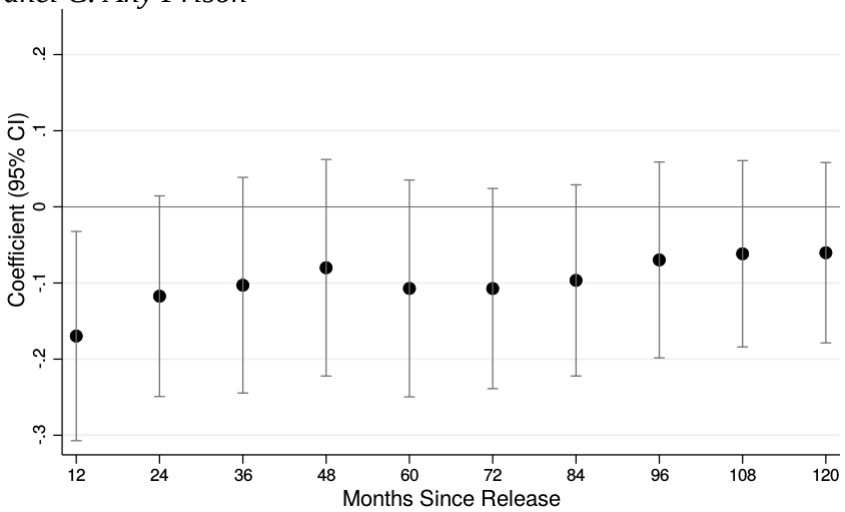
Panel A. Any Conviction



Panel B. More than One Conviction

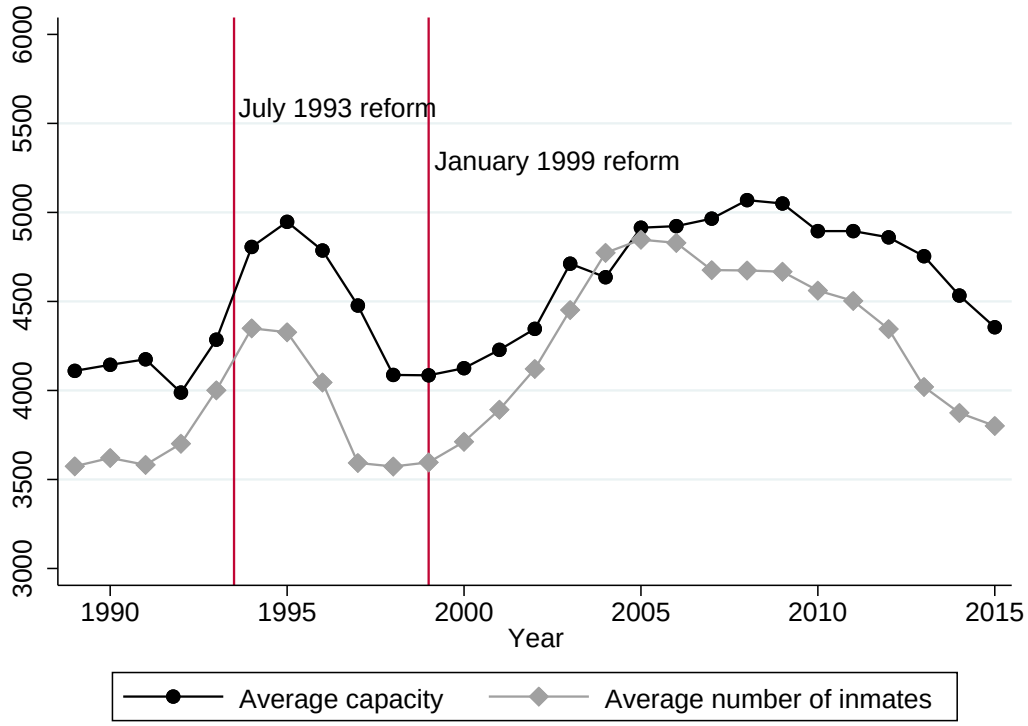


Panel C. Any Prison



Note – Panels A-C present the reduced form effect and 95% confidence interval of the share of time served law on the outcome listed measured at *t* months since release. All specifications condition on being alive and not having migrated from Sweden by month *t*.

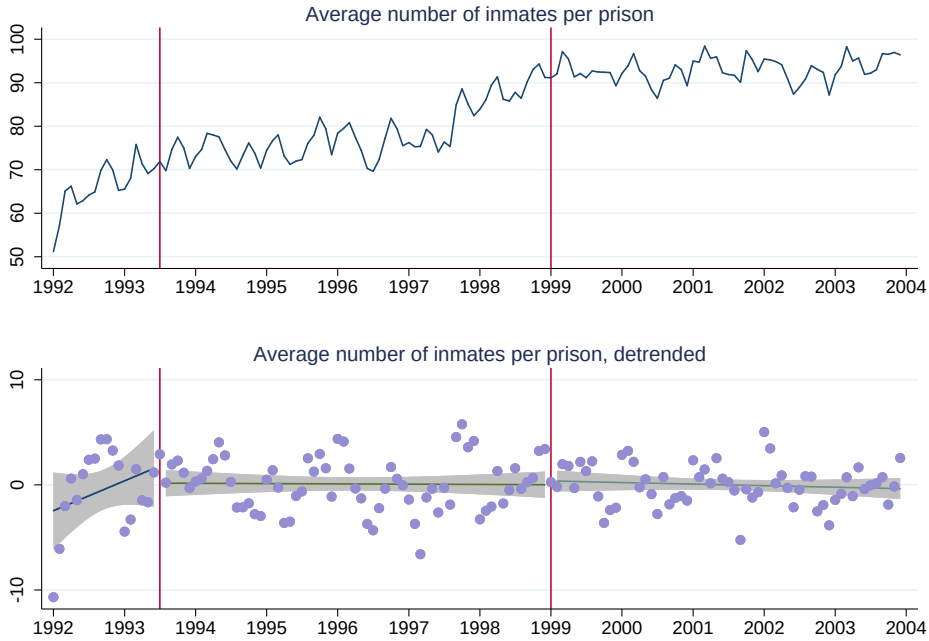
Appendix Figure 5. Average Annual Stock of Inmates and Average Annual Prison Capacity



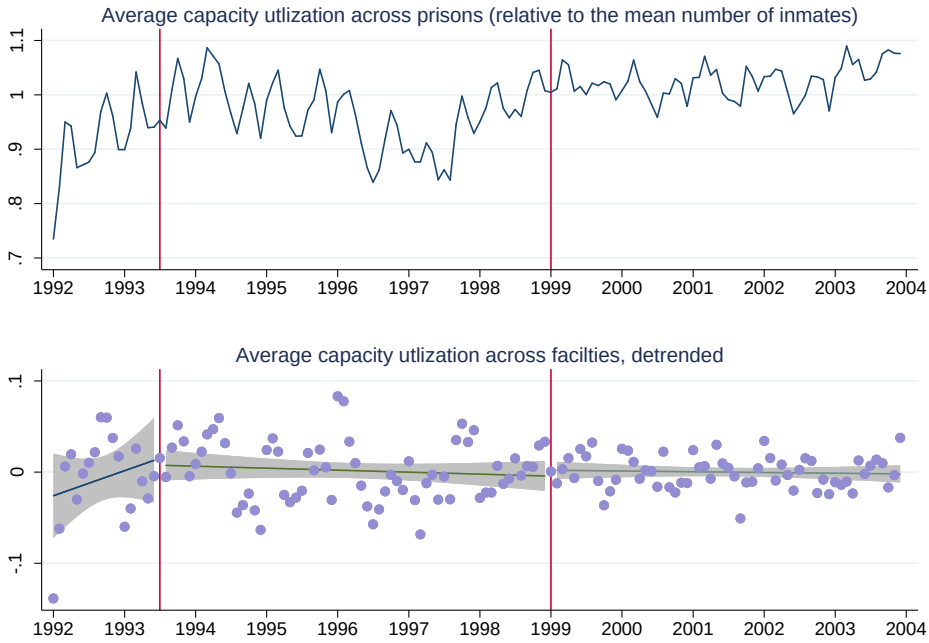
Note – This figure plots the average annual prison capacity against the average annual number of prison inmates. The average occupancy rate for 1989 to 2015 is 90%. Source: The Swedish Prison and Probation Service (2019).

Appendix Figure 6. Monthly Prison Level Descriptive Statistics, 1992-2004.

Panel A. Average number of inmates per prison.



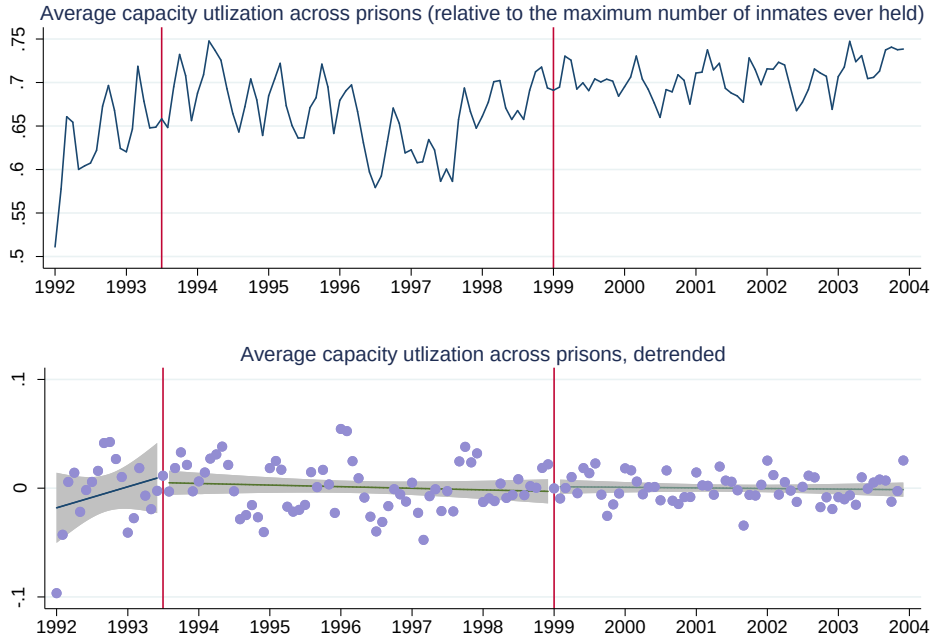
Panel B. Average capacity utilization across prisons measured by the number of inmates in a prison relative to the mean number of inmates ever in that prison.



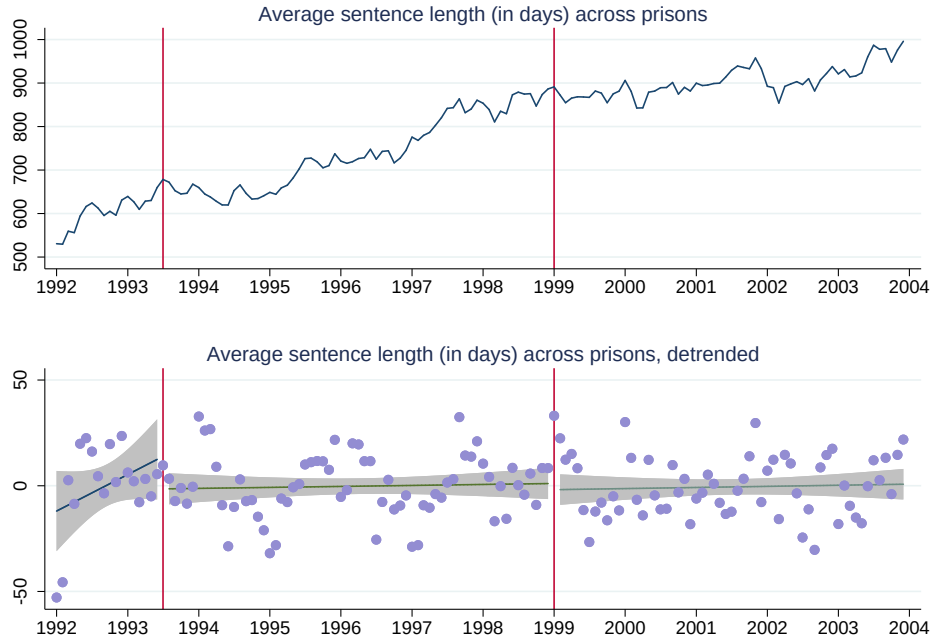
Note – Monthly time series are calculated using information on all inmates (including those not in our estimation sample) and all prisons. Monthly time series are detrended using year fixed effects, month of the year fixed effects (to remove seasonality), and a continuous month trend (just as in our baseline specifications). We plot these data and draw linear regression lines (along with 95% confidence intervals) to the left and to the right of the reform months.

Appendix Figure 6 (continued). Monthly Prison Level Descriptive Statistics, 1992-2004.

Panel C. Average capacity utilization across prisons measured by the number of inmates in a prison relative to the maximum number of inmates ever in that prison.



Panel D. Average sentence length of inmates averaged across all prisons.



Note – Monthly time series are calculated using information on all inmates (including those not in our estimation sample) and all prisons. Monthly time series are detrended using year fixed effects, month of the year fixed effects (to remove seasonality), and a continuous month trend (just as in our baseline specifications). We plot these data and draw linear regression lines (along with 95% confidence intervals) to the left and to the right of the reform months.

Appendix Table 1. Early Release Share of Time Served Laws by Sentence Month Bin and Reform Period

Sentence Month Bin	Pre-1993 Reform	Between 93 and 99 Reforms	Post 1999 Reform
0	1	1	1
1	1	1	1
2	1	1	0.67
3	0.67	0.67	0.67
4	0.5	0.67	0.67
5	0.5	0.67	0.67
6	0.5	0.67	0.67
7	0.5	0.67	0.67
8	0.5	0.67	0.67
9	0.5	0.67	0.67
10	0.5	0.67	0.67
11	0.5	0.67	0.67
12	0.5	0.67	0.67
13	0.5	0.64	0.67
14	0.5	0.62	0.67
15	0.5	0.60	0.67
16	0.5	0.58	0.67
17	0.5	0.57	0.67
18	0.5	0.55	0.67
19	0.5	0.54	0.67
20	0.5	0.53	0.67
21	0.5	0.52	0.67
22	0.5	0.51	0.67
23	0.5	0.51	0.67
24	0.5	0.50	0.67
25	0.5	0.5	0.67
26	0.5	0.5	0.67
27	0.5	0.5	0.67
28	0.5	0.5	0.67
29	0.5	0.5	0.67
30	0.5	0.5	0.67
31	0.5	0.5	0.67
32	0.5	0.5	0.67
33	0.5	0.5	0.67
34	0.5	0.5	0.67
35	0.5	0.5	0.67
36	0.5	0.5	0.67
37	0.5	0.5	0.67
38	0.5	0.5	0.67
39	0.5	0.5	0.67
40	0.5	0.5	0.67
41	0.5	0.5	0.67
42	0.5	0.5	0.67
43	0.5	0.5	0.67
44	0.5	0.5	0.67
45	0.5	0.5	0.67
46	0.5	0.5	0.67
47	0.5	0.5	0.67
48	0.5	0.5	0.67

Note – This displays the values of the EarlyReleaseLaw variable, which is used as the instrument. The 93 reform was on July 1, 1993, while the 99 reform when into effect on January 1, 1999. It is whether the defendant's conviction date is before or after the reform date that determines which regime the sentence is under. The only exception is if the reform decreased sentence length (as in the case of the 99 reform for 2 month sentences); in this case, it is the start date of the sentence that is the relevant date.

Appendix Table 2. Sample Restrictions

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

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

Appendix Table 3. Sample Sizes per Sentencing Bin: Overall and Around Each Reform

		Conviction within 2 years of:	
sentence_month_bin	Analysis Sample	1993 Reform	1999 Reform
0	2668	1807	365
1	20088	11743	3625
2	9003	4412	2538
3	7535	3167	2515
4	7242	3004	2552
5	3280	1228	1243
6	7017	2985	2431
7	2013	723	798
8	4634	1958	1558
9	1622	647	589
10	3170	1291	1159
11	873	360	324
12	3948	1626	1426
13	593	244	213
14	1270	510	506
15	911	384	340
16	882	374	306
17	280	126	102
18	1934	745	737
19	229	108	75
20	577	231	226
21	282	116	105
22	309	127	116
23	83	42	25
24	1618	699	609
25	100	37	44
26	155	59	54
27	173	75	60
28	148	53	60
29	48	21	13
30	886	343	328
31	51	20	17
32	134	52	48
33	51	21	17
34	73	31	27
35	20	10	7
36	946	391	343
37	41	17	15
38	62	19	27
39	53	16	21
40	45	16	17
41	14	7	4
42	375	134	154
43	15	8	6
44	44	19	12
45	28	12	6
46	26	12	10
47	7	3	3
48	553	226	197
Total N (0-48 Month Bins)	86109	40259	25973
N: 4-48 months (analysis sample)	46815	19130	16930
N (4-48 months): treated bins		16829	4850
N (4-48 months): untreated bins		2301	12080

Note – Shaded bins indicate that the bin was treated, i.e. faced high share time served, after the 1993 and/or 1999 reforms. The number of observations in treated and untreated bins includes sentences before and after the reforms.

Appendix Table 4. Sensitivity Analysis of First Stage Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	baseline	drop if start - conv > 365	keep pretrial only	keep trim high	keep trim low	keep juveniles	add straddle samples, use conv date (as if untreated)	add straddle samples - use start date (as if treated)	drop 4-6 month sentence bins	drop 4 month window around each reform
law_share_sent	271.037	264.259	275.891	270.192	271.455	269.643	223.121	245.914	308.209	274.851
	(25.771)***	(25.219)***	(25.309)***	(28.578)***	(25.315)***	(25.848)***	(24.228)***	(27.716)***	(26.692)***	(26.841)***
F	111	110	119	89	115	109	85	79	133	105
Observations	46815	43926	47671	47044	47008	46977	50355	50355	29276	44532
R-squared	0.91	0.91	0.91	0.84	0.91	0.91	0.91	0.91	0.89	0.91

Note – This table presents the first estimates (i.e. regressions of prison days served on the share time served prescribed by the law) for the entire analysis sample of 4-48 month sentences in column (1). The remaining columns assess the sensitivity of these estimates to relaxing various sample restrictions. All specifications uses the sample alive and in Sweden at time of release (at t = 0). The baseline specification (sentence month fixed effects, continuous month of conviction trend, year of conviction fixed effects and full set of controls *X*) is estimated for all columns. Standard errors clustered on sentence month bin in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table 5. Robustness of Reduced Form Mortality Results to the Exclusion of Controls

	Entire Sample at Month			
Dep Variable	12	24	36	120
Death (Any Cause)	-0.022*	-0.037**	-0.031	-0.032
	[0.013]	[0.016]	[0.023]	[0.035]
Death by Type:				
Suicide	-0.010*	-0.011*	-0.015**	-0.019*
	[0.005]	[0.005]	[0.007]	[0.011]
Violent	-0.004**	-0.006	-0.004	0.006
	[0.002]	[0.004]	[0.005]	[0.007]
Cancer/Circ/Digestive	0.004	-0.005	-0.002	-0.047**
	[0.005]	[0.006]	[0.007]	[0.019]
Circulatory	0.003	-0.005	-0.000	-0.022**
	[0.004]	[0.006]	[0.006]	[0.011]
Digestive	0.001	0.003	-0.001	-0.009
	[0.002]	[0.003]	[0.005]	[0.012]
Cancer	-0.000	-0.003	-0.001	-0.016
	[0.003]	[0.004]	[0.004]	[0.012]
Alc/Narc Exclusive	-0.011	-0.009	-0.006	0.016
	[0.007]	[0.012]	[0.015]	[0.022]
N	46287	45934	45629	44560
Controls	CRIM ONLY	CRIM ONLY	CRIM ONLY	CRIM ONLY

Note – Each column conditions on the sample that never emigrated from Sweden t months since date of release, indicated at the top of each column. The outcome for the first row is death due to any cause, while the other panels look at specific causes of death, indicated with either ICD codes or coroner flags. In contrast to the main results, these specifications only include criminal justice controls and exclude controls for pre-incarceration health, as well demographics and socioeconomic characteristics at the time of incarceration. Standard errors clustered on sentence month bin. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table 6. Culling Analysis: Does Exposure to Treatment Affect Chance of Death in Prison?

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable = Died in Prison				
	Baseline Reduced Form Specification			Pre-Post (with Bin Fixed Effects)	
	Pooled Sample	1993 Sample	1999 Sample	1993 Sample	1999 Sample
law_share_sent	-0.004	-0.004	-0.012		
	[0.005]	[0.005]	[0.010]		
Post 1999 Reform Dummy					-0.000033
					[0.001]
Post 1993 Reform Dummy				0.00054	
				[0.00037]	
Observations	46868	19149	16950	19149	16950
R-squared	0.01	0.01	0.02	0	0.01

Robust standard errors, clustered on sentence month bin, in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is died in prison. In the sample of 4-48 month sentences, 53 died in prison. The first three columns estimate the baseline reduced form specification, while (4) and (5) just asks whether there is a differential chance of death in prison before and after the reform, controlling for bins. There are so few such deaths, one cannot estimate this separately for each bin, though bin fixed effects are included.

Appendix Table 7. Additional Reduced Form Mortality Robustness and Sensitivity Checks

Death (Any Cause) at month *t*			Suicide at month *t*			Violent Death at month *t*		
12	24	36	12	24	36	12	24	36
Baseline (N = 46287, 45934, 45629)								
-0.019	-0.031*	-0.027	-0.010*	-0.011**	-0.016**	-0.004*	-0.005	-0.003
[0.012]	[0.016]	[0.023]	[0.005]	[0.006]	[0.007]	[0.002]	[0.004]	[0.005]
First 4-48 months sentence (N = 26789, 26565, 26372)								
-0.012	-0.022	-0.014	-0.015**	-0.016**	-0.024***	0.000	0.000	0.001
[0.013]	[0.017]	[0.025]	[0.007]	[0.007]	[0.009]	[0.002]	[0.005]	[0.006]
Last 4-48 month sentence (N = 26757, 26520, 26325)								
-0.011	-0.018	-0.000	-0.017	-0.022*	-0.028*	-0.006	-0.012	-0.011
[0.022]	[0.025]	[0.042]	[0.010]	[0.011]	[0.014]	[0.004]	[0.007]	[0.010]
Drop 4-month conviction window around each reform (N = 44026, 43689, 43395)								
-0.036**	-0.052***	-0.042	-0.010*	-0.013**	-0.017**	-0.005*	-0.006	-0.005
[0.015]	[0.019]	[0.028]	[0.006]	[0.006]	[0.008]	[0.002]	[0.005]	[0.007]
Prison Sentence Day Fixed Effects (clustered on day) (N = 46287, 45934, 45629)								
-0.016	-0.026*	-0.021	-0.010*	-0.011**	-0.016**	-0.004	-0.005	-0.002
[0.011]	[0.016]	[0.024]	[0.005]	[0.005]	[0.007]	[0.002]	[0.004]	[0.005]
Prison Sent. Month Bin X Conviction Year Fixed Effects (N = 46287, 45934, 45629)								
-0.016	0.000	-0.054*	-0.021**	-0.018**	-0.039***	-0.003	-0.005	-0.002
[0.017]	[0.024]	[0.027]	[0.008]	[0.009]	[0.014]	[0.004]	[0.008]	[0.006]
Prison Month Bin x Month of Conviction Trend (N = 46287, 45934, 45629)								
-0.021	-0.030*	-0.024	-0.010*	-0.012**	-0.016**	-0.005**	-0.006	-0.003
[0.013]	[0.017]	[0.024]	[0.006]	[0.006]	[0.007]	[0.002]	[0.005]	[0.005]
Include Straddle Sample: Using Conviction date (N = 49792, 49410, 49080)								
-0.018	-0.028	-0.016	-0.008**	-0.010*	-0.013*	-0.004	-0.005	-0.003
[0.014]	[0.019]	[0.023]	[0.004]	[0.006]	[0.007]	[0.003]	[0.004]	[0.005]
Include Straddle Sample: Using Start Date (N = 46287, 45934, 45629)								
-0.02	-0.032*	-0.034	-0.008*	-0.010*	-0.013*	-0.004	-0.006	-0.004
[0.013]	[0.018]	[0.022]	[0.004]	[0.005]	[0.006]	[0.003]	[0.004]	[0.005]
Include those with post-trial detention only (N = 47131, 46771, 46456)								
-0.021	-0.035**	-0.029	-0.010*	-0.013**	-0.017**	-0.005**	-0.006	-0.004
[0.013]	[0.016]	[0.022]	[0.005]	[0.006]	[0.007]	[0.002]	[0.005]	[0.006]
Keep High and Low Trimmed Share of Time Served (N = 46699, 46340, 46031)								
-0.02	-0.033**	-0.029	-0.010*	-0.012**	-0.016**	-0.004*	-0.005	-0.003
[0.012]	[0.016]	[0.022]	[0.005]	[0.006]	[0.007]	[0.002]	[0.004]	[0.005]
Keep Juveniles (N = 46447, 46093, 45787)								
-0.018	-0.031*	-0.028	-0.010*	-0.011*	-0.016**	-0.004*	-0.005	-0.003
[0.012]	[0.016]	[0.023]	[0.005]	[0.006]	[0.007]	[0.002]	[0.004]	[0.005]

Note -- Each specification conditions on the sample that never emigrated from Sweden t months since date of release, indicated at the top of each column. The outcome (any death, suicide, violent death) is indicated in the first row of the table. The first row presents the results of estimating the baseline specification with the full set of controls. Each additional panel: (i) relaxes the sample included to assess the sensitivity to the sample creation procedure, or (ii) adds additional fixed effects to the specification to capture more refined trends. Samples sizes at 12, 24, and 36 months are listed, respectively, in each panel heading. Standard errors are clustered on sentence month bin. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix Table 8: Reduced Form Mortality: Full Heterogeneity Analysis

Death (Any Cause) at month:			Suicide at month:			Violent Death at month:			CCD at month:
12	24	36	12	24	36	12	24	36	120
Baseline									
-0.019	-0.031*	-0.027	-0.010*	-0.011**	-0.016**	-0.004*	-0.005	-0.003	-0.048**
[0.012]	[0.016]	[0.023]	[0.005]	[0.006]	[0.007]	[0.002]	[0.004]	[0.005]	[0.019]
46287	*45934*	*45629*	*46287*	*45934*	*45629*	*46287*	*45934*	*45629*	*44560*
< 33 years old									
-0.034**	-0.051**	-0.055**	-0.005	-0.005	-0.005	-0.003	-0.001	-0.002	-0.002
[0.014]	[0.021]	[0.025]	[0.007]	[0.007]	[0.009]	[0.003]	[0.005]	[0.005]	[0.014]
24417	*24231*	*24047*	*24417*	*24231*	*24047*	*24417*	*24231*	*24047*	*23410*
>= 33 years old									
0.001	0.001	0.005	-0.015**	-0.019*	-0.031**	-0.005	-0.01	-0.005	-0.107**
[0.021]	[0.028]	[0.040]	[0.007]	[0.010]	[0.014]	[0.005]	[0.010]	[0.010]	[0.041]
21870	*21703*	*21582*	*21870*	*21703*	*21582*	*21870*	*21703*	*21582*	*21150*
property offender									
-0.037*	-0.038	-0.069**	-0.006	-0.003	-0.006	0.003	0.001	0.001	-0.043
[0.019]	[0.024]	[0.026]	[0.004]	[0.005]	[0.007]	[0.004]	[0.006]	[0.005]	[0.031]
20084	*19948*	*19816*	*20084*	*19948*	*19816*	*20084*	*19948*	*19816*	*19385*
violent offender									
-0.014	-0.051*	-0.01	-0.014	-0.019	-0.025**	-0.014**	-0.019*	-0.013	-0.108***
[0.025]	[0.026]	[0.033]	[0.013]	[0.013]	[0.011]	[0.006]	[0.010]	[0.010]	[0.033]
15243	*15119*	*15011*	*15243*	*15119*	*15011*	*15243*	*15119*	*15011*	*14630*
psychiatric hospitalization in the last five years									
-0.021	-0.065	-0.039	-0.038*	-0.053**	-0.044*	-0.010*	-0.022**	-0.028**	-0.061
[0.045]	[0.057]	[0.071]	[0.019]	[0.020]	[0.022]	[0.005]	[0.011]	[0.011]	[0.056]
9897	*9840*	*9792*	*9897*	*9840*	*9792*	*9897*	*9840*	*9792*	*9635*
general ward hospitalization in the last five years									
-0.026	-0.055	-0.036	-0.014	-0.016	-0.018	-0.004	-0.012	-0.007	-0.06
[0.023]	[0.038]	[0.051]	[0.010]	[0.010]	[0.013]	[0.004]	[0.010]	[0.012]	[0.038]
19176	*19065*	*18960*	*19176*	*19065*	*18960*	*19176*	*19065*	*18960*	*18573*
no hospitalization in the last five years									
-0.022*	-0.026	-0.029	-0.003	-0.003	-0.012	-0.005	-0.003	0.001	-0.026
[0.013]	[0.021]	[0.026]	[0.007]	[0.007]	[0.011]	[0.003]	[0.005]	[0.008]	[0.025]
22073	*21862*	*21691*	*22073*	*21862*	*21691*	*22073*	*21862*	*21691*	*21096*

Note -- Each specification conditions on the sample that never emigrated from Sweden t months since date of release, indicated at the top of each column. The outcome (any death, suicide, violent death) is indicated in the first row of the table. Each panels corresponds to a different sub-sample; the first is the full sample used in the baseline analysis with the full set of controls. Standard errors are in brackets clustered on sentence month bin. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample sizes are in italics.

Appendix Table 9. Summary Statics for Sentence Month Bins that Are Affected by the 1993 and 1999 Early Release Reforms.

	Affected by the 1993 Reform 4-23 Month Sentence Bins		**Affected by the 1999 Reform 13-48 Month Sentence Bins**	
	N=16829		**N=4850**	
variable	mean	sd	mean	sd
prison sentence months	8.88	4.53	23.45	9.42
prison sentence days	268.45	136.38	705.99	281.58
Early release law (share time served law)	0.59	0.08	0.61	0.07
share time served	0.50	0.14	0.54	0.11
prison days	132.17	72.30	384.79	182.75
assumed conviction year	1993.15	1.12	1998.59	1.15
start year sentence	1993.43	1.16	1998.81	1.25
end year sentence	1993.76	1.24	1999.87	1.44
dui	0.04	0.18	0.00	0.07
drugs alcohol	0.11	0.32	0.21	0.41
traffic	0.02	0.13	0.00	0.06
property	0.50	0.50	0.26	0.44
violent	0.29	0.46	0.48	0.50
other	0.05	0.21	0.03	0.18
number crimes contemporaneous	7.08	6.64	6.13	6.83
number crimes past	60.98	65.74	48.63	67.03
number prison past	5.64	6.57	4.54	6.48
any pretrial detention	0.68	0.47	0.81	0.39
male	0.95	0.21	0.95	0.22
Swedish citizen	0.83	0.38	0.79	0.40
born Sweden	0.79	0.41	0.72	0.45
age at prison start	33.09	9.13	33.56	10.16
primary school_lag1	0.12	0.32	0.08	0.27
short high school_lag1	0.38	0.49	0.43	0.50
long high school_lag1	0.40	0.49	0.38	0.49
short university_lag1	0.01	0.10	0.01	0.11
long university_lag1	0.01	0.10	0.03	0.16
phd_lag1	0.00	0.02	0.00	0.02
education_missing_lag1	0.08	0.27	0.07	0.25
married_lag1	0.20	0.40	0.21	0.41
number children at conviction	1.04	1.31	1.08	1.35
employed in november_lag1to3	0.75	1.01	0.50	0.94
log average income_lag1to3	10.44	1.37	10.39	1.45
hospital days alcohol_lag1to3	0.36	3.24	0.27	2.57
hospital days narcotics_lag1to3	0.87	7.04	0.66	6.89
hospital days psychiatric_lag1to3	6.51	45.67	4.29	36.47
hospital days other_lag1to3	2.66	9.29	2.52	16.65

Note – Differences discussed in the text are highlighted here in grey.

Appendix Table 10. Exclusion Restriction Tests: Smaller Reform Windows

Death (Any Cause) at month				Suicide at month				Violent Death at month	CCD Death at month
12	24	36	120	12	24	36	120	12	120
1993 Reform									
Panel A. Baseline (2 year window)									
-0.022	-0.021	-0.025	-0.016	-0.013*	-0.009	-0.018**	-0.022	-0.007***	-0.022
[0.015]	[0.020]	[0.023]	[0.042]	[0.007]	[0.007]	[0.009]	[0.014]	[0.003]	[0.034]
Panel B. 1.5 year window									
-0.022	-0.009	-0.011	-0.012	-0.013*	-0.010	-0.020**	-0.025	-0.007*	-0.027
[0.014]	[0.020]	[0.025]	[0.052]	[0.007]	[0.007]	[0.008]	[0.017]	[0.003]	[0.036]
Panel C. 1.5 year window (without year of conviction fixed effects)									
-0.023	-0.012	-0.010	-0.008	-0.012*	-0.008	-0.018**	-0.025	-0.007**	-0.026
[0.015]	[0.021]	[0.023]	[0.052]	[0.006]	[0.007]	[0.008]	[0.017]	[0.003]	[0.037]
Panel D. 1 year window									
-0.068**	-0.050	-0.021	-0.054	-0.023*	-0.026*	-0.032**	-0.018	-0.007*	-0.021
[0.029]	[0.035]	[0.040]	[0.095]	[0.012]	[0.014]	[0.015]	[0.030]	[0.004]	[0.055]
Panel E. 6-month window									
-0.068	-0.016	-0.124*	-0.007	-0.040*	-0.044**	-0.071***	0.043	-0.006	-0.003
[0.044]	[0.059]	[0.067]	[0.176]	[0.021]	[0.021]	[0.026]	[0.045]	[0.005]	[0.075]
1999 Reform									
Panel A. Baseline (2 year window)									
-0.040*	-0.087**	-0.069	-0.082	0.003	-0.008	-0.009	-0.015	-0.005	-0.062
[0.023]	[0.034]	[0.043]	[0.092]	[0.009]	[0.010]	[0.011]	[0.023]	[0.007]	[0.052]
Panel B. 1.5 year window									
-0.056	-0.053	-0.043	-0.023	0.003	-0.002	-0.006	-0.023	-0.009	-0.025
[0.035]	[0.045]	[0.060]	[0.100]	[0.010]	[0.012]	[0.014]	[0.030]	[0.007]	[0.052]
Panel C. 1.5 year window (without year of conviction fixed effects)									
-0.057*	-0.056	-0.044	-0.025	0.007	0.001	-0.001	-0.017	-0.007	-0.040
[0.034]	[0.044]	[0.058]	[0.092]	[0.010]	[0.012]	[0.014]	[0.028]	[0.008]	[0.048]
Panel D. 1 year window									
-0.068	-0.044	-0.029	0.001	-0.005	-0.009	-0.012	-0.011	-0.022*	-0.024
[0.044]	[0.054]	[0.065]	[0.120]	[0.010]	[0.014]	[0.014]	[0.040]	[0.012]	[0.061]
Panel E. 6-month window									
-0.067	-0.045	-0.020	0.028	-0.002	-0.003	-0.003	-0.102*	-0.037	-0.061
[0.067]	[0.095]	[0.101]	[0.221]	[0.006]	[0.021]	[0.021]	[0.059]	[0.035]	[0.083]

Note – Each column conditions on the sample never emigrated from Sweden *t* months since date of release. Panel A uses the baseline specification with a 2-year window on either side of the reforms. All other specifications drop individuals to correspond to the noted sample windows. Shorter windows exclude conviction year fixed effects; panels B and C show that this not important. Standard errors are in brackets clustered on sentence month bin. * significant at 10%; ** significant at 5%; *** significant at 1%.