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The Effects of Electronic Monitoring on Offenders and their Families *

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Abstract

Electronic monitoring (EM) is a popular instrument to reduce large prison populations. Evidence on the effects of EM on criminal recidivism is, however, limited and it is unclear how this alternative to incarceration affects the labor market outcomes of offenders. Moreover, little is known about potential spillover effects on family members. We study the introduction of EM in Sweden in 1997 wherein offenders sentenced to up to three months in prison were given the possibility to avoid entering prison by substituting to EM. Our difference-in-differences estimates comparing the change in the prison inflow rate of eligible offenders to that of non-eligible offenders with slightly longer sentences show that the reform dramatically decreased incarcerations. Our main finding is that EM lowers criminal recidivism and improves offenders' labor market outcomes. There is also some evidence of improvements in the short and intermediate run outcomes of the children of the offenders. The main channels through which EM operates seem to be by allowing offenders to maintain regular work and potentially also by reducing employer discrimination. Our calculations suggest that the social benefits of EM are at least six to nine times larger than the fiscal savings from reduced prison expenditure. This makes the welfare improvements from EM potentially much greater than what has been previously recognized.

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1 Introduction

Electronic monitoring (EM) has become a key tool for countries trying to reduce rising prison expenditure (e.g., Bartels and Martinovic 2015).[1] While countries differ greatly with respect to the timing when EM was first introduced, with the United States and Sweden being among the earliest adopters, the basic features of the EM programs are similar: an electronic device monitors the offender under curfew by global positioning system (GPS) and allows those sentenced for less severe types of crimes to participate in treatment programs and regular work (Bartels and Martinovic 2015). Most estimates suggest that the costs of EM are an order of magnitude lower than those of incarceration, mainly because fewer prison staff is needed to perform the monitoring now automated (Kyckelhahn 2011).

Besides limiting the fiscal costs of large prison systems, there are many reasons to believe that EM also offers other social benefits. Perhaps most importantly, EM may improve labor market prospects by increasing the possibility for offenders to maintain or find employment. Having a job or being willing to search for one are fundamental components of many EM programs and labor market opportunity is also a strong predictor of successful rehabilitation (e.g., Freeman 1999; Yang 2017; ETUI 2019; Williams and Weatherburn 2020). EM may further improve labor market outcomes by reducing employer discrimination of ex-prisoners (e.g., Western, Kling, and Weiman 2001; Lofstrom and Raphael 2016). EM potentially also improves rehabilitation by preventing accumulation of criminal capital in prison and by preserving family relationships (e.g., Western et al. 2001; Di Tella and Schargrodsky 2013; Lofstrom and Raphael 2016).[2] The potential benefits of EM are not necessarily limited to the offender himself but may also operate as an externality within the family. The causal effects of EM are, however, theoretically unclear, as serving time at home may also increase the risk of relapsing into crime by making low punishment salient (e.g., Becker 1968; Drago et al. 2009; Chalfin and McCrary 2017).[3] Family members may also be adversely affected if EM implies spending more time at home with a negative role model. Estimating the causal effect of EM is, however, challenging because of the difficulties involved in separating the effect of EM from correlated unobservables (e.g., Di Tella and Schargrodsky 2013; Henneguelle, Monnery, and Kensey 2016; Williams and Weatherburn 2020).[4]

[1] In the United States, the number of accused and convicted criminal offenders subject to electronic monitoring is estimated to have risen nearly 140 percent over the period 2005-2015 (PEW 2016).

[2] See also Stevens (2017); Agan and Starr (2018); Grogger (2018).

[3] It is also possible that EM in fact provides more opportunities to commit crime when serving the sentence at home instead of in prison. For more discussions on deterrence, see Nagin (2013) and Chalfin and McCrary (2017).

[4] For instance, correlated unobservables may be induced by the justice system when distributing EM to offenders

In this paper, we estimate the causal effects of increased access to EM in the context the Swedish criminal justice system. Our work draws on two strengths of the Swedish setting. First, Swedish administrative data allow us to measure the impacts of EM both for the offenders and their family members. The data contain information on a wide range of labor market outcomes as well as criminal recidivism. The children were in the ages of 0 to 14 at the time of the parent's trial, which represents a critical period in a child's life when they may be particularly sensitive to changes in the home environment. We are able to follow the children into adulthood and observe outcomes such as teen criminal convictions, school performance, and adult labor market outcomes. The second strength of our setting is that we are able to isolate exogenous variation in access to EM by studying the large expansion in 1997 where EM was scaled up from a small local pilot scheme to the entire country. This reform implied that individuals sentenced to at most three months in prison became eligible to apply for EM after having received their sentence from the court, thereby avoiding entering prison in the first place. Generous rules of eligibility implied that the incarceration rate in Sweden immediately fell sharply among individuals sentenced to prison terms up to three months.[5]

To isolate the causal effects of the scaling up of EM in Sweden in 1997, we use a difference-in-differences strategy comparing the change in the outcomes of eligible offenders before and after the reform to that of ineligible offenders sentenced to between four and twelve months in prison. We start by showing that the reform led to a significant 30 percentage-point reduction in the incarceration rate with highly similar pre-trends for eligible and ineligible offenders. Moreover, there is no evidence that the sentence length changed around the reform date, suggesting that courts did not adjust their sentencing practice in response to the reform. We then turn to estimating the effect of increased access to EM on the offenders themselves. Our results show that the reform significantly lowered the three year re-conviction rate by about 2.1 percent and reduced the risk of re-arrest by 4.4 percent. The reform also improved labor market outcomes: the likelihood of being employed within three years after conviction increased by 12.7 percent and average earnings increased by 22.5 percent. These improvements do not only occur in the first year but remain large also in the second and third year, suggesting that the beneficial effects of EM may be long lasting. Our back-of-the-envelope calculations reveal that the social benefits in terms of increased earnings alone is between six and nine times larger than the direct fiscal savings from using EM instead of prison. The benefits are particularly large for males and those who were sentenced for violent crimes and drunk driving. Our results are robust to a battery of

with the highest likelihood of success.

[5] Not everyone sentenced to prison ends up actually serving time. The main reason for not entering prison is that time spent in pre-trial detention is subtracted from the sentence.

specification checks, including narrowing the sentence bandwidth for inclusion in the control group, adopting a regression discontinuity design based on time from conviction to reform, and we find small and insignificant estimates in various placebo analyses.

We proceed by using our rich data to shed some light on potential mechanisms. We investigate whether EM prevents accumulation of criminal capital in prison by separating between types of crimes that require skills that offenders may be more likely to acquire while in prison (e.g., theft or selling of drugs) and more spontaneous types of crimes (e.g., violent crimes or drunk driving). We find a significant decrease in non-acquired crimes and insignificant estimates for acquired crimes. Next, we investigate the possibility that EM improves social integration by allowing offenders to maintain family relationships. The results show no significant effects of access to EM on the risk of divorce/separation. We then examine the possibility that EM improves offender outcomes by allowing them to keep or find jobs. We do so by splitting the sample by baseline employment status and re-estimate the model. While we find larger improvements in labor market outcomes for offenders who were employed at baseline, we also find significant positive effects for non-employed offenders. This finding suggests that EM potentially improves labor market outcomes not only by allowing offenders to maintain their jobs but potentially also by providing more opportunities to search for jobs. We end by examining the role of employer discrimination. The results show that access to EM significantly improves various measures of firm quality (e.g., average earnings). While we are hesitant to draw strong conclusions from this finding because firm quality likely correlates with unobserved firm attributes that may be connected to the outcomes we consider, our results provide the first tentative evidence that reduced employer discrimination may play some role in explaining why EM improves the outcomes of ex-offenders.

While we document large social benefits of increased access to EM, there may also be potentially important but previously neglected spillover effects onto family members. Such externalities, which could either be positive or negative, should be accounted for when considering the full social welfare effects of expanding access to EM. We find that parental access to EM significantly increases the probability that the child completes compulsory school by 3.5 percent and significantly raises disposable income at age 25 by 6.3 percent. We do not find any significant effects for the other (non-convicted) parent. Taken together, the large improvements in labor market outcomes for offenders coupled with improvements in some of the outcomes for the children and the absence of major adverse effects for the partner suggests that the social benefits from EM are much higher than what was previously appreciated.

Our results relate to an important literature estimating the impact of EM on criminal recividism. While most studies fail to account for correlated unobservables (see Renzema and Mayo-Wilson (2021) for a review), recent research has benefited from quasi-experimental research designs (e.g., Di Tella and Schargrodsky 2013; Henneguelle, Monnery, and Kensey 2016; Williams and Weatherburn 2020). One of the most compelling pieces of evidence is by Di Tella and Schargrodsky (2013) who in the context of Argentina exploit the random assignment of detainees to judges with a different propensity to allocate EM. The authors find that EM significantly reduces the one-year recidivism rate by up to 48 percent. Williams and Weatherburn (2020) use a similar random-judge design in the context of Australia and shows that EM decreases the two-year recidivism rate by about 28 percent. Henneguelle et al. (2016) instrument for the use of EM using a local introduction of the option of EM in French courts and find that EM reduces the likelihood of reoffending within five years by about 10 percent. Marie (2009) examines the impact of electronic monitoring in the context of early release for offenders serving prison sentences in England and Wales and finds significant reductions in re-offending.

We make three key contributions to this literature. Firstly, we document the effect of EM on a broader set of outcomes that also include labor market outcomes. While preserving labor market ties has been a key argument for implementing EM, there is no robust empirical evidence to date.[6]. Secondly, we investigate spillover effects on family members. The possibility of important but previously neglected spillover effects from EM on family members is suggested by recent research on the effects of parental incarceration on family members (e.g., Dobbie, Grönqvist, Niknami, Palme, and Priks 2018; Norris, Pecenco, and Weaver 2021; Arteaga 2022), but it is unclear whether these results extend also to the type of offenders typically targeted by EM. Documenting the effects of EM for a broad set of outcomes in multiple populations at risk of being affected should allow for a greater understanding of the overall welfare implications concerning the use of EM. Thirdly, we are able to shed light on several of the mechanisms that may produce the causal effects we document. Existing studies have been unable to provide evidence on the underlying channels because proper data has not been available. It should also be mentioned that, with few exceptions (e.g., Williams and Weatherburn 2020), previous research has mostly studied the use of EM in settings where there is another alternative, for instance at the pre-trial phase, where bail might otherwise be used, and for early release from prison, where parole might be considered an alternative. By contrast, the context we study

[6]The only studies we are aware of that investigate outcomes other than recidivism are Andersen and Andersen (2014) and Fallesen and Andersen (2017). The former paper examines two policy reforms that expanded the use of EM in Denmark. Lacking direct measures of labor supply, the authors use the take-up rate of welfare benefits one year after sentence as a proxy, finding a negative effect. The latter study shows that access to EM increased marital stability in Denmark.

concerns the use of EM as a "front-end" alternative to prison, i.e., such that EM provides a true substitute to incarceration.

Our work also relates to the literature on the impacts of incarceration on an individual's own outcomes (see reviews by, e.g., Western et al. 2001). Recent studies use random assignment of cases to judges who differ in their propensity to sentence to prison in the United States (Mueller-Smith 2015; Dobbie, Goldin, and Yang. 2018), Sweden (Dobbie et al. 2018) and Norway (Bhuller, Dahl, Løken, and Mogstad 2020).[7] The results from this research are somewhat inconclusive, with some studies finding adverse effects (Mueller-Smith 2015; Dobbie et al. 2018) while other studies find beneficial effects, in particular for individuals who where unemployed before the trial (Bhuller et al. 2020). A few studies also investigate the effect of parental incarceration on children's outcomes, generally finding that parental incarceration is associated with worse outcomes (see Wildeman 2010, and Murray, Farrington, and Sekol 2012, for recent reviews). A handful of papers go beyond these associations to estimate the causal effects of parental incarceration on children with mixed results. Using a random-judge design, Dobbie et al. (2018) find that parental incarceration increases teen crime, reduces school performance and hurts employment and earnings.[8] Norris et al. (2021) find that, in the United States, parental incarceration decreases teen crime, has no impact on teen parenthood, and increases the probability that children live in wealthy neighborhoods as adults. Arteaga (2022) finds positive effects of parental incarceration in Colombia on their children's educational attainment. [9] The diverging results in these studies do not offer any guidance on the possible effects of EM on children.[10]

The rest of the paper is organized as follows. In Section 2, we describe the Swedish criminal justice system and the use of EM. Section 3 describes our data. We discuss our empirical design in Section 2. The results are presented in Section 5 and Section 6 concludes.

[7] In other related work using a random-judge design, all from the United States, Kling (2006) estimates the impact of sentence length, Aizer and Doyle (2015) estimate the impact of juvenile incarceration, and Dobbie et al. (2018) estimate the impact of pre-trial incarceration. Other quasi-experimental studies include Kuziemko (2013) who takes advantage of both a mass release and discontinuities in sentencing guidelines, and shows that longer incarceration implies a significant reduction in the risk of reoffending; using a similar strategy. Landersjo (2015) shows that seemingly exogenous increases in prison time improve post-release employment.

[8] Dobbie et al. (2018) also document negative effects of incarceration on the offenders themselves. Our results are consistent with these findings. Our findings for children are also broadly consistent with the results in Dobbie et at. A key difference between the studies is that the population of offenders differs. While we study relatively low-risk offenders who receive short prison sentences, Dobbie et al. consider the effect for the universe of offenders sentenced to incarceration.

[9] Bhueller et al. (2018) find no significant effects of parental incarceration on school performance and children's risk of crime but the estimates are imprecise.

[10] While several studies have used cross-sectional data to show that incarceration increases the risk of marital dissolution (e.g., Apel et al. 2010), this finding has only been verified in a quasi-experimental context by Dobbie et al. (2018). We are not aware of studies examining the effects of incarceration on spousal outcomes.

2 Institutional Background

Here we describe the institutional context. This outline draws heavily from previous description of the Swedish system (e.g. BRÅ 1999; Wennerberg 2013; Bungerfeldt 2014; Bartels and Martinovic 2015; Dobbie et al. 2018), and we refer to these publications for further details.

2.1 Institutional Setting and the 1997 Electronic Monitoring Reform

The criminal justice system in Sweden is similar to that practiced in most other OECD countries, with the exception of the United States which is an outlier in many dimensions. One of the most striking differences between the systems is that sentence lengths are considerably shorter in the OECD. For instance, more than 80 percent of sentences in Sweden are shorter than one year. The actual time served is also much less than the recorded sentences, as nearly all prisoners in Sweden receive probation after two-thirds of the sentence unless there are extraordinary circumstances.[11] By contrast, the typical prison sentence is 2.9 years in the United States (PEW 2016).

Large prison populations and costly rehabilitation programs have increased prison expenditures in most countries (GPT 2020). The possibility of reducing these costs has been the primary argument in favor of implementing EM. Following the introduction of EM in the United States in the 1990s, most OECD countries now use it extensively in various forms. One of the most common form involves a short prison term being entirely replaced by EM. This form is known as "front door" electronic monitoring.

Radio frequency EM was introduced in Sweden in 1994 as a means of making "home detention" secure and enforceable. The introduction was part of a small-scale experimental scheme that was to last for two years. At the time, Sweden was divided into 45 probation districts and the pilot scheme was (non-randomly) assigned to five of these districts. It was intended for offenders above age 18 who had been sentenced to at most two months in prison. Early evaluations deemed the pilot scheme as cost-effective and on January 1st, 1997, it was extended to all probation districts and its length extended from two to three months (BRÅ 1999).

Unlike most countries, but similar to many U.S. states, EM is in Sweden not imposed by the

[11] In Sweden, individuals arrested for a crime carrying a prison term of one or more years can be detained before trial if there is a risk that they will flee prosecution, obstruct the investigation, or commit a new crime. For those individuals who later receive prison sentences, this time will be subtracted from the sentence. There is no plea bargaining in the Swedish court system, ruling out the possibility that a defendant would plead guilty in exchange for a reduced sentence.

court. Instead, offenders must apply to the Prison and Probation Service to serve their prison sentence under EM. Shortly after the verdict of the court, all offenders who are sentenced to prison are informed by the Swedish Prison and Probation Services about the possibility of applying for EM and are provided with an application form. In the first year of the reform, approximately 75 percent of the offenders applied to the scheme (BRÅ 1999). As in most other countries, eligibility for EM is conditional on the offender having acceptable accommodation, regular employment, being willing to search for and take job, agreeing to be tested for alcohol and drugs, and accepting home visits from the Prison and Probation Service. No specific offence excludes an offender from participation but those living in the same locality as their victim or victims, mostly in domestic violence cases, are normally excluded from participation. All members of the offender's household need to give their consent. Employers are also informed, and the Prison and Probation Service may occasionally visit the workplace to verify that the individual is actually working. Among those who applied for EM in 1997, 87 percent were granted this alternative penalty, usually within a few weeks after the prison sentence (BRÅ 1999). The only out-of-pocket cost for offenders to participate is a small fee (5 USD per day) to the national crime victim fund.

Also similar to most other countries, the use of EM in Sweden is combined with a curfew lasting 24/7 with leave only allowed for certain pre-authorized tasks such as work, studies, and participating in addiction treatment programs (see Bartels and Martinovic 2015). Failing to comply with either the curfew or the other rules implies a risk of having to serve the remainder of the sentence in prison. This happened to about 6 percent among those who started EM in 1997 (BRÅ 1999). The most common reason to abort EM was failure to comply with the ban on alcohol and drugs. In the first year of the reform, participants worked on average 31.5 hours per week and spent 2.9 hours in various addiction treatment programs offered by the Prison and Probation Service (e.g., alcohol and drug treatment).

Figure 1 summarizes key changes in the criminal justice system in Sweden during the relevant period, together with the evolution of actual incarcerations and EM participation.[12] The top panel of the figure shows that the EM pilot scheme that started on August 1st, 1994, in a limited number of probation centers only involved a small number of participants. On January 1st, 1997, EM was generalized to the whole of Sweden, for prison sentences of up to 3 months. This change corresponded to a large increase in the annual number of EM participants, from a few hundred to almost 5,000. This event coincided with a large decrease in the annual number of incarcerations,

[12] Data is from the annual publications *Kriminalstatistisk Årsbok 1993-2014* and *Rättsstatistisk Årsbok 1985-1992*.

which fell from about 14,000 to about 9,000. On April 1st, 2005, EM was extended to prison sentences of up to 6 months. The extension was followed by a small increase in the number of individuals on EM at the same time as overall incarcerations decreased slightly.

Our empirical analysis focuses on the large reform in 1997. On January 1st, 1999, the government introduced a new type of sentence, which combined probation with a requirement that the offender took part in some community service activities without compensation (e.g., elderly care or gardening). Community service was supposed to be used by the courts as an alternative to prison for low-risk offenders. This group clearly overlaps with the primary target group for EM. Consequently, we can see that the annual number of EM participants decreases after this reform (see Figure 1). To avoid conflating the two reforms, the empirical analysis focuses on offenders sentenced before January 1st, 1999.[13] In 1991, we see a drop in the number of incarcerations. This drop coincides with a reform that reduced the proportion of prison sentences for drunk drivers and implied that the share of aggravated drunk drivers who were sentenced to prison fell from about 70 percent the year before the reform to 42 percent. Since a relatively large share of the offenders who are granted EM are sentenced for drunk driving (see details below), we choose to start our analysis in 1992.

The lower panel of Figure 1 also displays the breakdown of the total number of admissions to EM by principal offence, grouped into broad categories. The vast majority of persons admitted to EM (between 70 and 80 percent) are those convicted for either gross drunk driving or violent crimes. Drunk driving alone accounts for between 50 and 60 percent of EM admissions. Almost all of the persons admitted to EM for traffic offences were convicted for aggravated drunk driving as opposed to simple drunk driving. Offenders sentenced to prison for violent crimes are the second largest group subject to EM (between 20 and 25 percent of all EM admissions). EM is rather uncommon for theft and drug related crime (less than 10 percent of persons admitted to EM).

2.2 How did the Reform Affect Incarceration?

Figure 2 (top panel) breaks down the total number of incarcerations over the observation period into two groups that are defined on the basis of the prison sentence length: (i) up to three months; (ii) between 4 and 12 months.[14] The figure confirms that that eligibility to EM was

[13] On January 1st, 1999, there was also another reform targeting prison sentences that increased the share of time inmates were required to serve from one half to two-thirds.

[14] The reason why the incarceration rate is below 100 percent for individuals sentenced to less than 3 months in prison is that those who have been detained before the trial are allowed to subtract off the length of their pre-trial

conditional on the length of the prison sentence. The figure clearly shows that, as expected, persons admitted to EM are only found among those who were sentenced to prison for up to 3 months. In this group, the number of incarcerations dropped by almost 50 percent when the reform was implemented in 1997. When we consider persons who were sentenced to between 4 and 12 months in prison, the number of incarcerations shows no evidence of a change in 1997. Importantly, we see that the pre-reform trends are highly similar in both groups.

Figure 2 also shows that the 2005 extension of EM to prison sentences of up to 6 months (instead of 3 months previously) was associated with a small decrease in the number of incarcerations for persons with prison sentences between 4 and 12 months. For offenders sentenced to up to 3 months, we see no change in the incarceration rate. However, because the magnitude of the reform in 2005 is relatively small and because it existed in parallel to the reform in 1999 that introduced community services that was used as a substitute for EM, we focus on the 1997 reform.

Figure 2 (lower panel) also plots the difference in the annual incarceration rate for offenders sentenced to up to three months relative to offenders sentenced to between 4 and 12 months. These difference-in-differences estimates are obtained from the following regression specification:[15]

$$Incarceration_{i,s} = \alpha + \sum_{\substack{k=1991 \\ k\neq 1996}}^{2004} \beta_k \cdot \mathbb{1}\{l_{i,s} \leq 3\}\mathbb{1}\{s = k\} + \gamma \cdot \mathbb{1}\{l_{i,s} \leq 3\} + \theta_s + \epsilon_{i,s}, \qquad (1)$$

where $Incarceration_{i,s}$ is an indicator for whether individual i, sentenced to a prison sentence in year s, was incarcerated, $\mathbb{1}\{l_{i,s} \leq 3\}$ is an indicator for whether the individual received a prison sentence length of up to 3 months, and θ_s are fixed effects for the year of prison sentence. The interaction term coefficients β_k are normalized to zero in 1996, so that the difference in incarceration rates between individuals with prison sentences of up to 3 months vs. of 4–12 months are measured relatively to the last pre-reform year.[16]

By plotting the β_k coefficients year-by-year, we are able to visually assess the underlying

detention from their prison sentence. The average pre-trial detention time in Sweden is about two months (BRÅ 2017). This explains why among individuals who receive short prison sentences of up to 3 months, approximately 20 percent are not incarcerated following their sentence. Moreover, in a few cases, offenders sentenced to prison in the district courts are acquitted in the appeal court.

[15] We weight the regressions by population size to replicate the underlying micro data.

[16] Strictly speaking, the coefficients β_k should be interpreted as the effects of the reform on the difference between "pseudo" incarceration rates, i.e., the incarceration rates that can be inferred from the aggregate statistics by dividing (i) the number of individuals who were incarcerated in a given year among those who received a prison sentence of a certain length by (ii) the number of individuals who received a prison sentence of the same length in the same year. These pseudo incarceration rates could differ slightly from the true incarceration rates if some individuals sentenced in a given year are not incarcerated in the same year.

assumption of common trends between the two groups of offenders. In the lower panel of Figure 2 we can see that, while all pre-reform coefficients are close to zero and in most cases also statistically insignificant, there is a sharp significant drop in the incarceration rate in 1997 when the reform is implemented. The point estimates suggest that the incarceration rate fell by about 30 percentage points in 1997 and remained persistently lower, although, as expected, we can see some signs of an increase in 1999 when the competing reform that introduced community service was introduced. All post-reform estimates are highly statistically significant.

3 Micro Data

Our micro data originate from several administrative registers collected and maintained by Statistics Sweden. The registers contain information on the entire Swedish population above age 15 each year from 1990 to 2016. These data have been linked to the Swedish Conviction Register and the Crime Suspicion Register kept by the National Council for Crime Prevention (BRÅ). We obtained complete records of all criminal convictions during the period. The data include information on crime type, date of the crime, as well as the sentence ruled by the court, and covers convictions in Swedish district courts (the court of first instance). One conviction may include several crimes and we observe all crimes within a single conviction. The conviction data exclude some minor offences such as speeding tickets, but include, e.g., driving without a license, and driving under the influence of alcohol or drugs. The Crime Suspicion Register contains information on individuals who are considered as likely suspects after the criminal investigation by the police or prosecutor. We refer to these individuals as having been arrested, since this is the closest analogy to the system used in many other countries.

We use the conviction register to identify the individuals who were sentenced to prison in 1992–1998 and extract information on their sentence length. From the registers, we are able to create measures of recidivism both in terms of re-convictions and re-arrests. We augmented the dataset with information on family ties, employment status, earnings, marital status as well as criminal convictions of the children. We also have data on the children's final grade point average (GPA) in compulsory school.

Table 1 shows sample averages for selected variables separately for the eligible or "treated" offenders, i.e., those sentenced to at most three months of prison and for the non-eligible control group with longer sentences. For the control group we include offenders sentenced to between 4 and 12 months in prison. While we use a wider interval for the control group to maximize

statistical precision, we also show that our estimates are not sensitive to using 6 months as the upper limit. We can see that there are no meaningful differences in average demographic characteristics or educational attainment. Eligible offenders are, however, more advantaged in terms of their baseline labor market characteristics, reflecting the fact that EM was more likely to be granted if the offender already had a job. We can also see that eligible offenders are less likely to have been convicted one year earlier. This reflects the fact that the treated offenders were sentenced for less severe types of crimes than the ineligible offenders. When looking at the offender outcomes measured within three years after the trial, it is clear that the eligible offenders have more favorable outcomes after the trial.[17] For instance, while 32.2 percent of the eligible offenders are employed three years after the trial, the corresponding figure for non-treated offenders is 15.5 percent. These differences may be produced by a multitude of factors and should not be given a causal interpretation.

4 Research Design

To estimate the causal effect of the reform, we adopt a research design similar to model (1), which uses non-eligible offenders as a control group to estimate the counterfactual. We restrict our sample to individuals sentenced to at most 12 months in prison. We further control for sentence length directly in the regressions. By doing so, we account for the possibility that offenders sentenced to longer prison terms, even within our already narrow bandwidth of 4 to 12 months, may differ in unobserved characteristics that potentially correlate with the outcomes we consider.[18] We estimate the following model:

$$Y_{i,s} = \alpha + \beta \cdot \mathbb{1}\{l_{i,s} \leq 3\} \cdot \mathbb{1}\{s \geq 1997\} + \gamma \mathbb{1}\{s \geq 1997\} + \lambda \mathbb{1}\{l_{i,s} \leq 3\} + \theta_s + X_{i,s}\mu + \epsilon_{i,s} \quad (2)$$

where $Y_{i,s}$ denotes the outcome of offender i who was sentenced to prison in year s, $\mathbb{1}\{s \geq 1997\}$ is an indicator for the prison sentence being imposed after the 1997 reform, and $\mathbb{1}\{l_{i,s} \leq 3\}$ an indicator for the sentence length being of up to 3 months. The year fixed effects, θ_s, control for nationwide changes affecting all offenders similarly. The vector $X_{i,s}$ controls for pre-determined individual characteristics (year of birth, gender, immigrant, educational attainment, and pre-

[17] We use a three year window since difference-in-differences comparisons over long time periods are challenging because of the potential for even modest differential trends to confound estimates. A three-year follow-up period is also a standard used by the Swedish National Institute for Crime Prevention when reporting recidivism statistics.

[18] We are unable to estimate a regression discontinuity model. The reason is that, with a few exceptions, courts tend to sentence individuals to integer months, meaning that the offenders in our sample in practice are concentrated at only a few mass points: at one, two, and three months.

reform earnings and employment). We also control for court and crime type and linearly for sentence length. Under the standard assumption of common trends, the coefficient β recovers the intention-to-treat (ITT) effect of increased access to EM on offenders' outcomes.

4.1 Validating the Research Design

We have already seen that the pre-reform trends in the incarceration rates are close to parallel for eligible and non-eligible offenders, suggesting that the key assumption indeed holds. Figure A1 in the Appendix shows that this is also the case when considering the labor market and crime outcomes of the offenders. Another potential concern is that the courts may have adjusted their sentencing practices in response to the reform, perhaps by manipulating the sentence length to ensure that some offenders would not escape a prison term. While this issue is partially alleviated by the fact that the use of EM was uncoupled from the courts and instead controlled by the Board of Prison and Rehabilitation after court sentencing, this does not completely discard this risk. To address this concern empirically, we exploit the idea that any change in sentencing practice should show up as a change in the share of offenders in our sample that are sentenced to up to three months in prison. Figure A2 in the Appendix plots the probability of being sentenced to up to three months in prison in our sample against the month of the trial relative to the month of the reform. The grey shaded area is the associated 95 percent confidence interval. We find no significant change in the probability of being sentenced to up to three months when the reform came into effect.

Having verified empirically that there are no visible differential pre-trends in the outcomes, and no significant changes in the density of the eligible sample around the time of the reform, we next turn to our main results.

5 Results

In this section, we examine the causal effects of the 1997 EM reform on the offender's own outcomes. We then probe the robustness of our results and investigate treatment effect heterogeneity. We continue by attempting to shed some light on the underlying mechanisms. Finally, we examine the effects of EM on the children and the other non-convicted parent.

5.1 Effects of EM on the Offenders

Table 2 presents our difference-in-differences estimates of the impact of electronic monitoring on offender outcomes, where the coefficient displayed is the coefficient β in Model (2) on the interaction between the indicator for being eligible and the post-reform indicator. All outcomes are measured as averages (by offender) over the first three years after conviction. Criminal recidivism is either an indicator for having a new conviction or an indicator for having been arrested. Employment is an indicator for being registered as formally employed. Earnings are defined as total annual labor earnings. Disposable income is total post-tax income from labor, capital, and transfers. Column (1) shows the results from a baseline model that only controls for demographic characteristics and the main effects. Columns (2) through (5) sequentially include additional controls, ending with our preferred specification that controls for demographics, pre-determined socioeconomic characteristics, court and crime type fixed effects and sentence length.[19]

Consistent with previous work (Di Tella and Schargrodsky 2013; Henneguelle et al. 2016; Williams and Weatherburn 2020), the estimates in Panel A show that offenders eligible for EM are significantly less likely to re-offend. For example, the estimates from our preferred specification in column (5) show that eligible offenders are 1.3 percentage-point less likely to be re-convicted in the first three years after the trial compared to non-eligible offenders, a 2.1 percent decrease from the baseline mean of 62.8 percent. This result holds also when measuring criminal recidivism using arrest data: the decrease is of 2.7 percentage points in column (5), or 4.4 percent, relative to the baseline mean. The results for labor market outcomes are shown in Panel B. It is clear that eligible offenders are much more likely to be employed and also have higher incomes. Our preferred estimates indicate that the reform increased the probability of being employed for eligible offenders by 3.4 percentage points (12.7 percent) and led to a 22.4 percent increase in annual earnings (13,326/59,347). We also see a significant increase in disposable income (6.7 percent).

The estimates are strongly robust to sequentially adding more controls. The only exception is the small drop in the effect size that is observed when we control for pre-determined socioeconomic characteristics (column 2). As we will see later, this finding likely reflects treatment effect

[19] Controlling for sentence length accounts for unobserved heterogeneity among individuals further away from the cutoff, but the problem with few mass points in sentence length prevents us from estimating a standard RD-model with differential slopes and higher order polynomials in sentence length. We are, however, able to complement our research design with results from an alternative RD-in time (RDiT) identification strategy that supports our main results.

heterogeneity. Many of the included controls are strong correlates of both the eligibility status and the outcomes we consider. Thus, the fact that the estimates hardly change when adding key controls such as sentence length suggests that other potential omitted factors are unlikely to bias the estimates in any meaningful way. We provide a number of other robustness checks in the appendix. Table A1 shows that the estimates in the first through the third year after the trial are similar, indicating that our results are not driven by a mechanical (anti-)incapacitation effect. In Table A2, we find similar results when shrinking the bandwidth for inclusion in the control group from 4–12 months to 4–6 months. We also estimated a model where the reform year was set one year prior to the true reform, conditional on the actual reform. We find much smaller estimates than in the baseline specification, most of which are statistically insignificant (see Table A2). Finally, we estimated a RDiT-model based on the time in months between the trial and the reform. In this analysis, we restricted the sample to eligible offenders sentenced within one year around the reform date while controlling linearly for time until reform. A smaller sample size coupled with the fact that an RD-model effectively only uses observations close to the cutoff for identification makes the RDiT-estimates less precise. While the 95 percent confidence interval overlaps with our DiD estimates the RD-estimates for criminal recidivism are, however, not statistically significant. The estimates for labor market outcomes are highly similar and significant.

If one is willing to assume that the interaction between eligibility status and post-reform is not correlated with the error term in Model (2), and does not directly influence the outcomes of the offenders other than through increasing the take-up of EM, then it is possible to scale these ITT estimates by the effect of the reform on the take-up of EM to obtain the implied instrumental variable estimates, which can be interpreted as local average treatment effects (LATEs), i.e., the average treatment effects for compliers. Doing so shows that using EM has economically significant effects on the offenders' outcomes. For instance, the implied instrumental variable estimates suggest that EM decreases the probability of being re-convicted by 4.3 (1.3/.30) percentage points. These estimates are smaller than the IV estimates in Di Tella and Schargrodsky (2013), showing that EM leads to a 15 percentage-point decrease in the prison recidivism rate in Argentina, and also smaller than the 22 percentage-point reduction in re-offending in Australia shown by Williams and Weatherburn (2020). The difference in results can possibly be attributed to differences in institutional context (e.g. the effect is partly offset by the extensive Swedish welfare state) as well as to potential differences in the characteristics of compliers.

While there are no similar studies looking at labor market outcomes, our implied IV-estimates suggests that EM increases the likelihood of being employed by 11.3 percentage points. To put our estimates in perspective, we conducted a partial back-of-the-envelope calculation where we compared our earnings estimates with estimates of the fiscal savings from EM. The Board of Prison and Rehabilitation evaluates the daily costs for one prisoner in a low security prison to be of 1,645 SEK (235 USD) and the cost for one client participating in the EM-program to be of 400 SEK (55 USD) (BRÅ 1999). This means that the direct fiscal savings from EM for a client that uses EM for three months is 112,050 SEK (90*(1,645-400)) or 12,600 USD. In Table 2, we showed that exposure to EM increases annual earnings over the first three post-sentence years by 1,431 USD (13,326 SEK). We scale the upper and lower bounds of the 95% confidence interval by the estimated drop in the incarceration rate (.30). Assuming that the effect is constant over the rest of the lifetime and discounting future earnings through age 67 back to age 33 (the mean age in our sample) with a discount rate of 3 percent, we find that EM participants earn between 70,000 and 103,000 USD more in present value over their remaining years in the labor force compared to non-participants. We caution that these long-run calculations are speculative because it is not certain how well the effect that we document approximates the impact on lifetime earnings. Still, these estimates suggest that the increase in earnings alone from EM participation is between 6 and 9 times larger than the direct fiscal savings from EM. Because of the difficulties involved in evaluating the social benefits from crime reductions, we refrain from undertaking this exercise, but note that most studies attempting to quantify the social costs of criminal involvement find that these costs are substantial (e.g., Mueller-Smith 2016). These benefits would be on top on the welfare gains from improved labor market outcomes.

5.2 Treatment Effect Heterogeneity

Table 3 presents results from regressions where we partition the sample into relevant subgroups to investigate treatment effect heterogeneity. In columns (1) and (2), we can see that while most estimates are smaller for females, they essentially show the same pattern as for males. Since there are only 4,603 females in the sample (compared to 76,383 males), these estimates are however less precise. When stratifying the sample by (median) age, we see that there is a tendency for slightly larger improvements in the outcomes among those individuals who were aged 18–29 (see column 3) when they were convicted than for those aged 30–59 (see column 4). Lastly, when splitting the sample by employment status, we see that the effect size is larger for individuals who were employed at baseline (column 5). We will return to this result in the next

sub-section where we discuss potential mechanisms.

Table A3 and Table A4 in the Appendix present the results from some additional subgroup analyses. While there are stronger beneficial effects of increased access to EM among offenders with no prior criminal history, we also find improvements in the outcomes of offenders with criminal records. One implication of this finding is that EM may offer some benefits also to more hardened criminals. When splitting the sample by level of education, we find slightly larger effects for high educated offenders but there are significant improvements in the outcomes also for low educated offenders.

Table A4 shows separate estimates for the four most common types of crime among offenders who were granted EM during the period we study: property crime, violent crime, drug related crime, and drunk driving. We can see that the benefits of increased access to EM are largely concentrated among offenders sentenced for violent crimes or drunk driving. By contrast, most estimates for property crime and drug related offences are smaller in magnitude and often not statistically significant.

5.3 Mechanisms

In this section, we explore four potential mechanisms that might explain our findings: (1) EM hinders offenders from accumulating criminal capital behind bars, (2) it preserves family relationships, (3) it increases the potential for offenders to maintain or find jobs, and (4) it reduces employer discrimination. Our results are shown in Table 4.

Criminal capital. The idea that spending time in prison allows inmates to learn how to commit certain types of crimes is not new in the literature. For example, Bayer et al. (2009) present compelling evidence that individuals who serve time with offenders having a similar criminal background are more likely to repeat the same type of crime.[20] We examine this potential mechanism by distinguishing between types of crimes for which there is a higher element of learning involved and crimes that one is less likely to learn in prison. Examples of acquired types of crime are property crime and drug related crime (e.g., producing or selling), while examples of non-acquired crimes are violent crimes and drunk driving (see Bayer et al. 2009). We estimate regressions where the dependent variable is an indicator for being re-convicted for

[20] Stevens (2017) shows that exposure to young inmates who are from homes that are less stable and who have behavioral problems increases recidivism but finds little evidence of skill transfer or network formation mechanisms. Other studies showing evidence of the importance of peer effects in criminal behavior include e.g. Glaeser et al. (1996); Ballester et al. (2006); Billings et al. (2019).

acquired crimes and an indicator for being re-convicted for non-acquired crimes. As can be seen in Table 4, the estimate for acquired crimes is close to zero and statistically insignificant. By contrast, the estimate for non-acquired crimes is significant and shows that the reform led to a 2 percentage-points (5.2 percent) decrease in the likelihood of being re-convicted. This finding is inconsistent with the notion that EM prevents criminals from accumulating criminal capital behind bars.[21]

Family ties. Stable marriages are believed to provide a pathway out of crime for adult offenders, but imprisonment may increase the risk of separation (e.g., Apel et al. 2010). In the context of Denmark, Fallesen and Andersen (2017) show that EM significantly lowers the risk of relationship dissolution during the first five years following conviction, possibly because EM may lower the risk for household strain due to a decrease in the offender's financial and emotional support to their partner during imprisonment (e.g., Apel et al. 2010). To investigate this possibility, we estimate regressions where the dependent variable is an indicator for being divorced (or separated). While a large share of offenders (40.5 percent) divorce or separate within three years after their conviction, the results in Table 4 show no significant effect of increased access to EM on the likelihood of divorcing.

Labor market attachment. Regular employment is one of the strongest predictors of rehabilitation success and it is therefore not surprising that having a job or being willing to search for one are key ingredients in most EM programs (e.g., ETUI 2019; Williams and Weatherburn 2020). Theoretical models further suggest that preventing the depreciation of human capital that occurs in prison when skills are unused may also improve offenders' employment prospects also in the long run (e.g., Lochner 2013). While the evidence is mixed, many empirical studies show that incarceration deteriorates labor market outcomes (e.g., Mueller-Smith 2015; Dobbie et al. 2018).[22] Our results in Table 3 where we stratified the sample by baseline employment status prior to conviction showed that the significant improvements in outcomes of the offenders were more heavily concentrated among offenders with more stable connections to the labor market before they were convicted. This finding suggests that maintaining labor market attachment seems to be an important mechanism producing the beneficial effects of EM. It is, however, interesting to note that we also find significant improvements in the offender outcomes among

[21] It should be noted, however, that because EM is only granted to offenders receiving shorter sentences, our findings do not rule out the possibility that there are skill transfers between prisoners serving longer sentences.

[22] Most prisons and jails offer some type of training or education, suggesting that it is possible for prison inmates to also invest during incarceration periods. However, the fact that most estimates indicate a decline in earnings and employment following a prison term suggests that losses in earnings potential due to human capital depreciation (and possibly also other mechanisms) outweigh any gains from new skills that may be acquired in prison.

those who were less well connected (column (5)) to the regular labor market. One reason for this could be because of increased job search intensity when enrolled in EM.

Employer discrimination. EM may also improve labor market outcomes by reducing employer discrimination against ex-prisoners (e.g., Western et al. 2001; Lofstrom and Raphael 2016). We attempt to shed light on this mechanism by exploiting that our data contain information on some firm quality characteristics, in particular average earnings and the share of university educated co-workers. This exercise should nonetheless be interpreted with caution since these measures are likely plagued by worker sorting across firms. Moreover, any change in firm quality may also reflect changes in own behavior. With these caveats in mind, our results suggest that increased access to EM significantly improves firm quality: the results in Table 4 show that the EM reform was associated with a 1.8 percent increase in the average earnings of eligible offenders' co-workers and a 1.3 percentage-point increase in the share having a university education. This at least provides tentative evidence that reduced employer discrimination against ex-prisoners may play some role in EM improving offender outcomes.

In summary, the above results do not provide strong support to the notion that EM improves outcomes by preventing offenders from accumulating criminal capital in prison or by allowing them to maintain family stability. Rather, they are consistent with the social benefits from EM being least partially driven by the fact that it allows offenders to maintain employment and possibly also by reduced discriminatory behavior of employers towards ex-prisoners. More research is however needed to verify these results.

5.4 Spillover Effects on Family Members

We have seen that increased access to EM significantly improves the future outcomes of offenders. In Table A5 in the Appendix we show that these findings also hold for the subset of offenders with children. There are plenty of reasons to expect the reform to have also affected individuals connected to the offender, even if the direction of the effect is uncertain. For instance, children may benefit from increased family resources or from not having to experience the trauma or stigma of having a parent sent to prison, as suggested by Wildeman (2010) and Murray et al. (2012). On the other hand, children may suffer from keeping a potentially bad role model in the home. Similarly, spouses may either benefit or suffer from their partner serving time at home under curfew rather than in prison. For instance, there may be a reverse "added worker" effect in which the improved labor market outcomes experienced by the convicted head of the

household reduces the labor supply of the spouse (e.g., Lundberg 1985). Increased possibilities to share household tasks and child rearing may also allow the spouse to allocate more time to work, making the net effect ambiguous.

Following Dobbie et al. (2018) we investigate this empirically by studying the outcomes of the children aged 11–14 at the time of the trial, a critical period in a child's life when they may be particularly sensitive to shocks to the home environment. We also study their non-convicted parent. Table 5 shows estimates of the impact of the reform on the children's outcomes: Criminal conviction at ages 15–17 is a dummy for the child having been convicted between age 15 and 17; Compulsory school GPA is the percentile ranked (by cohort) final grade point average in compulsory school (age 16); High school diploma at age 19 is a dummy for having completed high school by age 25; Employment at age 25 is a dummy for being employed at age 25; Earnings is total annual labor earnings at age 25. As before, the first column reports variable means while columns (2) through (5) report the estimates from specifications where we add additional controls. The standard errors are clustered at the family level.

We find that parental exposure to the reform significantly increases the probability that the child has a compulsory school degree at age 16 by 2.9 percentage points (3.5 percent of sample mean). We also find that parental exposure increases the child's disposable income at age 25 by 6.2 percent when evaluated against the sample mean. While most estimates are statistically insignificant, the overall pattern of the estimates suggests improved outcomes. The estimates are precise enough to rule out large adverse effects of the reform on the children. For instance, the upper limit of the 95 percent confidence interval for criminal conviction and arrest rules out effect sizes larger than 0.028 and 0.019 percentage point increases in the likelihood of conviction and arrest, respectively.

Table 6 presents the results of the effect of the reform on the outcomes of the other non-convicted parent. We do not find any significant effect of the reform on the outcomes of the other parent. The estimates are, however, relatively imprecise.

Our findings are broadly consistent with the results in Dobbie et al. (2018) who find negative effects of parental incarceration on the outcomes of their children. Our estimates are however less precise and smaller in magnitude.[23]

In summary, while we do find evidence of significant positive effects for some outcomes of the

[23] A difference between the two studies is that the populations included are different: while Dobbie et al. identify the effect for the universe of offenders sentenced to incarceration, we study relatively low-risk offenders who are sentenced to short prison terms and are then approved for EM.

family members, our key finding is that the estimates are precise enough to rule out large adverse effects.

6 Concluding Remarks

EM is widely used throughout the world to combat the high costs of large prison systems. Yet, even though the anticipated effects of EM on the offenders themselves are ambiguous, there is surprisingly little rigorous empirical evidence, especially concerning labor market outcomes. Moreover, data limitations have prevented past research from learning about potentially important spillover effects on partners and children.

We present evidence from Sweden's early introduction of EM in 1997 whereby individuals sentenced to three months in prison became eligible for EM as an alternative to prison. Our difference-in-differences estimates that use offenders above the eligibility cutoff as a control group show that the increased access to EM significantly lowered criminal recidivism between 2.1 and 4.4 percent and boosted earnings and employment by 22.5 and 12.7 percent, respectively. The key mechanism behind these improvements seems to be that EM provides an opportunity for offenders to keep their jobs or search for new ones. Tentative evidence further suggests that reduced discriminatory behavior of employers may play some role in explaining the improved outcomes, although more research is needed to confirm this result. While the benefits of EM are concentrated on the offenders themselves, the reform does not generate collateral damage in terms of worse outcomes for the family members. If anything, we find that parental exposure to the reform improves some of their children's outcomes. We therefore conclude that EM likely implies social benefits beyond its direct fiscal savings and that these benefits are much larger than what has previously been recognized. Our back-of-the-envelope calculations suggest that the benefits just in terms of improved earnings of the offenders are between 6 to 9 times the direct fiscal savings from reduced prison staff.

Like the other Nordic countries, Sweden has a much more generous social welfare system than most other OECD countries. This social welfare system includes high-quality health care and education programs for children, as well as generous public income security programs for adults. Child care is also highly subsidized, with about 90 percent of the costs covered by the state for most families. In addition, every child has equal access to free education in their local area and at universities. Families are also eligible for a means-tested social aid program that provides economic resources sufficient to keep them out of poverty as a last resort. All of these programs

are considerably more generous than their equivalents in many other countries. It is therefore possible that the social benefits of EM in other countries with less generous welfare systems may be even larger. Our implied IV estimates for recidivism are certainly smaller than the effects of EM in Argentina, where Di Tella and Schargrodsky (2013) find that EM reduces the recidivism rate by 15 percent. That said, unlike Sweden, many countries exclude offenders sentenced for violent crimes from eligibility to EM. These are offenders who may have particularly poor prospects to rehabilitate under EM. Moreover, despite the large welfare state, there are no specific supports provided to the children of incarcerated parents in Sweden, nor are there official efforts to even identify these children by school or government administrators. In this respect, Sweden is not very different from other countries. Finally, it should be noted that we are unable to estimate the deterrence effects of a less strict incarceration policy on the population at large (e.g., Chalfin and McCrary 2017). Our analysis may therefore understate the social costs of EM. This is also an important avenue for future research.[24]

[24]In their literature review, Chalfin and McCrary (2017) conclude that"within the range of typical changes to sanctions in contemporary criminal-justice systems, the evidence suggests that the magnitude of deterrence owing to more severe sentencing is not large´´, and that "the current elasticity of crime with respect to prison populations is approximately 0.2´´.

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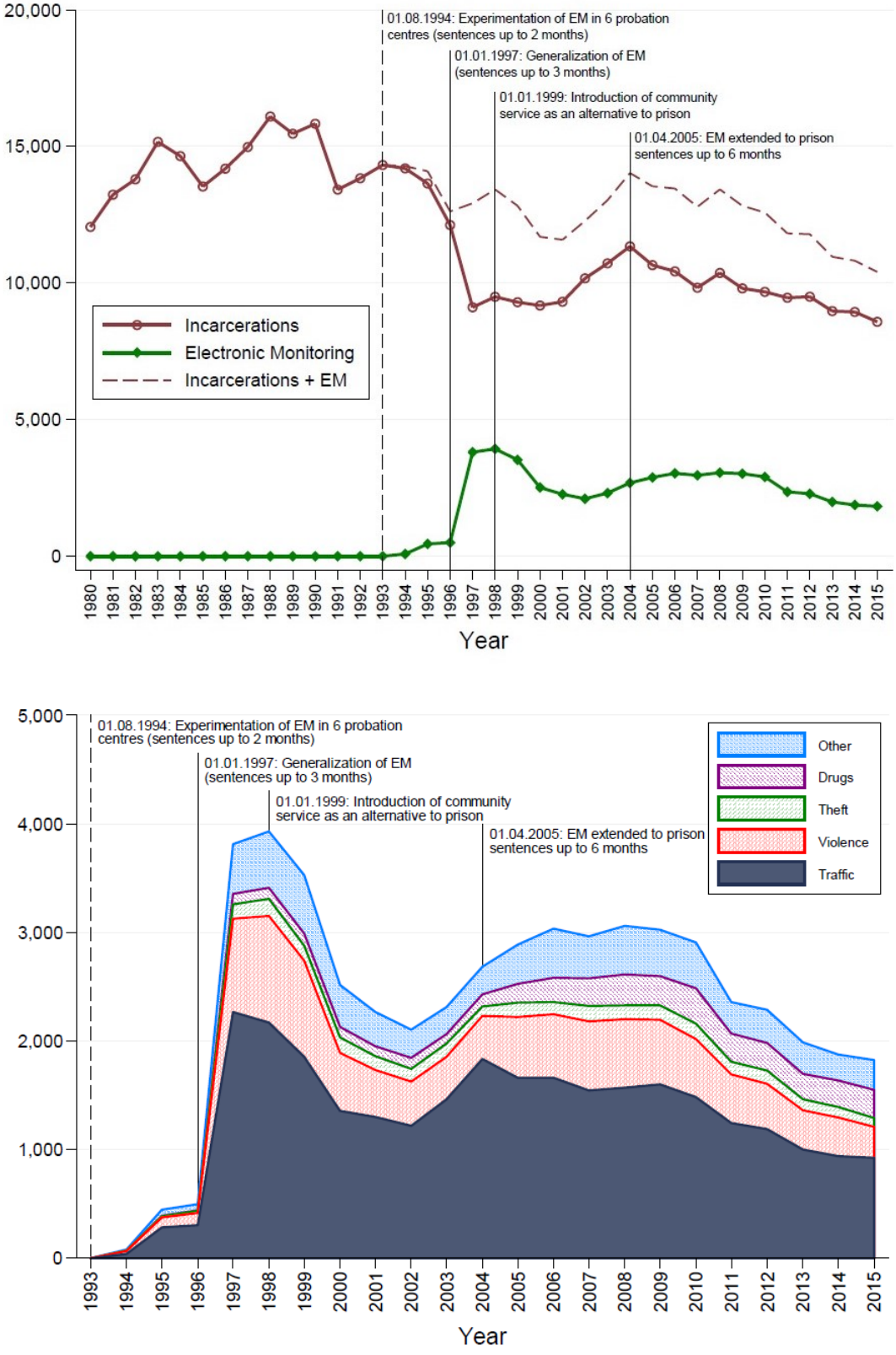


Figure 1: Incarceration and Electronic Monitoring in Sweden, 1980-2015

Notes: The figure is based on publicly available data from the Board of Prison Services (*Rättsstatistisk Årsbok 1985-1992* and *Kriminalstatistisk Årsbok 1993-2014*). The upper panel shows the number of individuals incarcerated and the number of individuals admitted to EM 1980–2015. The lower panel shows the breakdown of the total number of admissions to EM by main offence over the period 1993–2015.

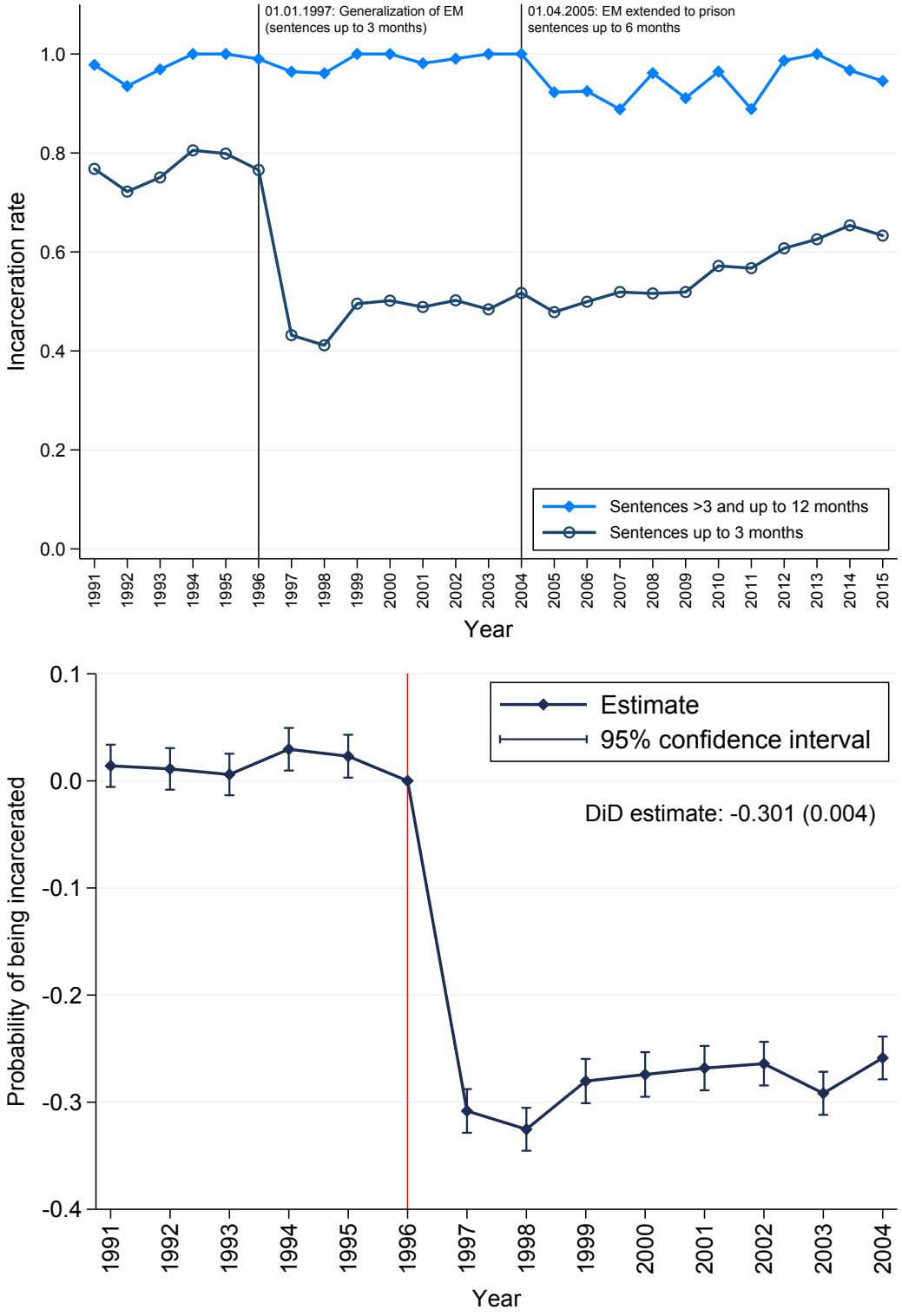


Figure 2: Incarceration rates for individuals with prison sentences up to 12 months

Notes: The figure is based on publicly available data from the Board of Prison Services (*Rättsstatistisk Årsbok 1985-1992* and *Kriminalstatistisk Årsbok 1993-2014*. The incarceration rate is below unity because any potential pre-trial detention is subtracted from the sentence. The upper panel shows the raw trends. The lower panel shows the difference in the annual incarceration rate between offenders sentenced to up to three months relative to offenders sentenced to between 4 and 12 months (i.e., difference-in-differences estimates). The estimates are relative to 1996, which is the last year before the reform.

Table 1: Descriptive Statistics of the Offenders, by Eligibility Status

	Eligible Offender	Non-Eligible Offender
Panel A: Offender Characteristics and Baseline Outcomes	(1)	(2)
Male	0.940	0.949
Native born	0.782	0.772
Age at trial	34.346	33.301
Less than high school	0.470	0.535
High school	0.483	0.436
More than high school	0.046	0.030
Employment in 1 year before trial	0.340	0.176
Earnings (100s SEK) in 1 year before trial	676.940	332.815
Criminal conviction in 1 year before trial	0.410	0.598
Panel B: Type of crime		
Property offense	0.187	0.415
Violent offense	0.214	0.219
Drunk driving offense	0.348	0.032
Drug offense	0.051	0.107
Other offense	0.200	0.227
Panel C: Offender Outcomes		
Convicted of new crime	0.567	0.757
Arrested for new crime	0.552	0.741
Employment	0.322	0.155
Earnings (100s SEK)	727.928	313.062
Disposable income (100s SEK)	1,123.075	856.163
Observations	54,691	26,295

Note: The table shows mean values for variables included in the analysis. The eligible group consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the non-eligible control group consists of individuals sentenced to between 4 and 12 months in the same period. The baseline variables in Panel A are measured in one year before the initial trial. The offender outcomes in Panel C are measured as an average within three years after the trial.

Table 2: Main results of the effect of the 1997 EM expansion on offenders' criminal recidivism and labor market outcomes

	Outcome Mean (SD)	(1)	(2)	(3)	(4)	(5)
Panel A: Crime						
Convicted of new crime	0.628	−0.020***	−0.015**	−0.016**	−0.013*	−0.013*
	(0.483)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Arrested for new crime	0.614	−0.035***	−0.030***	−0.031***	−0.028***	−0.027***
	(0.487)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Panel B: Labor Market						
Employment	0.268	0.042***	0.037***	0.037***	0.034***	0.034***
	(0.387)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)
Earnings (100s)	593.468	155.146***	141.577***	140.817***	134.693***	133.255***
	(986.284)	(15.248)	(12.792)	(12.810)	(12.657)	(12.663)
Disposable income (100s)	1,036.568	77.819***	71.617***	73.433***	70.554***	69.580***
	(717.732)	(11.117)	(10.402)	(10.367)	(10.239)	(10.239)
Year FE	–	Yes	Yes	Yes	Yes	Yes
Demographic controls	–	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls	–	–	Yes	Yes	Yes	Yes
Court FE	–	–	–	Yes	Yes	Yes
Crime FE	–	–	–	–	Yes	Yes
Sentence length	–	–	–	–	–	Yes
Observations		80,986	80,986	80,986	80,986	80,986

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on offender outcomes. The coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group (54,691 individuals) consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the control group (26,295 individuals) are offenders sentenced to between 4 and 12 months during the same period. All outcomes are measured as an average over the three years following the trial. Criminal recidivism is either an indicator for having a new conviction or an indicator for having been re-arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). Column (1) reports estimates controlling for pre-determined demographic characteristics (year of birth, gender, immigrant) and the main effects. Column (2) additionally controls for socioeconomic characteristics: educational attainment, and pre-sentence (one year lag) earnings and employment controls. Column (3) expands the set of controls to also include court and municipality fixed effects. Column (4) controls for crime type fixed effects. Column (5) is our preferred specification that also controls linearly for sentence length (in months). Robust standard errors are shown in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

Table 3: Subgroup Results

	Man	Woman	Aged 18–29	Aged 30–59	Employed	Not employed
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Crime						
Convicted of new crime	−0.015**	−0.004	−0.024**	−0.010	−0.000	−0.012
	(0.007)	(0.028)	(0.011)	(0.009)	(0.014)	(0.008)
	[0.631]	[0.585]	[0.641]	[0.620]	[0.454]	[0.750]
Arrested of new crime	−0.027***	−0.045	−0.039***	−0.023***	−0.030**	−0.017**
	(0.007)	(0.028)	(0.011)	(0.008)	(0.014)	(0.008)
	[0.616]	[0.574]	[0.614]	[0.613]	[0.476]	[0.736]
Panel B: Labor Market						
Employment	0.034***	0.040**	0.042***	0.032***	0.027**	0.019***
	(0.005)	(0.018)	(0.009)	(0.006)	(0.012)	(0.005)
	[0.272]	[0.209]	[0.295]	[0.252]	[0.473]	[0.126]
Earnings (100s)	135.402***	105.662***	148.455***	129.244***	167.758***	37.624***
	(13.245)	(37.492)	(19.185)	(16.530)	(31.609)	(10.056)
	[604.449]	[413.024]	[620.221]	[577.021]	[1,110.271]	[242.496]
Disposable income (100s)	70.238***	46.935	65.392***	76.408***	99.772***	22.730**
	(10.522)	(45.398)	(15.583)	(13.375)	(20.939)	(11.225)
	[1,030.991]	[1,128.198]	[948.366]	[1,090.792]	[1,290.040]	[858.630]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes	Yes	Yes
Crime FE	Yes	Yes	Yes	Yes	Yes	Yes
Sentence length	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,383	4,603	30,660	50,326	29,965	45,487

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on offender outcomes. The coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group (54,691 individuals) consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the control group (26,295 individuals) are offenders sentenced to between 4 and 12 months during the same period. All outcomes are measured as an average over the three years following the trial. Criminal recidivism is either an indicator for having a new conviction or an indicator for having been re-arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). All regressions control for pre-determined demographic characteristics (year of birth, gender, immigrant), the main effects, socioeconomic characteristics (educational attainment, and pre-sentence (one year lag) earnings and employment controls), court and municipality fixed effects, crime type fixed effects and linearly for sentence length (in months). Employed/Not-Employed in columns (5) and (6) is measured in the year before the trial. Robust standard errors are shown in parenthesis and sub-sample means are shown in brackets. */**/*** denote significant at the 10/5/1 percent level.

Table 4: Mechanisms

Panel A: Criminal Capital	Outcome Mean (SD)	(1)
Acquired crimes	0.413	−0.004
	(0.492)	(0.007)
Non-aquired crimes	0.382	−0.020**
	(0.486)	(0.008)
Panel B: Firm Quality		
Average earnings (100s)	1,883.490	35.188*
	(760.914)	(21.204)
Share of university educated	0.170	0.013***
	(0.171)	(0.005)
Panel C: Familiy Ties		
Separation/divorce	0.405	0.000
	(0.491)	(0.022)
Year FE	–	Yes
Demographic controls	–	Yes
Socioeconomic controls	–	Yes
Court FE	–	Yes
Crime FE	–	Yes
Sentence length	–	Yes
Observations		80,986

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on offender outcomes where the coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group (54,691 individuals) consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the control group (26,295 individuals) are offenders sentenced to between 4 and 12 months during the same period. Acquired crimes include property crime and selling of drugs and non-acquired crimes include violent crimes and drunk driving. Firm quality is measured conditional on employment. Robust standard errors in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

Table 5: Effects on Children

	Outcome Mean (SD)	(1)	(2)	(3)	(4)	(5)
Panel A: Teen Crime						
Criminal conviction at ages 15-17	0.256	−0.016	−0.013	−0.009	−0.008	−0.008
	(0.437)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Crime arrest at ages 15-17	0.292	−0.028	−0.024	−0.020	−0.019	−0.019
	(0.455)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
Panel B: Teen Educational Outcomes						
Compulsory school GPA (pct. rank)	28.275	1.921*	1.407	1.145	1.138	1.153
	(25.111)	(1.089)	(1.063)	(1.066)	(1.067)	(1.067)
Compulsory school degree at age 16	0.825	0.035**	0.031*	0.029*	0.030*	0.029*
	(0.380)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
High school diploma at age 19	0.368	0.026	0.020	0.012	0.012	0.011
	(0.482)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Panel C: Adult Labor Market Outcomes						
Employment at age 25	0.600	0.014	0.012	0.011	0.011	0.011
	(0.490)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Earnings at age 25	1,427.909	88.619	84.677	80.894	80.979	80.998
	(1,285.432)	(59.653)	(59.378)	(59.221)	(59.096)	(59.154)
Disposable income at age 25	1,508.372	94.847**	93.693**	95.680**	95.695**	95.379**
	(825.757)	(37.310)	(37.397)	(37.599)	(37.697)	(37.679)
Year FE	–	Yes	Yes	Yes	Yes	Yes
Demographic Controls	–	Yes	Yes	Yes	Yes	Yes
Socioeconomic Controls	–	–	Yes	Yes	Yes	Yes
Court FE	–	–	–	Yes	Yes	Yes
Crime FE	–	–	–	–	Yes	Yes
Sentence length	–	–	–	–	–	Yes
Observations		12,530	12,530	12,530	12,530	12,530

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on the outcomes of the children of the offenders where the coefficient displayed is on the interaction between an indicator for being child to an offender eligible for EM and an indicator for post-reform. The children are aged 11-14 at the time of the trial. The eligible group consists of children to the eligible offenders sentenced to at most three months in prison between 1992 and 1999 and the ineligible group are children to the offenders sentenced to between 4 and 12 months during the same period. Teen criminal behavior is measured using either an indicator for having a conviction at ages 15-17 or an indicator for having been arrested at ages 15-17. Teen educational outcomes are either an indicator for having a compulsory school degree by age 16 or the percentile rank (by cohort) of the final grade point average in compulsory school. Adult labor market outcomes are measured either as an indicator for being employed at age 25, total annual labor earnings (SEK) at age 25 or disposable income (SEK) at age 25. Standard errors clustered at the family level in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

Table 6: Effects on other (Non-Convicted) Parent

	Outcome Mean (SD)	(1)	(2)	(3)	(4)	(5)
Panel A: Crime						
Convicted of a crime	0.132	0.008	0.011	0.012	0.013	0.013
	(0.338)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Arrested of a crime	0.065	−0.018	−0.018	−0.016	−0.016	−0.016
	(0.246)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Panel B: Labor Market						
Employment	0.536	0.015	0.010	0.004	0.004	0.004
	(0.449)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Earnings (100s)	1,088.915	55.184	39.062	29.166	27.186	27.520
	(1,055.742)	(51.156)	(50.311)	(49.995)	(49.853)	(49.885)
Disposable income (100s)	1,903.259	−25.055	−31.753	−29.901	−31.797	−30.772
	(701.010)	(33.998)	(33.771)	(33.924)	(33.836)	(33.745)
Year FE	–	Yes	Yes	Yes	Yes	Yes
Demographic Controls	–	Yes	Yes	Yes	Yes	Yes
Socioeconomic Controls	–	–	Yes	Yes	Yes	Yes
Court FE	–	–	–	Yes	Yes	Yes
Crime FE	–	–	–	–	Yes	Yes
Sentence length	–	–	–	–	–	Yes
Observations		12,530	12,530	12,530	12,530	12,530

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on the outcomes of the other (non-convicted) parent where the coefficient displayed is on the interaction between an indicator for being partner to an offender eligible for EM and an indicator for post-reform. The eligible group consists of partners to the eligible offenders sentenced to at most three months in prison between 1992 and 1999 and the ineligible group are partners to the offenders sentenced to between 4 and 12 months during the same period. All outcomes are measured as an average within three years after the trial. Criminal behavior is measured using either an indicator for having a conviction or an indicator for having been arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). Standard errors clustered at the family level in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

A Additional Results

Table A1: Offender Labor Market Outcomes 1 to 3 years after Conviction

	Outcome Mean	(1)	(2)	(3)	(4)	(5)
Employment 1 year after trial	0.245	0.049***	0.044***	0.043***	0.041***	0.041***
	(0.430)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Employment 2 years after trial	0.274	0.039***	0.035***	0.034***	0.032***	0.031***
	(0.446)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Employment 3 years after trial	0.295	0.039***	0.035***	0.035***	0.032***	0.032***
	(0.456)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Earnings (100s) 1 year after trial	522.198	151.247***	137.004***	136.542***	130.830***	129.469***
	(970.923)	(14.871)	(12.388)	(12.397)	(12.277)	(12.279)
Earnings (100s) 2 years after trial	602.140	163.338***	150.353***	149.810***	143.227***	141.684***
	(1,058.597)	(16.529)	(14.273)	(14.292)	(14.156)	(14.164)
Earnings (100s) 3 years after trial	677.794	160.083***	146.423***	145.808***	138.395***	136.999***
	(1,160.888)	(19.327)	(17.247)	(17.273)	(17.108)	(17.115)
Year FE	–	Yes	Yes	Yes	Yes	Yes
Demographic controls	–	Yes	Yes	Yes	Yes	Yes
Socioeconomic controls	–	–	Yes	Yes	Yes	Yes
Court FE	–	–	–	Yes	Yes	Yes
Crime FE	–	–	–	–	Yes	Yes
Sentence length	–	–	–	–	–	Yes
Observations		80,986	80,986	80,986	80,986	80,986

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on offender outcomes where the coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group (54,691 individuals) consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the control group (26,295 individuals) are offenders sentenced to between 4 and 12 months during the same period. The outcomes are measured as an average within three years after the trial. Criminal recidivism is either an indicator for having a new conviction or an indicator for having been re-arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). Robust standard errors are shown in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

Table A2: Robustness Checks

	Control group 4-6 months (1)	Placebo reform in 1996 (2)	Regression discontinuity (3)
Panel A: Crime			
Convicted of new crime	−0.015*	0.014	0.008
	(0.008)	(0.009)	(0.013)
	[0.612]	[0.628]	[0.562]
Arrested for new crime	−0.026***	−0.008	−0.001
	(0.008)	(0.009)	(0.012)
	[0.597]	[0.614]	[0.599]
Panel B: Labor Market			
Employment	0.043***	0.007	0.049***
	(0.006)	(0.007)	(0.010)
	[0.286]	[0.268]	[0.333]
Earnings (100s)	140.476***	40.997**	147.904***
	(14.792)	(16.130)	(32.224)
	[637.811]	[593.468]	[786.994]
Disposable income (100s)	61.120***	7.813	29.730
	(11.954)	(14.939)	(25.531)
	[1,066.991]	[1,036.568]	[1,139.762]
Year FE	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes
Court FE	Yes	Yes	Yes
Crime FE	Yes	Yes	Yes
Sentence length	Yes	Yes	Yes
Observations	70,243	80,986	14,496

Notes: The table shows results from various robustness tests. Column (1) shrinks the bandwidth in the control group from 4–12 months to 4–6 months. Column (2) assigns placebo reform to the year prior to the actual reform and re-estimate model (2) while controlling for the current reform. Column (3) uses the sample of eligible offenders and estimates an RDiT-model where the running variable is months from the trial relative to the reform date. Robust standard errors are shown in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

Table A3: Additional Subgroup Results

	Criminal History	No Crim. History	Low Education	High Education
	(1)	(2)	(3)	(4)
Panel A: Crime				
Convicted of new crime	-0.012^{*}	0.006	-0.001	-0.021^{**}
	(0.007)	(0.016)	(0.010)	(0.011)
	[0.784]	[0.292]	[0.699]	[0.555]
Arrested for new crime	-0.012^{*}	-0.046^{***}	-0.006	-0.046^{***}
	(0.007)	(0.017)	(0.009)	(0.010)
	[0.758]	[0.302]	[0.682]	[0.542]
Panel B: Labor Market				
Employment	0.031^{***}	0.016	0.029^{***}	0.036^{***}
	(0.005)	(0.013)	(0.007)	(0.008)
	[0.168]	[0.487]	[0.220]	[0.332]
Earnings (100s)	87.422^{***}	141.019^{***}	96.050^{***}	158.106^{***}
	(11.437)	(35.574)	(15.303)	(21.416)
	[336.768]	[1,150.762]	[456.702]	[770.783]
Disposable income (100s)	36.265^{***}	116.851^{***}	48.939^{***}	90.713^{***}
	(9.954)	(26.231)	(13.702)	(16.042)
	[889.726]	[1,355.361]	[945.582]	[1,158.815]
Year FE	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Crime FE	Yes	Yes	Yes	Yes
Sentence length	Yes	Yes	Yes	Yes
Observations	55,369	25,617	36,770	38,117

Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on offender outcomes where the coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group (54,691 individuals) consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the control group (26,295 individuals) are offenders sentenced to between 4 and 12 months during the same period. The outcomes are measured as an average over the first three years after the trial. Criminal recidivism is either an indicator for having a new conviction or an indicator for having been re-arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). Robust standard errors are shown in parenthesis and sub-sample means are shown in brackets. */**/*** denote significant at the 10/5/1 percent level.

Table A4: Results by Crime Type

	Property	Violent	Drugs	DUI
	(1)	(2)	(3)	(4)
Panel A: Crime				
Convicted of new crime	−0.005	−0.026	0.027	−0.020
	(0.009)	(0.016)	(0.024)	(0.032)
	[0.885]	[0.564]	[0.772]	[0.384]
Arrested of new crime	−0.015*	−0.041***	0.026	−0.050*
	(0.009)	(0.016)	(0.023)	(0.029)
	[0.842]	[0.583]	[0.757]	[0.379]
Panel B: Labor Market				
Employment	0.014**	0.050***	0.007	0.069***
	(0.007)	(0.012)	(0.016)	(0.022)
	[0.097]	[0.319]	[0.131]	[0.450]
Earnings (100s)	26.184*	179.763***	20.927	245.134***
	(13.725)	(28.536)	(31.773)	(49.017)
	[179.823]	[699.757]	[256.205]	[1,070.671]
Disposable income (100s)	2.530	69.332***	40.021	142.128***
	(14.129)	(19.494)	(28.152)	(37.448)
	[755.654]	[1,117.278]	[849.574]	[1,336.721]
Year FE	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
Socioeconomic controls	Yes	Yes	Yes	Yes
Court FE	Yes	Yes	Yes	Yes
Crime FE	Yes	Yes	Yes	Yes
Sentence length	Yes	Yes	Yes	Yes
Observations	55,369	25,617	36,770	38,117

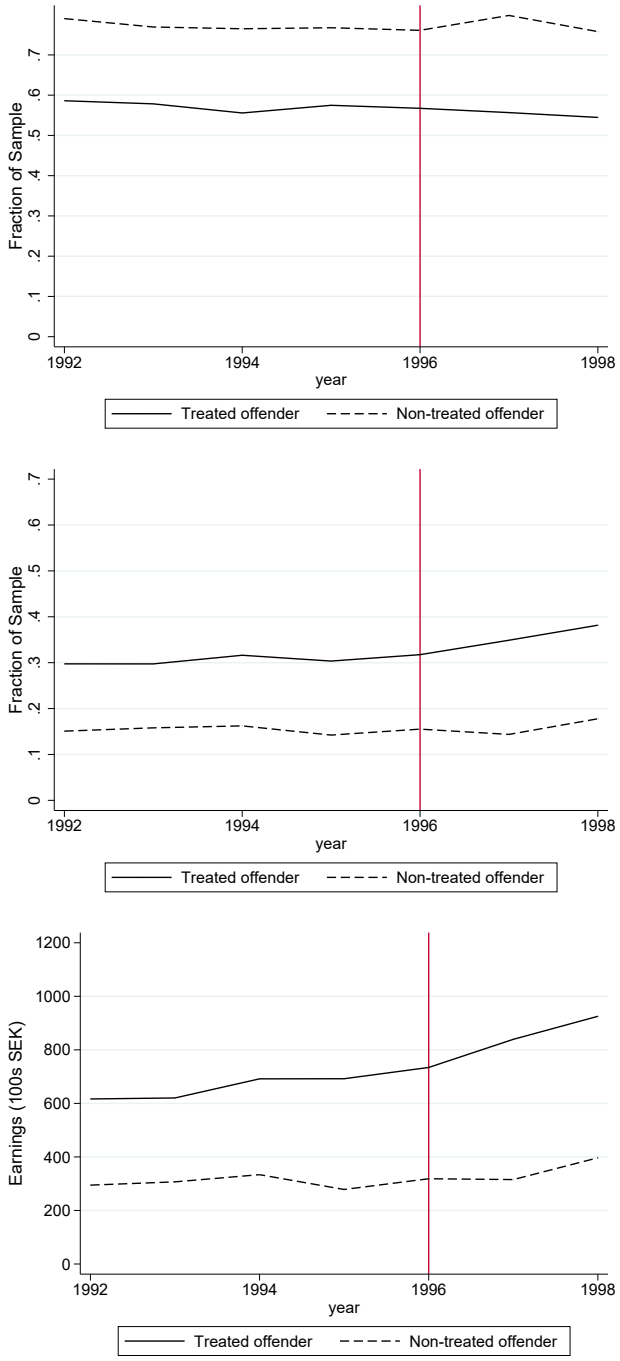
Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on offender outcomes where the coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group (54,691 individuals) consists of offenders sentenced to at most three months in prison between 1992 and 1999 and the control group (26,295 individuals) are offenders sentenced to more than 3 and up to 12 months during the same period. The outcomes are measured as an average within the first three years after the trial. Criminal recidivism is either an indicator for having a new conviction or an indicator for having been re-arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). Robust standard errors are shown in parenthesis and sub-sample means are shown in brackets. */**/*** denote significant at the 10/5/1 percent level.

Table A5: Results for Parent Offenders

	Outcome Mean (SD)	(1)	(2)	(3)	(4)	(5)
Panel A: Crime						
	0.628	−0.023	−0.014	−0.012	−0.006	−0.007
	(0.483)	(0.022)	(0.020)	(0.020)	(0.020)	(0.020)
	0.632	−0.048**	−0.040**	−0.040**	−0.035*	−0.035*
	(0.482)	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)
Panel B: Labor Market						
	0.260	0.040**	0.029**	0.025*	0.022	0.022
	(0.386)	(0.017)	(0.015)	(0.015)	(0.014)	(0.014)
	590.835	153.898***	119.449***	111.253***	106.702***	104.858***
	(1,085.752)	(48.038)	(40.244)	(40.572)	(40.285)	(40.316)
	1,130.975	130.227***	114.656***	110.181***	106.818***	106.382***
	(725.639)	(34.416)	(32.684)	(32.444)	(32.195)	(32.264)
Year FE	–	Yes	Yes	Yes	Yes	Yes
Demographic Controls	–	Yes	Yes	Yes	Yes	Yes
Socioeconomic Controls	–	–	Yes	Yes	Yes	Yes
Court FE	–	–	–	Yes	Yes	Yes
Crime FE	–	–	–	–	Yes	Yes
Sentence length	–	–	–	–	–	Yes
Observations		12,530	12,530	12,530	12,530	12,530

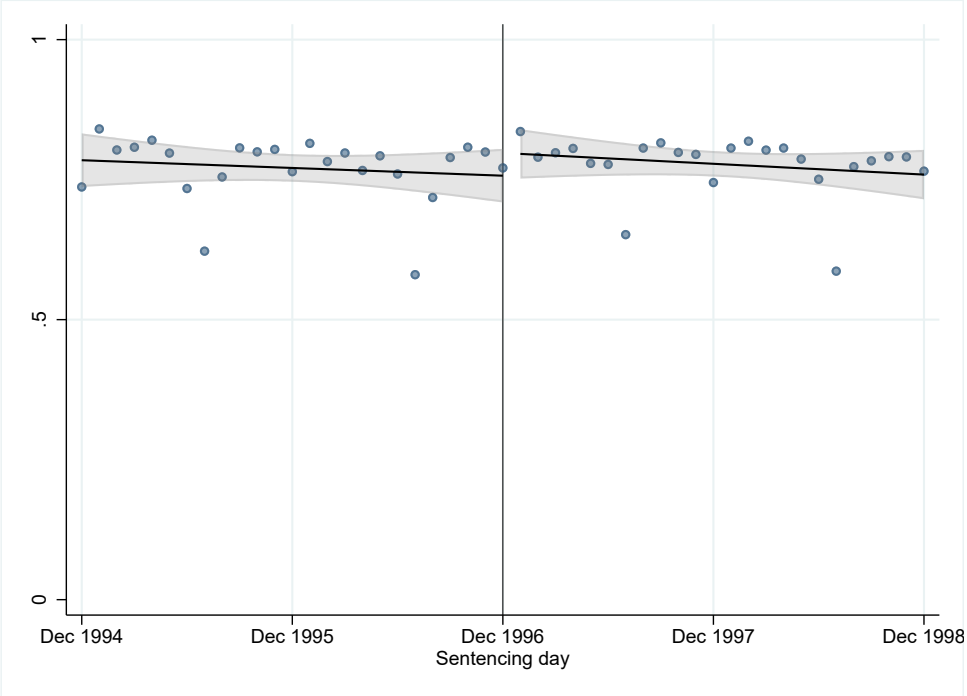
Note: This table reports OLS estimates of our DiD-model of the impact of increased access to electronic monitoring on the outcomes of the convicted parent where the coefficient displayed is on the interaction between an indicator for being an offender eligible for EM and an indicator for post-reform. The eligible group consists of partners to the offenders sentenced to at most three months in prison between 1992 and 1999 and the ineligible group are partners to the offenders sentenced to between 4 and 12 months during the same period. The outcomes are measured as an average within three years after the trial. Criminal behavior is either an indicator for having a conviction or an indicator for having been arrested. Employment is an indicator for being registered as formally employed. Earnings is defined as total annual labor earnings (SEK). Disposable income is total post-tax income from labor, capital and transfers (SEK). Standard errors clustered at the family level are shown in parenthesis. */**/*** denote significant at the 10/5/1 percent level.

Figure A1: Trends in Recidivism and Labor Market Outcomes, by Offender's Eligibility Status



Note: The figure examines the parallel trends assumption by plotting time-series of the key outcomes by treatment status of the offenders. The top panel shows the share employed; the middle panel shows the share re-convicted; the lower panel shows average earnings. Treated offenders sentenced up to three months in prison after January 1st, 1997, became eligible to electronic monitoring, whereas non-treated offenders sentenced to between 4 and 12 months remained ineligible throughout the period. All outcomes are measured as an average over three years after the trial.

Figure A2: Probability of being Sentenced to up to Three Months in Prison by Time-Distance to EM Expansion Reform of 1997



Note: The sample consists of offenders sentenced to up to 12 months in prison between 1994 and 1998. The graph plots the probability of being sentenced to up to three months in prison in our sample against a linear control in the month of the trial relative to the month of the reform. They grey shaded area is the associated 95 percent confidence interval.