Journal of Public Economics 230 (2024) 105051

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The effects of electronic monitoring on offenders and their families[☆]

Julien Grenet[a], Hans Grönqvist[b,*], Susan Niknami[c]

[a] Paris School of Economics and CNRS, France
[b] Linnaeus University and IFAU, Sweden
[c] Stockholm University, Sweden

ARTICLE INFO

JEL classification:
K42

Keywords:
Electronic monitoring
Incarceration
Labor supply
Crime
Spillovers

ABSTRACT

Electronic monitoring (EM) has emerged as a popular tool for curbing the growth of large prison populations. Evidence on the causal effects of EM on criminal recidivism is, however, limited and it is unclear how this alternative to incarceration affects the labor supply of offenders and the outcomes of their family members. We study the countrywide expansion of EM in Sweden in 1997 wherein offenders sentenced to up to three months in prison were granted the option to substitute incarceration with EM. Our difference-in-differences estimates, which compare the change in the prison inflow rate of treated offenders to that of non-treated offenders with slightly longer sentences, show that the reform significantly decreased the number of incarcerations. Our main finding is that EM not only lowers criminal recidivism but also increases labor supply. Additionally, EM improves the educational attainment and early-life earnings of the children whose parents were exposed to the reform. The primary mechanisms through which EM operates appear to involve the preservation of offenders' ties to the labor market, by reducing the barriers to both finding a job and changing employers. Our calculations suggest that the social benefits stemming from EM are about seven times larger than the fiscal savings associated with reduced prison expenditures, implying that the welfare gains from EM could be much greater than previously acknowledged.

0. Introduction

Electronic monitoring (EM) has become a pivotal instrument for countries seeking to reduce rising prison expenditure (e.g., Bartels and Martinovic, 2015).[1] While the timing of the introduction of EM varies widely across countries, with the United States and Sweden being among the early adopters, the fundamental characteristics of EM programs remain similar: an electronic device employs the global positioning system (GPS) to supervise individuals under curfew, allowing those convicted of less serious offenses to engage in rehabilitation programs and regular employment. Most available estimates suggest that the costs of EM are an order of magnitude lower than those of incarceration, mainly because fewer prison staff are needed to perform the monitoring now automated (Kyckelhahn, 2011).

In addition to mitigating the fiscal burdens associated with large-scale prison systems, there are many reasons to believe that EM offers other social benefits. Perhaps most importantly, EM could improve employment prospects by increasing the possibility for offenders to maintain their connection to the labor market. Having a job or being willing to search for one are fundamental components of many EM programs, and labor market opportunity has been found to be a strong predictor of successful rehabilitation (e.g., Freeman, 1999; Yang, 2017; De Troyer, 2020; Williams and Weatherburn, 2020). EM may also improve labor market outcomes by reducing discrimination by employers against ex-prisoners whose prison spells may either directly or indirectly be observed, e.g., through criminal background checks or employment gaps evident in their resumes (e.g., Western, Kling, and Weiman, 2001; Lofstrom and Raphael, 2016). EM could further contribute to rehabilitation by preventing the 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,

☆ We are grateful to the editor and reviewers, Matthew Lindquist, Hanna Mühlrad, Sara Roman, and seminar participants at Uppsala University, IFAU, Linnaeus University and Örebro University for useful comments. Adam Birgersson and Gabriel Nilsen provided excellent research assistance. This work has benefited from funding from Handelsbankens research foundations and the Swedish Research Council (VR). Julien Grenet acknowledges support from the Agence Nationale de la Recherche through EUR grant ANR-17-EURE-0001.

* Corresponding author.
E-mail addresses: julien.grenet@psemail.eu (J. Grenet), hans.gronqvist@lnu.se (H. Grönqvist), susan.niknami@sofi.su.se (S. Niknami).

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

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

https://doi.org/10.1016/j.jpubeco.2023.105051
Received 1 August 2022; Received in revised form 18 December 2023; Accepted 18 December 2023
Available online 29 December 2023

2016).[2] Moreover, the potential benefits of EM may extend beyond the offender alone and could positively impact the offender's family as well.

The causal effects of EM are, however, theoretically unclear, as allowing individuals to serve their sentences at home could potentially increase the risk of re-offending by making the punishment less salient (e.g., Becker, 1968; Drago et al., 2009; Chalfin and McCrary, 2017).[3] Furthermore, family members may be adversely affected by being forced to spend more time at home with the offender. Still, estimating the causal effect of EM poses a significant challenge due to the difficulties involved in isolating its impact from correlated unobservable factors (e.g., Di Tella and Schargrodsky, 2013; Henneguelle, Monnery, and Kensey, 2016; Williams and Weatherburn, 2020).[4]

In this paper, we estimate the causal effects of increased access to EM in the context of the Swedish criminal justice system. Our work leverages three key advantages offered by the Swedish setting. First, rich Swedish administrative data allow us to measure the impacts of EM across a wide spectrum of outcomes, including labor supply, and to gain insights into the potential underlying mechanisms. The second strength of our setting lies in our ability to isolate exogenous variation in access to EM. We achieve this by examining a large expansion of EM in 1997, wherein EM transitioned from being a small-scale local pilot program to a nationwide initiative. Third, the reform implied that individuals sentenced to prison for up to three months could opt to entirely circumvent incarceration. This means that the context we study involves EM serving as a complete alternative to imprisonment.

To isolate the causal effects stemming from the expansion of EM in Sweden in 1997, we use a difference-in-differences strategy that compares, before and after the reform, the outcomes of offenders who received prison sentences of up to three months (treatment group) to the outcomes of offenders sentenced to prison terms ranging from 4 to 12 months (control group). We start by showing that the reform led to a significant 30 percentage-point reduction in the incarceration rate of offenders in the treatment group in comparison to the control group, with highly similar pre-trends for both groups.[5] We also verify empirically that the length of sentences did not undergo significant changes around the time of the reform, suggesting that courts did not alter their sentencing practices in response. This finding is expected since the decision to grant EM was made independently by a separate government agency following sentencing.

We then turn to estimating the effects of increased access to EM on the offenders themselves. Our findings indicate that the reform resulted in several positive outcomes. Specifically, it significantly reduced the probability of being re-arrested within three years after the trial by 4.7 percent and lowered the three-year re-conviction rate by about 2.2 percent. It also improved labor market outcomes, with the likelihood of being employed within three years after the trial increasing by 13.1 percent and average earnings rising by 22.1 percent. Notably, these improvements are sustained beyond the first year after the trial, suggesting that the benefits of EM may have a lasting impact. These benefits are particularly pronounced for individuals who were already employed at the outset and those who had been sentenced for violent crimes (mostly assault) or driving under the influence of drugs or alcohol (DUI). Our results are robust to a battery of specification checks, including narrowing the sentence bandwidth for inclusion in the control group, adopting a difference-in-discontinuities design based on the time between the trial and the reform, and conducting placebo analyses.

We proceed by using our rich data to shed light on potential mechanisms. We investigate whether EM prevents the accumulation of criminal capital in prison by differentiating between crimes that require the development of specific skills (e.g., theft or drug dealing) and more spontaneous crimes (e.g., violent crimes or drunk driving). Our findings reveal a significant decrease in non-acquired crimes, while estimates for acquired crimes are not statistically significant. This suggests that the benefits of EM are not primarily driven by its potential to prevent individuals with criminal convictions from accumulating criminal skills behind bars. Next, we investigate the possibility that EM enhances social integration by allowing offenders to maintain their family relationships. Our results do not strongly confirm this hypothesis, as we do not observe significant effects of access to EM on the risk of divorce or separation. We then explore the possibility that EM improves offenders' outcomes by allowing them to maintain ties to the labor market. We do so by decomposing the estimated effect on employment into three mutually exclusive components: (i) remaining with the same employer; (ii) switching to a new employer; (iii) transitioning from non-employment to employment. Our results suggest that EM improves offenders' labor market prospects mainly by reducing the barriers to both finding a job and to changing employers. This finding is also consistent with the hypothesis that EM makes it more challenging for potential employers to discriminate against ex-prisoners in the hiring process, for instance by reducing gaps in their resumes.

While there appear to be large social benefits associated with the use of EM, there may also be potentially important, yet previously overlooked, spillover effects on family members. These externalities, which could either be positive or negative, must be factored in when assessing the full social welfare effects of expanding EM access. Our results show that the reform significantly increased the likelihood of offenders' children completing compulsory schooling by 3.5 percent, and significantly raised their early-life earnings (at age 25) by 25.3 percent. We do not find any significant effects on the other (non-convicted) parent. Taken together, the large improvements in labor market outcomes for offenders, combined with improvements in some of the outcomes for their children and the absence of significant adverse effects on their partner, suggest that the social benefits of EM could be far greater than what was previously acknowledged. To provide an illustrative perspective, we undertake a back-of-the-envelope calculation of the social benefits of EM by combining our results for offenders and their families. These calculations consider the benefits both from improved earnings (for the offenders and their children) and reduced crime (for the offenders only). Our analysis suggests that the social benefits of EM could be at least seven times larger than the direct fiscal savings from using EM instead of incarceration.

Our results relate to a significant body of literature on the impact of EM on criminal recidivism. Many previous studies in this field have struggled to adequately account for correlated unobservable factors (see Renzema and Mayo-Wilson, 2005, for a review). However, recent research has benefited from the use of quasi-experimental research designs. One of the most compelling pieces of evidence is the study by Di Tella and Schargrodsky (2013) in the context of Argentina. Leveraging the random assignment of detainees to judges with varying propensities 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 show that EM decreases the two-year recidivism rate by approximately 28 percent. Henneguelle et al. (2016) instrument for assignment to EM based on its local introduction in French courts. Their

[3] It is also possible that EM creates additional opportunities for criminal activity when individuals serve their sentences at home rather than in prison. For further discussion of this aspect of deterrence, see Nagin (2013) and Chalfin and McCrary (2017).

[4] For example, the criminal justice system may introduce correlated unobservable factors when allocating EM to offenders with the highest probability of success.

[5] Not everyone who receives a prison sentence ultimately serves time behind bars. One primary reason for this is that the time spent in pre-trial detention is subtracted from the overall sentence.

findings indicate that EM reduces the likelihood of re-offending within five years by about 10 percent.[6]

Our research makes three key contributions to this literature. First, we extend the scope of outcomes studied, encompassing not only recidivism but also labor market outcomes. Despite the argument that EM can help to maintain labor market ties, there is no robust empirical evidence to date regarding its impact on labor supply.[7] Second, we investigate spillover effects on family members. The possibility of important but previously neglected spillover effects from EM is suggested by recent research on the effects of parental incarceration on family members (e.g. Dobbie, Grönqvist, Niknami, Palme, and Priks, 2018b; Norris, Pecenco, and Weaver, 2021; Arteaga, 2022). However, it remains uncertain whether these findings extend to the specific category of offenders typically targeted by EM. Our work aims to document the effects of EM across a wide array of outcomes in various populations at risk of being affected, providing a comprehensive view of the welfare implications of this alternative to incarceration. Third, we contribute to this literature by shedding light on several potential mechanisms that could explain the causal effects we observe. Existing studies have been hampered by the unavailability of proper data, making it difficult to provide evidence on the underlying channels through which EM operates.

Our work complements the study by Williams and Weatherburn (2020), which was the first to investigate EM as a "front-end" alternative to incarceration, representing a true substitute for imprisonment. Previous research instead considers EM at the pre-trial phase, where bail might otherwise be employed, and for early release from prison, where parole serves as a substitute. Our work is also related to the literature on the consequences of incarceration on an individual's own life outcomes (see, e.g., Western et al., 2001, for a review). Recent studies in this field have used random assignment of cases to judges who differ in their propensity to sentence to prison in the United States (Mueller-Smith, 2015; Dobbie et al., 2018a), Sweden (Dobbie et al., 2018b), and Norway (Bhuller et al., 2020).[8] The results from this research are somewhat inconclusive. While some studies find adverse effects of incarceration (Mueller-Smith, 2015; Dobbie et al., 2018a), others identify beneficial effects, especially for individuals who were unemployed before their trial (Bhuller et al., 2020). A few studies have also investigated the effects of parental incarceration on children's outcomes, generally finding that it is associated with worse outcomes (see Wildeman, 2010, and Murray et al., 2012, for recent reviews). A handful of papers go beyond these associations to estimate the causal effects of parental incarceration on children, yielding mixed results. Using a random-judge design, Dobbie et al. (2018b) find that parental incarceration increases teen crime, reduces school performance, and has negative consequences on employment and earnings.[9] Norris et al. (2021) find that, in the United States, parental incarceration reduces teen crime, has no impact on teen parenthood, and increases the likelihood that children live in affluent neighborhoods as adults. Arteaga (2022) finds positive effects of parental incarceration in Colombia on children's educational attainment.[10] Hence the diverging results from these studies do not offer clear guidance regarding the possible effects of EM on children.[11,12]

The remainder of the paper is organized as follows. Section 1 provides an overview of the Swedish criminal justice system and describes the key elements of the EM expansion reform of 1997. Section 2 presents our data and empirical design. The results are discussed in Section 3. Section 4 presents a cost–benefit analysis and Section 5 concludes.

1. Institutional background

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

The criminal justice system in Sweden is similar to that of many other OECD countries, with the notable exception of the United States, which stands as an outlier in may dimensions. One of the most striking differences lies in the length of sentences. In the U.S., the average prison sentence spans 2.9 years (PEW, 2016), whereas in Sweden, for instance, fewer than 20 percent of prison sentences exceed one year in duration. The effective time served by prison inmates in Sweden

[6] Two unpublished papers also investigate the effect of EM on recidivism. Marie (2009) examines the impact of EM in the context of early release for offenders serving prison sentences in England and Wales and finds significant reductions in re-offending. Rivera (2023) investigates the effect of EM as an alternative to both pre-trial release and pre-trial detention in Cook County, Illinois, and shows that, in comparison to detention, EM increases low-level pre-trial misconduct but reduces future recidivism.

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

[8] In other related studies using a random-judge design, all conducted in 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. (2018a) estimate the impact of pre-trial incarceration. Other quasi-experimental studies include (Kuziemko, 2013), which takes advantage of both a mass release of inmates and discontinuities in sentencing guidelines to show that longer periods of incarceration lead to a significant reduction in the risk of re-offending. Using a similar strategy, Landersjo (2015) shows that seemingly exogenous increases in prison time improve post-release employment outcomes.

[9] Dobbie et al. (2018a) also document negative effects of incarceration on the offenders themselves, and our results align with their conclusions. Our findings concerning the impact on children are also broadly consistent with those of Dobbie et al. However, an important difference between the studies is that while we focus on relatively low-risk offenders who receive short prison sentences, Dobbie et al. consider the effects for the entire spectrum of offenders sentenced to incarceration.

[10] Bhuller et al. (2018) find no significant effects of parental incarceration on school performance and children's risk of engaging in criminal activities. Their estimates are, however, relatively imprecise.

[11] While several studies have used cross-sectional data to document a positive correlation between incarceration and the risk of marital dissolution (e.g., Apel, Arjan, Blokland, Nieuwbeerta, and Van Schellen, 2010), this finding has only been verified in a quasi-experimental context by Dobbie et al. (2018a). We are not aware of studies examining the effects of incarceration on spousal outcomes.

[12] More loosely, our paper also contributes to the extensive body of literature examining the outcomes of more intensive community supervision. In the United States alone, approximately 4.8 million offenders are subject to various community supervision programs (Georgiou, 2014). Although some elements of these programs resemble aspects of electronic monitoring (e.g., home visits and drug screening), a key distinction lies in the timing of implementation, with community supervision typically occurring at the end of a prison sentence (i.e., "back-end"). In general, the findings in this literature indicate that more intense supervision is not associated with an increased risk of criminal recidivism. For instance, Boyle, Ragusa-Salerno, Lanterman, and Marcus (2013) compare the recidivism rates among parolees in New Jersey who were assigned to day reporting centers versus those assigned to traditional supervision programs. The results show that participants in both groups exhibit similar probabilities of failing to meet their parole conditions. Similarly, Georgiou (2014) finds no effect of a program in Washington State, which assigned varying levels of supervision intensity based on a risk assessment instrument, on re-offending rates. Finally, Barnes et al. (2012) investigate the effects of reduced supervision in the Philadelphia Low-Intensity Community Supervision Experiment, finding no evidence that this program increased the risk of re-offending.

is also considerably less than the officially recorded sentences, as nearly all prisoners receive probation after serving two-thirds of their sentence, barring exceptional circumstances.[13] Sentencing guidelines provide judges with a relatively large degree of discretion in determining sentences.[14] In practice, however, the distribution of sentences often falls within the lower spectrums, with the majority of sentences clustered toward the lower end of the sentencing range.[15]

Large prison populations and costly rehabilitation programs have increased prison expenditures in most countries (Penal Reform International / Thailand Institute of Justice (PRI/TIJ), 2020). The potential for cost reduction has been a central argument in support of implementing EM. Since the introduction of EM in the United States in the 1990s, most OECD countries have adopted it extensively in various formats. One of the most common forms is the complete substitution of a short prison term with EM, commonly referred to as "front door" electronic monitoring.

Radiofrequency EM was introduced in Sweden in 1994 as a means of making "home detention" secure and enforceable. This introduction was part of a small-scale experimental scheme that was initially planned to span 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 designed for offenders aged 18 and above who had received prison sentences of no more than two months. Early assessments of this pilot scheme suggested that, although the limited number of participants made it challenging to draw robust conclusions regarding its impact on criminal recidivism, it was cost-effective due to the fiscal savings it generated. On January 1st, 1997, EM was expanded to all probation districts, and the maximum duration of eligible prison sentences was extended from two to three months (Brottsförebyggande Rådet (BRÅ), 1999).

Unlike in most countries, but similar to many U.S. states, EM in Sweden is not imposed by the court as a mandatory measure. Instead, offenders are required to apply to the Swedish Prison and Probation Service (*Kriminalvården*) if they wish to serve their prison sentence under EM. Following the court's verdict, all individuals who receive prison sentences of up to three months are informed by the Prison and Probation Service about the option to apply to EM and are provided with the necessary application form. When the EM reform was implemented in 1997, the average waiting time between the trial and receiving this information was two months. The Prison and Probation Service then took one month to review the application. During this application and review period, sentenced offenders did not start their prison term. In the initial two years of the reform, approximately 75 percent of offenders who received prison sentences of up to three months applied for the EM program (Brottsförebyggande Rådet (BRÅ), 1999), of which 87 percent were granted approval.[16] Among those approved, 99 percent initiated EM and, out of these, 94 percent successfully completed the program. Similar to procedures in other countries, eligibility for EM in Sweden is contingent on several conditions, including having suitable accommodation, stable employment or a commitment to seek and obtain employment, willingness to undergo alcohol and drug testing, and the acceptance of home visits from the Prison and Probation Service. There is no specific offense that automatically disqualifies an offender from participation, nor does prior criminal history serve as a disqualifying factor. However, offenders living in the same locality as their victims, particularly in cases involving domestic violence, are typically ineligible for participation. Consent from all members of the offender's household is required, and employers are informed about the EM arrangement. Occasionally, representatives from the Prison and Probation Service may visit the workplace to verify the individual's employment status. The only direct cost for offenders to participate is a small fee (5 USD per day), which contributes to the National Crime Victim Fund (*Brottsofferfonden*). The Prison and Probation Service has the discretion to waive this fee if it determines it to be appropriate based on the client's financial circumstances. Approximately half of the clients received fee waivers in such cases.[17]

Similarly to most other countries, the use of EM in Sweden is coupled with a 24/7 curfew, with authorized leave only permitted for specific pre-approved activities such as work, studies, and participation in treatment programs (see Bartels and Martinovic, 2015).[18] Non-compliance with either the curfew or the other EM rules carries the risk of having to serve the remainder of the sentence in a prison setting. Approximately 6 percent of offenders placed on EM in 1997 experienced this outcome (Brottsförebyggande Rådet (BRÅ), 1999). The most common reason for terminating EM was the failure to comply with the prohibition on alcohol and drugs. During the first year of the reform, participants worked an average of 31.5 h per week and devoted 2.9 h to various addiction treatment programs provided by the Prison and Probation Service (e.g., alcohol and drug treatment).

Fig. 1 provides a summary of key changes in the criminal justice system in Sweden during the relevant period, along with the trends in actual incarcerations and EM participation.[19] The figure shows that the EM pilot scheme, implemented in a limited number of probation centers on August 1st, 1994, involved only a small number of participants. The generalization of EM to the whole of Sweden on January 1st, 1997, for prison sentences of up to three months, resulted in a large and rapid increase in the annual number of EM participants, rising from a few hundred in 1996 to nearly 4000 in 1998. Concurrently, there was a significant decrease in the annual number of incarcerations, declining from approximately 13,000 to about 9000. On April 1st, 2005, EM

[13] In Sweden, individuals arrested for a crime that carries a potential prison sentence of one year or more can be held in custody before their trial if there is a concern that they might evade prosecution, obstruct the investigation, or commit another offense. For those individuals who are subsequently sentenced to prison, the time spent in pre-trial detention is deducted from their overall sentence. The Swedish criminal justice system does not employ plea bargaining, which means that defendants cannot plead guilty in exchange for a reduced sentence.

[14] For instance, the sentencing guidelines outline the following ranges of prison sentences for various types of crime: up to 2 years of imprisonment for assault, 1 to 10 years of imprisonment for aggravated assault, up to 2 years of imprisonment for theft, 0.5 to 6 years of imprisonment for aggravated theft, fines or imprisonment up to 0.5 year for DUI, imprisonment for up to 2 years for aggravated DUI, imprisonment for up to 3 years for drug-related offenses, and a range of 2 to 10 years of imprisonment for aggravated drug-related offenses.

[15] Judges in Sweden are allowed to impose sentences that fall below the minimum threshold in cases where mitigating circumstances exist. These circumstances may include self-defense, provocation prompting the commission of the offense, or situations in which the offender, due to mental illness or other factors, lacks the capacity to comprehend the consequences of their actions.

[16] Brottsförebyggande Rådet (BRÅ) (1999) reports that approximately two-thirds of those denied access to EM were offenders who either lacked suitable housing or employment, or were in pre-trial detention for other criminal offenses. The remaining one-third were denied access for "other reasons".

[17] In contrast to some other countries, individuals incarcerated in Sweden are not subjected to any fees during their time in prison.

[18] Offenders placed on EM participate in treatment programs tailored to their identified needs as assessed by the Prison and Probation Service. These treatment programs, designed to address issues related to violence, domestic violence, sexual offenses, and addiction, closely resemble those offered in prison. They are overseen by caseworkers who are employed and trained by the Prison and Probation Service. In 2022, participation rates in these programs were relatively low, with only 19 percent of prison inmates and 11 percent of offenders serving non-custodial sentences taking part in such programs.

[19] This figure is based on publicly available data provided by the Swedish Prison and Probation Service (*Kriminalvården*) and retrieved from the annual publications *Rättsstatistisk Årsbok 1985–1992* (Statistiska Centralbyrån (SCB), 1985–1992) and *Kriminalstatistisk 1993–2015* (Brottsförebyggande Rådet (BRÅ), 1993–2015).

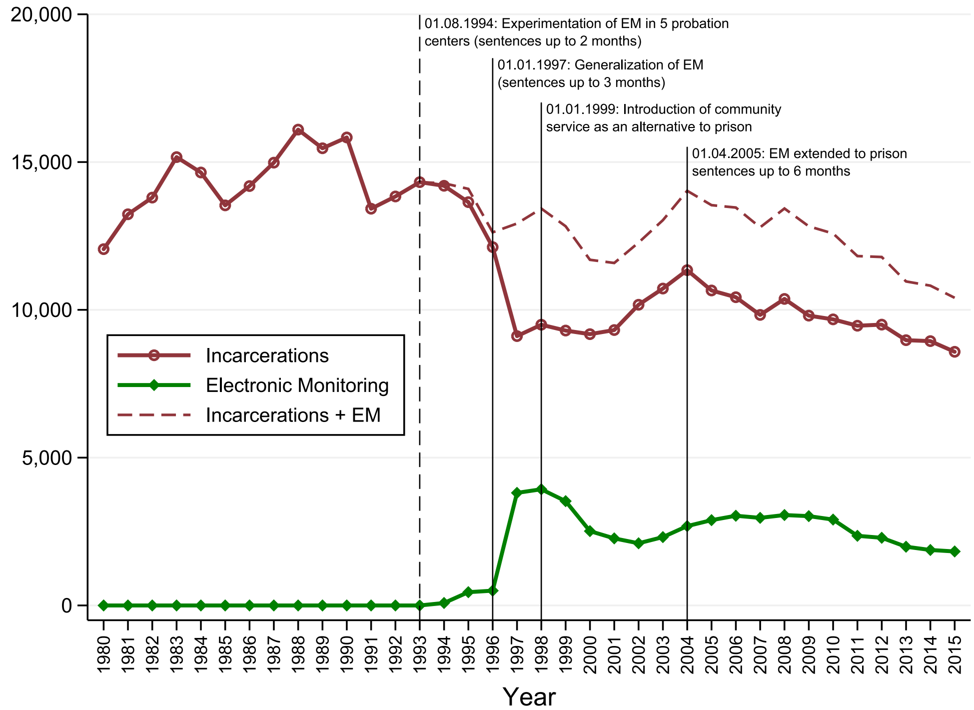

Fig. 1. Incarceration and Electronic Monitoring in Sweden, 1980–2015.
Notes: This figure is based on publicly available data provided by the Swedish Prison and Probation Service (*Kriminalvården*), retrieved from the annual publications *Rättsstatistisk Årsbok 1985–1992* (Statistiska Centralbyrån (SCB), 1985–1992) and *Kriminalstatistisk 1993–2015* (Brottsförebyggande Rådet (BRÅ), 1993–2015). It shows the number of individuals incarcerated and the number of individuals placed on EM from 1980 to 2015.

was further extended to include prison sentences of up to 6 months. This extension was followed by a minor increase in the number of individuals placed on EM, which occurred alongside a slight decrease in overall incarcerations.

Our empirical analysis is centered on the large reform implemented in 1997. However, on January 1st, 1999, the government introduced a new type of sentence that combined probation with a requirement for offenders to engage in community service activities without compensation, such as elderly care or gardening. Community service was intended to be an alternative to prison for low-risk offenders. This group of offenders clearly overlaps with the primary target group for EM, as evidenced by the decrease in the annual number of EM participants following this reform (see Fig. 1). To prevent the two reforms from being conflated in our analysis, we focus on offenders who were sentenced prior to January 1st, 1999.[20] The observed drop in the number of incarcerations in 1991 coincides with a reform that reduced the proportion of prison sentences for drunk drivers. As a result, the share of aggravated drunk drivers sentenced to prison declined from about 70 percent the year before the reform to 42 percent afterward. Given that a relatively large share of offenders granted EM are sentenced for drunk driving (as detailed below), we choose to start our analysis from the year 1992.

Fig. 2 shows the breakdown of the total number of admissions to EM by principal offense, grouped into broad categories. The overwhelming majority of individuals placed on EM (between 70 and 80 percent) were convicted for either aggravated DUI or violent crimes. DUI alone accounts for between 50 and 60 percent of EM admissions. Almost all individuals placed on EM for DUI were convicted of aggravated drunk driving rather than simple drunk driving. The second largest group of offenders subject to EM, making up approximately 20 to 25 percent of all EM admissions, consists of those sentenced to prison for violent crimes, which consist mostly of non-aggravated assaults.[21] EM is relatively less common for property crime and drug-related offenses (less than 10 percent of EM admissions).

2. Data and research design

2.1. Data

Our empirical analyses rely on micro data that originate from various administrative registers managed by Statistics Sweden. These registers contain information on the entire Swedish population aged 15 and above, spanning the years from 1990 to 2016. These data have been linked to the Swedish Conviction Register and the Crime Suspicion Register, both maintained by the National Council for Crime Prevention (*Brottsförebyggande rådet* - BRÅ). Within these records, we have access to comprehensive details concerning criminal convictions during this period. The data include information on the type of crime, the date of the crime, as well as the court-imposed sentence. This information pertains to convictions in Swedish district courts, which are the primary courts of first instance. A single conviction may encompass multiple crimes, and we observe all crimes within a given conviction. The conviction data exclude minor offenses such as speeding tickets but include offenses such as driving without a license and DUI. The Crime Suspicion Register provides information about individuals who are regarded as likely suspects following a criminal investigation conducted by the police or prosecutor. We refer to these individuals as having been "arrested", as this corresponds most closely to the terminology used in many other countries.

We use the conviction register to identify individuals aged 18–59 at the time of their trial, who were sentenced to prison between 1992

[20] On January 1st, 1999, there was also another reform that raised the requirement for inmates to serve a larger portion of their prison sentence, increasing it from one-half to two-thirds of the sentence length.

[21] Among offenders who were convicted for violent crimes and placed on EM, less than 2 percent were convicted for aggravated assault.

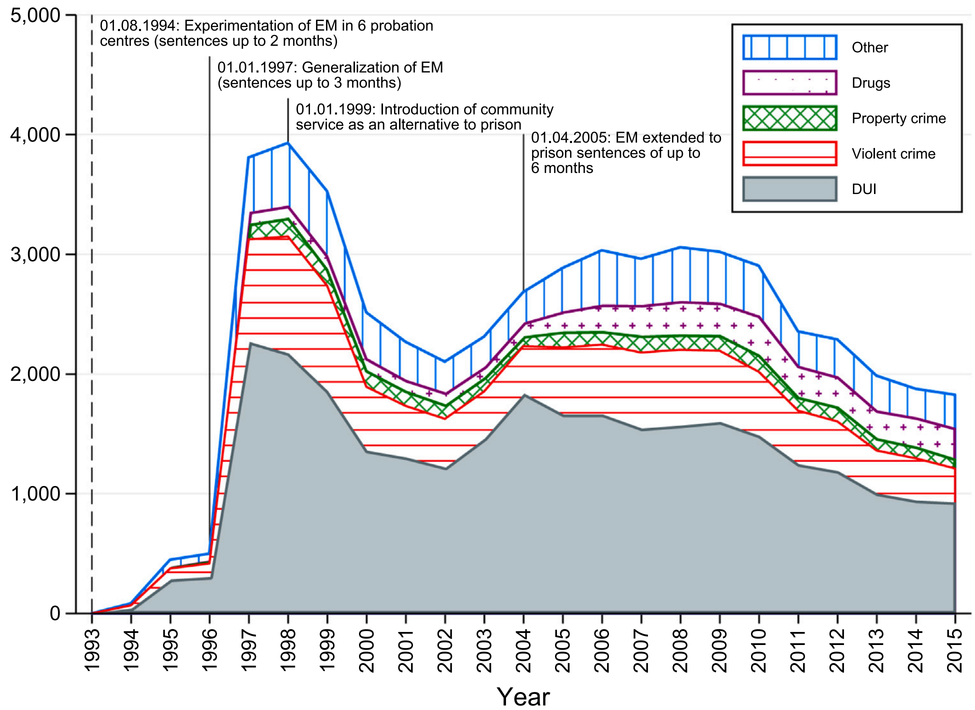

Fig. 2. Admissions to Electronic Monitoring by Main offense, 1993–2015.
Notes: This figure is based on publicly available data provided by the Swedish Prison and Probation Service (*Kriminalvården*), retrieved from the annual publication *Kriminalstatistisk 1993–2015* (Brottsförebyggande Rådet (BRÅ), 1993–2015). It shows the breakdown of the total number of admissions to EM by main offense from 1993 to 2015.

and 1998. Within the registers, we are able to extract details regarding the length of their sentences and construct measures of recidivism, including both re-convictions and re-arrests. To enrich our dataset, we include additional information pertaining to family relationships, employment status, earnings, marital status, and the criminal convictions of the children. We also have data on the children's final grade point average (GPA) in compulsory school and their educational attainment.

Table 1 provides summary statistics for two distinct groups of offenders: those who received a prison sentence of up to three months, which we use as the treatment group in our analyses since these individuals became eligible for EM in 1997, and those who received a prison sentence of between 4 and 12 months, which we use as the control group since they were not eligible for EM throughout the study period.[22] The differences in average demographic characteristics and educational attainment between the two groups are generally small. However, individuals in the treatment group have more favorable baseline labor market characteristics, reflecting the fact that offenders who receive shorter prison sentences tend to have stronger ties to the labor market. Offenders in the treatment group are also less likely to have been convicted one year earlier, which is expected since they were convicted for less severe types of crimes compared to those who received longer sentences. When looking at offender outcomes measured within three years following the trial, it is clear that they are more favorable for the treatment group.[23] For instance, 32.2 percent of the offenders in the treatment group are employed within three years after the trial, whereas the corresponding figure for the control group is 15.5 percent. These differences, however, may be influenced by a multitude of factors and hence cannot be given a causal interpretation.

2.2. Empirical strategy

To estimate the causal effects of the EM expansion reform, we employ a difference-in-differences research design. In this approach, we compare the outcomes of offenders in the treatment and control groups before and after the implementation of the 1997 reform. We restrict our sample to individuals who received prison sentences of at most 12 months. In our baseline specification, we include sentence length fixed effects, measured in months. This adjustment is made to account for the possibility that offenders sentenced to longer prison terms, even within our already narrow sentence length range, might exhibit differences in unobservable characteristics that could be potentially correlated with the outcomes of interest.[24] We estimate the following model:

$$Y_{i,l,s} = \alpha + \beta \cdot \mathbb{1}\{l \leq 3\} \cdot \mathbb{1}\{s \geq 1997\} + \gamma \mathbb{1}\{s \geq 1997\} + \delta_l + \theta_s + \boldsymbol{X}_i \mu + \epsilon_{i,l,s}, \quad (1)$$

where $Y_{i,l,s}$ denotes the outcome of offender i who was sentenced to l months in prison in year s, $\mathbb{1}\{s \geq 1997\}$ is an indicator for the prison sentence being imposed after the 1997 reform, $\mathbb{1}\{l \leq 3\}$ is an indicator for the prison sentence being of up to three months, and δ_l are sentence

[22] Although we use a wider interval for the control group to enhance statistical precision, our results are robust to using a 6-month upper limit for the prison sentence length (see Section 3.2).

[23] We use a three-year window to mitigate the risk that, when performing difference-in-differences comparisons over extended time spans, the estimates could be confounded by even modest differential trends between the treatment and control groups. Additionally, a three-year follow-up period aligns with the standard timeframe used by the Swedish National Council for Crime Prevention when reporting recidivism statistics.

[24] We are unable to estimate a standard regression discontinuity model, in which we would compare the outcomes of offenders just above and just below the three-month prison sentence cutoff. The reason is that, with only a few exceptions, courts typically sentence individuals to integer numbers of months. As a result, the majority of prison sentences in our sample are clustered at a limited number of discrete values, notably at one, two, and three months. However, for the sake of robustness, we complement our research design with an alternative difference-in-discontinuity strategy (see Section 3.2). The findings from this approach support our main conclusions.

Table 1
Summary statistics of the offenders, by treatment status.

	Treatment group	Control group
	Prison sentence: 1–3 months (1)	Prison sentence: 4–12 months (2)
Panel A. Offender characteristics and baseline outcomes		
Male	0.940	0.949
Native born	0.782	0.772
Age at trial	34.35	33.30
Less than high school degree	0.470	0.535
High school degree	0.483	0.436
More than high school degree	0.046	0.030
Employment in year before trial	0.340	0.176
Earnings (100s SEK) in year before trial	676.9	332.8
Criminal conviction in year before trial	0.410	0.598
Panel B. Type of crime		
Property crime	0.187	0.415
Violent crime	0.214	0.219
DUI	0.348	0.032
Drugs	0.051	0.107
Other	0.200	0.227
Panel C. Offender outcomes over three years after trial		
Arrested for new crime	0.552	0.741
	(0.497)	(0.438)
Convicted of new crime	0.567	0.757
	(0.496)	(0.429)
Employment	0.322	0.155
	(0.411)	(0.303)
Log earnings (100s SEK)	3.188	1.772
	(3.183)	(2.558)
Log disposable income (100s SEK)	6.563	6.124
	(1.348)	(1.525)
N	54,691	26,295

Notes: The table shows the mean values and standard deviations (in parentheses) of the variables used in the analysis. The treatment group consists of offenders who received prison sentences of up to three months between 1992 and 1998, while the control group consists of individuals sentenced to prison terms ranging from 4 to 12 months during the same period. The baseline variables listed in Panel A are measured one year before the initial trial. The distribution of the main offense associated with the prison sentence is shown in Panel B. The offender outcomes in Panel C are averaged over the three-year period following the trial. "Employment" is an indicator for being registered as formally employed. "Earnings" is total (log) annual labor earnings (in SEK). "Disposable income" is total (log) post-tax income from labor, capital, and transfers (in SEK).

length fixed effects.[25] The sentencing year fixed effects, denoted by θ_s, control for nationwide changes that impact all offenders in a similar fashion. The vector $\boldsymbol{X}_i$ includes controls for pre-determined individual characteristics (year of birth, gender, immigrant status, educational attainment, and pre-reform earnings and employment). Additionally, we control for court and crime type fixed effects. Under the common trend assumption, the coefficient β recovers the intention-to-treat (ITT) effect of increased access to EM on the outcomes of offenders who received prison sentences of up to three months.[26]

2.3. Validating the research design

The key identifying assumption underlying our difference-in-differences strategy is that, in the absence of the EM reform, the differences in outcomes between the treatment and control groups would have remained constant over time. We indirectly assess the validity of this assumption by plotting the difference-in-differences estimates for each year before and after the reform. The results of this analysis are presented in Figure A1 in the Appendix. They lend support to the common trend assumption as they indicate no significant differences in the pre-reform trends.

Another potential concern is that the Swedish courts might have adjusted their sentencing practices in response to the reform, possibly by manipulating sentence lengths to ensure that certain offenders would not avoid a prison term. While this concern is partially mitigated by the fact that the decision to assign EM was detached from the courts and instead managed by the Prison and Probation Service post-sentencing, it does not completely eliminate this risk. To explore this issue, we first leverage the idea that any alteration in sentencing practices should manifest in our sample as a change in the proportion of offenders sentenced to up to three months in prison. Figure A2 in the Appendix plots the (residualized) probability of receiving a prison sentence of up to three months within our sample against the month of the trial relative to the month of the reform, using a 24-month window.[27] The shaded gray area represents the associated 95 percent confidence interval. Reassuringly for our identification strategy, the estimated shift in the probability of being sentenced to up to three months at the reform cutoff is minimal (1.9 percentage points from

[25] Note that controlling for sentence length fixed effects precludes the need to control for the treatment group indicator $\mathbb{1}\{l \leq 3\}$. The inclusion of sentence-length fixed effects provides a flexible means of accounting for unobserved heterogeneity among individuals who are further away from the reform cutoff.

[26] Since the reform affected everyone at the same time and remained in effect, the concerns typically associated with staggered difference-in-differences research designs (e.g., Roth et al., 2023) do not apply to the current setting.

[27] To account for the apparent seasonal patterns that we observe in sentencing decisions, we first regress the indicator for receiving a prison sentence of up to three months on sentencing (calendar) month and crime type fixed effects, before plotting the residuals.

a baseline of 67.5 percent) and statistically insignificant. As a second validity test, we investigate whether the reform coincided with a shift in the composition of crimes leading to prison sentences eligible for EM. Such a shift might occur if the reform prompted adjustments in the severity of sanctions imposed for specific types of crimes within the criminal justice system. Figure A3 in the Appendix presents evidence that, when examining individuals who received a prison sentence of up to 12 months and categorizing them by the type of crime, there is no noticeable discontinuity in the probability of receiving a sentence of up to three months at the reform cutoff. Moreover, none of the pre- vs. post-reform differences are statistically significant.[28]

3. Results

This section presents the main results from our analysis of the effects of the EM reform. Using aggregate data, we start by investigating how the reform influenced incarceration rates. We then examine its effects on the outcomes of the offenders themselves. After probing the robustness of our findings, we explore treatment effect heterogeneity and take advantage of the richness of the data to gain insights into the underlying mechanisms. Finally, we assess the consequences of EM on the children of the offenders and on the other non-convicted parent.

3.1. Impact of the EM reform on incarcerations

Before proceeding to the results for the offenders, we examine the impact of the EM reform on incarcerations. As our individual-level data only provide information on prison sentences rather than actual incarcerations, we use aggregated data on incarcerations from the annual publications *Rättsstatistisk Årsbok 1985–1992* (Statistiska Centralbyrån (SCB), 1985–1992) and *Kriminalstatistisk Årsbok 1993–2015* (Brottsförebyggande Rådet (BRÅ), 1993–2015). These publications include information on incarcerations described by sentence length. In Panel A of Fig. 3, we break down the total number of incarcerations across the observation period into our treatment and control groups, based on the length of prison sentences (up to three months versus between 4 and 12 months).[29] The figure clearly illustrates that eligibility for EM was contingent on the length of the prison sentence. As expected, individuals placed on EM are exclusively found among those sentenced to prison for up to three months. In this group, the number of incarcerations dropped sharply when the reform was implemented in 1997. Conversely, when examining offenders sentenced to between 4 and 12 months in prison, there is no apparent change in the number of incarcerations in 1997. Importantly, pre-reform trends are remarkably similar in both groups.

Panel A of Fig. 3 also shows that the 2005 extension of EM to prison sentences of up to 6 months (as opposed to the previous limit of three months) was associated with a small decrease in the number of incarcerations for individuals with prison sentences ranging from 4 to 12 months. As expected, there is no discernible change in the incarceration rate for offenders sentenced to up to three months. Given the relatively modest scale of the 2005 reform and its coexistence with the reform that introduced community service as an alternative to EM in 1999 (see Section 1), we do not provide a separate analysis of this former reform and focus instead of the 1997 reform.

Panel B of Fig. 3 plots the difference in annual incarceration rates between the two groups of offenders: those sentenced to up to three months and those sentenced to between 4 and 12 months. These difference-in-differences estimates are obtained from a regression model similar to the model described by Eq. (1), except for the fact that aggregated data is used instead of individual-level data.[30] Consistent with the common trend assumption, the coefficients for the pre-reform years are close to zero and, in most cases, are not statistically significant. The implementation of the reform in 1997 coincided with a sharp and significant drop in the incarceration rate of individuals who received prison sentences of up to three months. The point estimates suggest that this rate fell by approximately 30 percentage points in 1997 and remained at a similar level in 1998. As expected, the subsequent reform of 1999, which introduced community service, led to a slight increase in the incarceration rate for the treatment group. All post-reform estimates are highly statistically significant.

3.2. Effects on offenders

Main results. Table 2 presents our difference-in-differences estimates of the impact of the EM reform on offender outcomes. In column (1), we report the means and standard deviations of the outcomes of interest, while the coefficients in columns (2) to (5) are on the interaction between the treatment group indicator and the post-reform indicator in Eq. (1). All outcomes are measured as averages (by offender) over the first three years following the trial. Criminal recidivism is measured by an indicator for having been arrested and an indicator for having a new conviction. Employment is an indicator for being registered as formally employed. Earnings are defined as total (log) annual labor earnings. Disposable income is total (log) post-tax income from labor, capital, and transfers. Column (2) shows the results from a baseline model that only controls for demographic characteristics, sentencing year, and sentence length fixed effects. The subsequent columns (3) through (5) incrementally introduce additional control variables, ending with our preferred

[28] These comparisons are performed by regressing, separately for each type of crime, a dummy that takes the value one if the offender received a prison sentence of up to three months on a dummy for the sentence being imposed after the reform.

[29] The reason why the incarceration rate for individuals sentenced to less than three months in prison was already below 100 percent before the EM reform is that those who had been detained before the trial were allowed to subtract the length of their pre-trial detention from their prison sentence. In Sweden, the average pre-trial detention period is approximately two months (Brottsförebyggande Rådet (BRÅ), 2017). This accounts for why approximately 20 percent of individuals who receive short prison sentences of up to three months are not incarcerated following their sentence. Note also that while pre-trial detention does not automatically disqualify individuals from EM, those placed in pre-trial detention for reasons unrelated to the current offense are not eligible for EM under Swedish penal code 1994:451. Another contributing factor for the incarceration rate being below 100 percent is that offenders sentenced to prison in district courts may subsequently be acquitted in the appeal court.

[30] The regression model is specified as follows:

$$Incarceration_{i,l,s} = \alpha + \sum_{\substack{k=1991\\k\neq1996}}^{2004} \beta_k \cdot \mathbb{1}\{l \le 3\} \cdot \mathbb{1}\{s = k\} + \gamma \cdot \mathbb{1}\{l \le 3\} + \theta_s + \epsilon_{i,l,s},$$

where $Incarceration_{i,l,s}$ is an indicator for whether individual i, sentenced to a prison sentence of l months in year s, was incarcerated, $\mathbb{1}\{l \le 3\}$ is an indicator for whether the individual received a prison sentence of up to three months, and θ_s are fixed effects for the year of sentencing. The individual-level data are reconstructed from the aggregated data published in the *Rättsstatistisk Årsbok 1985–1992* (Statistiska Centralbyrån (SCB), 1985–1992) and *Kriminalstatistisk Årsbok 1993–2015* (Brottsförebyggande Rådet (BRÅ), 1993–2015) yearbooks. The coefficients β_k on the interaction terms are normalized to zero in 1996 so that the difference in incarceration rates between individuals with prison sentences of up to three months vs. of 4–12 months are measured relatively to the last pre-reform year. 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 ones if some individuals sentenced in a given year were not incarcerated in the same year.

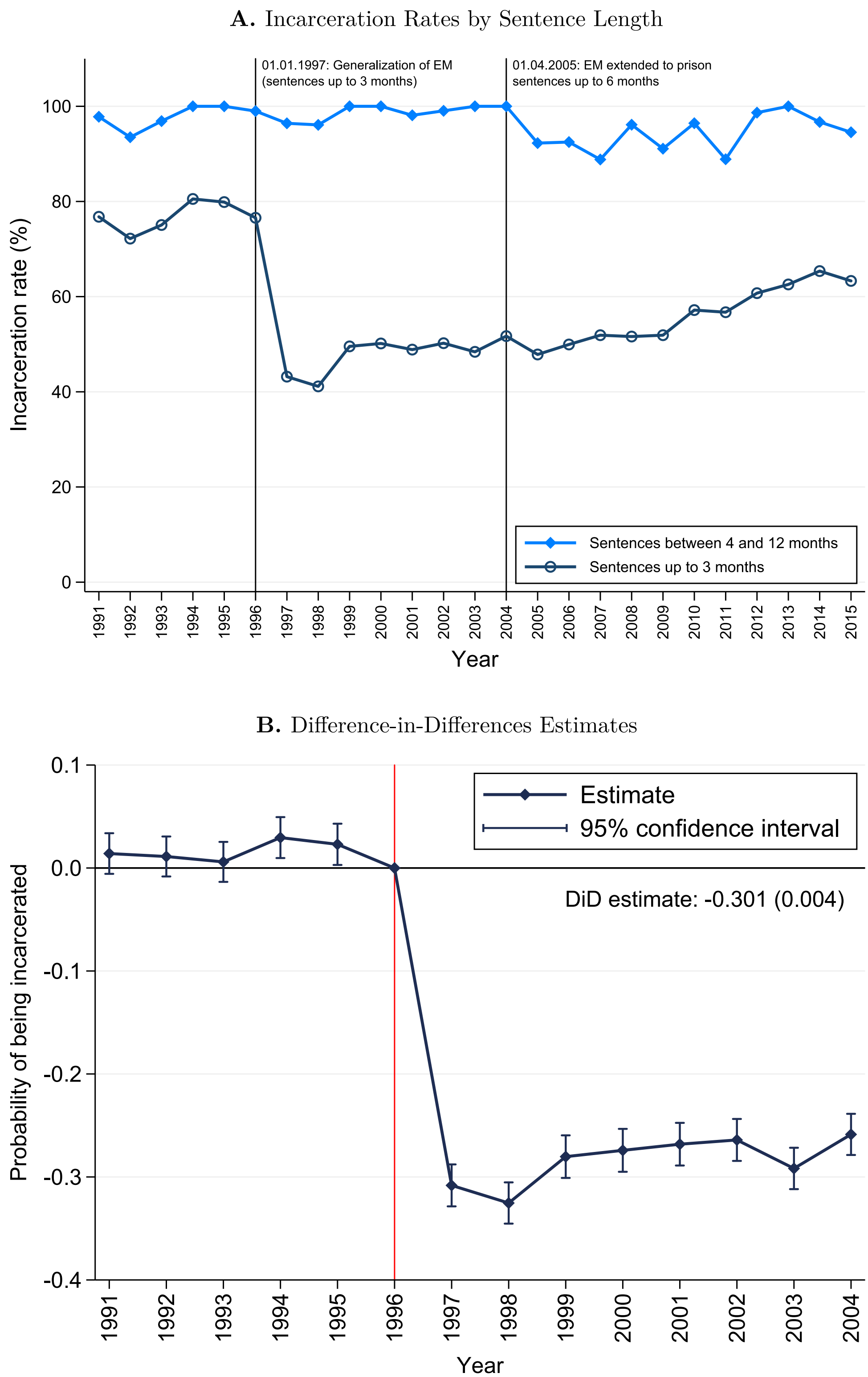

Fig. 3. Incarceration Rates for Individuals with Prison Sentences of up to 12 Months.
Notes: This figure is based on publicly available data provided by the Swedish Prison and Probation Service (*Kriminalvården*), retrieved from the annual publications *Rättsstatistisk Å rsbok 1985–1992* (Statistiska Centralbyrån (SCB), 1985–1992) and *Kriminalstatistisk 1993–2015* (Brottsförebyggande Rådet (BRÅ), 1993–2015). The incarceration rates are less than 100 percent because any potential pre-trial detention is subtracted from the sentence. Panel A shows the raw trends. Panel B shows the difference in the annual incarceration rate between offenders sentenced to up to three months in prison relative to offenders sentenced to between 4 and 12 months. These difference-in-differences estimates are relative to 1996, which is the last year before the reform.

Table 2
Effects of the expansion of electronic monitoring in 1997 on offenders' criminal recidivism and labor market outcomes: Main results.

	Outcome mean	Difference-in-differences estimates			
	(1)	(2)	(3)	(4)	(5)
Panel A. Crime					
Arrested for new crime	0.614	−0.038***	−0.030***	-−0.031***	−0.029***
	(0.487)	(0.007)	(0.007)	(0.007)	(0.007)
Convicted of new crime	0.628	−0.023***	−0.015**	−0.016**	−0.014**
	(0.483)	(0.007)	(0.007)	(0.007)	(0.007)
Panel B. Labor market outcomes					
Employment	0.268	0.044***	0.037***	0.036***	0.035***
	(0.387)	(0.006)	(0.005)	(0.005)	(0.005)
Log earnings (100s SEK)	2.729	0.300***	0.242***	0.236***	0.221***
	(3.067)	(0.048)	(0.042)	(0.042)	(0.041)
Log disposable income (100s SEK)	6.420	0.066**	0.063**	0.070***	0.068***
	(1.423)	(0.026)	(0.025)	(0.025)	(0.025)
Controls					
Sentencing year FEs	–	Yes	Yes	Yes	Yes
Sentence length FEs	–	Yes	Yes	Yes	Yes
Demographic characteristics	–	Yes	Yes	Yes	Yes
Socioeconomic characteristics	–	–	Yes	Yes	Yes
Court FEs	–	–	–	Yes	Yes
Crime type FEs	–	–	–	–	Yes
N	80,986	80,986	80,986	80,986	80,986

Notes: This table reports OLS estimates for the difference-in-differences (DiD) model that we use to assess the impact of expanded access to electronic monitoring on offender outcomes. Column (1) reports the means and standard deviations of the outcomes of interest. Columns (2) to (5) present the coefficients on the interaction between the treatment group indicator and an indicator for the post-reform period. The treatment group (54,691 individuals) consists of offenders sentenced to prison for up to three months between 1992 and 1998, while the control group (26,295 individuals) consists of offenders sentenced to prison terms ranging from 4 to 12 months during the same period. All outcomes are measured as averages over the three years following the trial. Criminal recidivism is measured by an indicator for having been re-arrested and an indicator for having a new conviction. "Employment" is an indicator for being registered as formally employed. "Earnings" is total (log) annual labor earnings (in SEK). "Disposable income" is total (log) post-tax income from labor, capital, and transfers (in SEK). The estimates in column (2) are from a model that controls for sentencing year fixed effects, sentence length fixed effects (in months), and pre-determined demographic characteristics (year of birth, gender, immigrant status). Column (3) additionally controls for socioeconomic characteristics (educational attainment, earnings and employment in the year before the trial). Column (4) expands the set of controls to include court fixed effects. Column (5) further controls for crime type fixed effects. Robust standard errors are reported in parentheses. */**/*** denote significant at the 10/5/1 percent level.

specification in column (5), which accounts for demographics, predetermined socioeconomic characteristics, sentencing year, sentence length, court, and crime type fixed effects.

Consistent with previous work (Di Tella and Schargrodsky, 2013; Henneguelle et al., 2016; Williams and Weatherburn, 2020), the estimates in Panel A show that the EM reform led to a significant reduction in re-offending rates. For instance, based on our preferred specification in column (5), offenders in the treatment group are 1.4 percentage point less likely to be re-convicted during the first three years after the trial compared to the control group, representing a 2.2 percent decrease from the baseline mean of 62.8 percent. This result holds also when using arrest data as a measure of criminal recidivism, with a decrease of 2.9 percentage points in column (5), or a 4.7 percent reduction relative to the baseline mean.

The results for labor market outcomes are shown in Panel B. Offenders in the treatment group are notably more likely to be employed and exhibit higher average earnings. According to our preferred specification, the reform increased the probability of employment for these offenders by 3.5 percentage points (13.1 percent) compared to the control group, and it led to a 22.1 percent increase in earnings.[31] Additionally, we find a significant increase in disposable income, of 6.8 percent.

Incapacitation effects. Offenders in the control group are by construction more likely to be incapacitated compared to those in the treatment group for the entire duration of their prison spell (at most 12 months). This could lead to underestimating the benefits of EM in terms of reduced recidivism since treated offenders, in the short run, might have more opportunities to re-offend. Conversely, the benefits of EM in terms of labor market outcomes could be partly "mechanical", if they are only due to offenders in the treatment group being able to work while those in the control group are unable to do so. We investigate the role of incapacitation by looking more closely at the dynamic response of the outcomes, beginning with recidivism in Table A1. Before turning to the results, it is important to note that in the Swedish context, offenders placed on EM have limited opportunities to engage in criminal activities because they are confined to their homes and workplaces, subject to continuous monitoring and unscheduled visits by parole officers. Because we lack individual-level information on incarceration and EM status, we are unable to directly investigate the incapacitation channel. However, our results broken down by year after the trial do not support the notion that the benefits of EM in terms of reduced recidivism are substantially muted by incapacitation effects in the control group.[32] In fact, the year-to-year estimates presented in Panel A of Table A1 suggest that EM reduces criminal arrests during the first two years following the trial but not necessarily beyond, with significantly larger effects in the initial year. When examining the dynamic effects on criminal convictions (Panel B), the results are less precise. While they hint at

[31] We choose to use the log of earnings in the analysis due to the substantial differences in earnings levels between the treatment and control groups (see Table 1). To ensure consistency in the sample across all outcomes, we assigned a value of 1 SEK to individuals with zero or missing earnings before taking logs. Note that the estimates based on the log transformation closely mirror those obtained using the specification in levels (point estimate: 1385.3; s.e.: 131.7), which yields a 23.3 percent increase relative to the baseline mean.

[32] Recall that prison sentences do not exceed 12 months for offenders in the control group, with the majority of their sentences leaning toward the lower threshold of 4 months. Hence most of these offenders were incapacitated only during a portion of the first year after the trial.

Table 3
Subgroup results.

	Men (1)	Women (2)	Aged 18–29 (3)	Aged 30–59 (4)	Employed (5)	Not employed (6)
Panel A. Crime						
Arrested for new crime	−0.028***	−0.057**	−0.038***	−0.025***	−0.030**	−0.015*
	(0.007)	(0.029)	(0.011)	(0.009)	(0.014)	(0.008)
	[0.616]	[0.574]	[0.614]	[0.613]	[0.476]	[0.736]
Convicted of new crime	−0.016**	−0.015	−0.024**	−0.011	−0.001	−0.010
	(0.007)	(0.029)	(0.012)	(0.009)	(0.014)	(0.008)
	[0.631]	[0.585]	[0.641]	[0.620]	[0.454]	[0.750]
Panel B: Labor market outcomes						
Employment	0.034***	0.045**	0.042***	0.032***	0.024**	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]
Log earnings (100s SEK)	0.225***	0.233	0.270***	0.211***	0.057	0.155***
	(0.043)	(0.149)	(0.071)	(0.051)	(0.086)	(0.047)
	[2.766]	[2.117]	[3.116]	[2.491]	[4.431]	[1.530]
Log disposable income (100s SEK)	0.069***	0.055	0.103**	0.058*	0.048	0.040
	(0.026)	(0.102)	(0.042)	(0.031)	(0.039)	(0.033)
	[6.415]	[6.512]	[6.295]	[6.497]	[6.798]	[6.146]
N	76,383	4,603	30,660	50,326	29,965	45,487

Notes: This table reports OLS estimates for the difference-in-differences (DiD) model that we use to assess the impact of increased access to electronic monitoring on offender outcomes, for different subgroups of individuals. The coefficients in columns (1) to (6) are on the interaction between the treatment group indicator and an indicator for the post-reform period. The treatment group (54,691 individuals) consists of offenders sentenced to prison for up to three months between 1992 and 1998, while the control group (26,295 individuals) consists of offenders sentenced to prison terms ranging from 4 to 12 months during the same period. Non-employed is defined as having no registered employment in the three years before the trial and employed is defined as having any employment during this period. as All outcomes are measured as averages over the three years following the trial. Criminal recidivism is measured by an indicator for having been re-arrested and an indicator for having a new conviction. "Employment" is an indicator for being registered as formally employed. "Earnings" is total (log) annual labor earnings (in SEK). "Disposable income" is total (log) post-tax income from labor, capital, and transfers (in SEK). All regressions control for sentencing year and sentence length fixed effects (in months), pre-determined demographic characteristics (year of birth, gender, immigrant status), socioeconomic characteristics (educational attainment, earnings and employment in the year before the trial), as well as court and crime type fixed effects. Robust standard errors are reported in parentheses and sub-sample means are reported in square brackets. */**/*** denote significant at the 10/5/1 percent level.

larger effects in absolute terms in the third year post-trial, there are no statistically significant differences across the three years following the trial.[33] Turning to labor market outcomes in Table A2, the results are only partially consistent with the incapacitation hypothesis. On the one hand, the employment effects are indeed larger in the first year. On the other hand, the positive effects on employment and earnings persist in the subsequent years. Moreover, the employment effects in the initial year are not significantly larger than in the second and third years, while the results for earnings suggest that the effect size increases over time, albeit not significantly. Overall, these results indicate that the benefits of EM in terms of labor market outcomes are not entirely driven by a mechanical "anti-incapacitation" effect.

Robustness checks. The fact that our estimates hardly change when we incrementally add control variables suggests that the influence of potential omitted factors is unlikely to be substantial. In the Appendix, we carry out a number of robustness checks to further confirm the validity of our research design. Our estimates are robust to narrowing the bandwidth for inclusion in the control group from 4–12 months to 4–6 months (see Table A3, column 1). Furthermore, we implement a "placebo" regression where we set the reform year to 1996, one year prior to the actual reform, and re-estimate the model described by Eq. (1) while controlling for the actual reform. In this case, the estimates are considerably smaller and not statistically significant, reinforcing the credibility of our baseline results (see Table A3, column 2). As a final robustness check, we estimate a "difference-in-discontinuity" design model (RD-DD) where we examine whether the outcomes change discontinuously around the reform date while at the same time accounting for any potential seasonality in sentencing decisions by using earlier pre-reform years to control for the average discontinuity around the cutoff.[34] The estimates for labor market outcomes are slightly larger than our baseline estimates and most of our estimates are not significantly different compared to baseline (see Table A3, column 3). The only exception is for disposable income where we find a negative estimate that is significant at the 10 percent level.

3.3. Treatment effect heterogeneity

Table 3 presents the results of regression analyses where we partition the sample into different subgroups to explore treatment effect

[33] It is also possible that the lower recidivism rates of treated offenders could reduce their future prison spells, allowing them to maintain stronger ties to the labor market. This could potentially explain the positive effects on labor market outcomes beyond the initial year. Unfortunately, we cannot directly investigate this possibility by examining the impact of future incarcerations. Instead, we use future prison sentences as a proxy for future incarcerations. The results in Panel C of Table A1 do not strongly support the hypothesis that future incapacitation drives the labor market effects beyond the first year, as we do not observe significant effects of EM on future prison sentences.

[34] We follow Ahrsjö (2023) and estimate the following model:

$$Y_{imp} = \gamma_0 + \gamma_1 Treat_{imp} + \gamma_2 (Treat_{imp} * Reform_{imp}) + \gamma_3 date_{imp} + \gamma_4 date_{imp} * Treat_{imp} + \boldsymbol{X}_i \mu + \lambda_p + \epsilon_{imp}, \quad (2)$$

where Y_{imp} is the outcome of interest for individual i, sentence date m and period p (a period is defined as the time interval between July 1st of one year and June 30th of the following year, which we label as year/year+1 for convenience); $Treat_{imp}$ is an indicator that equals one if the sentence was received in 1997 or 1995, and zero if it was received in 1996 or 1994; $Reform_{imp}$ is an indicator that equals one if the sentence was received in 1996/1997. The running variable, sentencing date ($date_{imp}$), enters the regression linearly and is re-centered around January 1st, while allowing the slope to vary before and after that cutoff. The period fixed effects (λ_p) capture the main effect of the reform period. The vector $\boldsymbol{X}_i$ includes the full set of controls for pre-determined individual characteristics as in the baseline model in Table 2. The control period is 1994/1995. The coefficient γ_2 captures the effect of the EM reform. Standard errors are clustered at the court and individual level.

heterogeneity. In columns (1) and (2), we observe that while most estimates are smaller for females, they follow a similar pattern to those for males. However, due to the smaller number of females in the sample (4,603 compared to 76,383 males), these estimates are less precise. When stratifying the sample by (median) age, we notice that individuals aged 18–29 at the time of conviction (column 3) tend to experience slightly larger improvements in outcomes compared to those aged 30–59 (column 4). Lastly, when splitting the sample by baseline employment status, we find that the effect size is in general larger for individuals who were employed at baseline (column 5) than for those who were not employed (column 6).[35] We will return to this result in the next subsection, where we discuss potential mechanisms.

Table A4 and Table A5 in the Appendix present the results from additional subgroup analyses. These results should be interpreted with some caution, as the statistical precision diminishes when stratifying the sample into subgroups. In Table A4, we present separate estimates for the four most common types of crime among offenders granted EM during the study period: property crime, violent crime, drug-related crime, and DUI. The benefits of increased access to EM appear to be concentrated primarily among offenders sentenced for violent crimes or DUI (columns 2 and 4). In contrast, most estimates for property crime and drug-related offenses are smaller in magnitude and often not statistically significant (columns 1 and 3). Table A5 shows a tendency for stronger beneficial effects of increased access to EM among offenders with a criminal history (column 1). However, we also observe improvements in the outcomes of offenders with no prior criminal records (column 2). This suggests that EM may offer benefits to a range of offenders, including those with a more extensive criminal background. When splitting the sample by level of education, we find slightly larger effects for offenders with at least a high school degree (column 4), but there are also significant improvements in the outcomes of offenders with less than a high school degree (column 3).

3.4. Mechanisms

In this section, we explore three 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. We present the results of our analyses related to these mechanisms in Table 4.

Criminal capital. The idea that spending time in prison allows inmates to learn how to commit certain types of crime is not new in the literature. For instance, Bayer et al. (2009) present compelling evidence that individuals who serve time in prison with offenders of similar criminal background are more likely to repeat the same type of crime.[36] To examine this potential mechanism, we distinguish between crimes for which prison may provide opportunities for learning and those for which such opportunities are less likely. Crimes that could be considered “acquired” include property crime and drug-related offenses, such as drug production or trafficking. In contrast, “non-acquired” crimes include violent offenses and drunk driving, which are less likely to be learned within a prison environment (see Bayer et al., 2009). We estimate regression models where the dependent variable is an indicator for being re-convicted for acquired crimes and another indicator for being re-convicted for non-acquired crimes. As shown in Panel A of Table 4, the estimate for acquired crimes is close to zero and statistically insignificant. However, the estimate for non-acquired crimes is significant and indicates that the reform led to a 2.2 percentage-point (5.8 percent) decrease in the likelihood of being re-convicted for such crimes. Overall, these results do not align with the idea that the benefits of EM in terms of reduced recidivism are primarily driven by its ability to prevent offenders from accumulating criminal capital while incarcerated.[37]

Family ties. Stable marriages are often considered a path for adult offenders to move away from a life of crime. If EM reduces the risk of separation or divorce compared to incarceration, it may enhance social integration. In Denmark, Fallesen and Andersen (2017) show that EM significantly reduces the risk of relationship dissolution during the first five years following conviction, possibly because EM helps

Table 4
Mechanisms.

	Outcome mean (1)	DiD estimate (2)
Panel A. Criminal capital		
Acquired crimes	0.413	−0.006
	(0.492)	(0.007)
Non-acquired crimes	0.382	−0.022***
	(0.486)	(0.008)
Panel B. Family ties		
Separation/divorce	0.405	−0.003
	(0.491)	(0.022)
Panel C. Labor Market attachment		
Employment	0.268	0.035***
	(0.387)	(0.005)
Same employer	0.093	−0.000
	(0.270)	(0.003)
New employer	0.078	0.018***
	(0.234)	(0.003)
Non-employment to employment	0.098	0.018***
	(0.243)	(0.004)
N	80,986	80,986

Notes: This table reports OLS estimates for the difference-in-differences (DiD) model that we use to assess the impact of increased access to electronic monitoring on offender outcomes. Column (1) reports the means and standard deviations of the outcomes of interest. Column (2) presents the coefficients on the interaction between the treatment group indicator and an indicator for the post-reform period. The treatment group (54,691 individuals) consists of offenders sentenced to prison for up to three months between 1992 and 1998, while the control group (26,295 individuals) consists of offenders sentenced to prison terms ranging from 4 to 12 months during the same period. All outcomes are measured as averages over the three years following the trial. “Acquired crimes” include property crime and drug dealing while “non-acquired crimes” include violent crimes and driving under the influence of drugs or alcohol. The employment effect in Panel C is decomposed into three components (i) the probability of remaining with the same employer as in the year before the trial, (ii) the probability of switching to a new employer, and (iii) the probability of transitioning from non-employment to employment. All regressions control for sentencing year and sentence length fixed effects (in months), pre-determined demographic characteristics (year of birth, gender, immigrant status), socioeconomic characteristics (educational attainment, earnings and employment in the year before the trial), as well as court and crime type fixed effects. Robust standard errors are reported in parentheses. */**/*** denote significant at the 10/5/1 percent level.

[35] When splitting the sample based on the employment status, we lose 5,543 observations from the main sample. These individuals are missing baseline employment status because they were not registered as living in Sweden in three years before the trial. These observations are included in the main analysis where we also control for the missing data. Note, however, that the estimates in columns (5) and (6) of Table 3 are not significantly different from the main results reported in Table 2.

[36] Stevens (2017) shows that exposure to young inmates from unstable homes with behavioral issues increases recidivism, but finds limited evidence of skill transfer or network formation mechanisms. Other studies highlighting the importance of peer effects in criminal behavior include, e.g., Glaeser et al. (1996), Ballester et al. (2006), and Billings et al. (2019).

[37] It should be noted, however, that since EM is granted only to offenders serving shorter sentences, our findings do not necessarily rule out the possibility of skill transfers occurring among prisoners serving longer sentences.

alleviate household strain by allowing the offender to continue providing financial and emotional support to their partner while serving their sentence (e.g., Apel et al., 2010). To investigate whether this mechanism applies in our setting, we estimate a regression model where the dependent variable is an indicator for divorce or separation. Despite a significant percentage of offenders experiencing relationship dissolution within three years after their trial (40.5 percent), the results in Panel B of Table 4 do not show a significant effect of increased access to EM on the likelihood of divorcing or separating. Therefore, we do not find evidence that in the Swedish context, the benefits of EM for offenders are primarily mediated by the preservation of their family relationships.

Labor market attachment. Regular employment is often considered one of the strongest predictors of successful rehabilitation, which is why most EM programs emphasize the importance of securing a job or actively searching for one (e.g., De Troyer, 2020; 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' long-term job prospects (e.g., Lochner, 2004). While the evidence on this matter is mixed, many empirical studies show that incarceration deteriorates labor market outcomes (e.g., Mueller-Smith, 2015; Dobbie et al., 2018b).[38] Our results in Table 3, where we stratified the sample by baseline employment status (see columns 5 and 6), showed that the significant improvements in offenders' outcomes were more pronounced among those who had been employed in the year preceding their trial. This suggests that maintaining ties to the labor market could be an important mechanism behind the beneficial effects of EM.

To better understand how EM might enhance labor market outcomes, we decompose the employment effects of the EM reform into three mutually exclusive components: (i) remaining with the same employer; (ii) switching to a new employer; and (iii) transitioning from non-employment to employment. This approach enables us to differentiate and evaluate the contribution of each component to the overall employment effect, helping us understand whether the improvements in employment outcomes stem primarily from the continuation of existing employment relationships or from the ability to secure new job prospects. The results of this analysis are shown in Panel C of Table 4. While the estimates for the probability of remaining with the same employer are statistically insignificant, both the estimates for changing employers and the estimates for transitioning from non-employment to employment are statistically significant and of similar magnitude. This suggests that EM primarily increases employment by reducing barriers to both finding a job and changing employers, with each component contributing about 50 percent to the overall employment effect (0.018/0.035).

In addition to providing offenders with more time for job searching compared to incarceration, the positive effects of EM on employment outcomes could potentially be attributed to a reduction in employer discrimination. Although EM does not erase the mention of a prison sentence, it is likely to mitigate the stigma associated with having spent time in prison – a piece of information that employers may infer by scrutinizing gaps in a job applicant's resume or by posing questions during job interviews (e.g., Western et al., 2001; Lofstrom and Raphael, 2016). While the data at hand does not allow us to provide conclusive evidence regarding this mechanism, we consider it a plausible channel that would deserve further investigation in future research.[39]

[38] While most correctional facilities provide training and education opportunities for inmates, most empirical studies show a decline in earnings and employment after a prison term. This suggests that the negative impact of human capital depreciation (and possibly other factors) outweighs any potential gains from acquiring new skills while incarcerated.

[39] It is also conceivable that labor market attachment is a mediator for reduced recidivism, although we are unable to directly test this mechanism.

In summary, our exploration of mechanisms does not strongly support the idea that EM improves outcomes by preventing offenders from accumulating criminal capital in prison or by preserving family stability. Instead, our results suggest that the social benefits of EM arise, at least in part, from its ability to allow offenders to maintain ties to the labor market and potentially reduce employer discrimination against ex-prisoners.

3.5. Spillover effects on family members

We have seen that expanded access to EM significantly enhances the future prospects of offenders. In Table A6 in the Appendix, we show that these findings also hold true for the subset of offenders with children. Besides the direct impact of the reform on the offenders themselves, there are several reasons to anticipate that their family members may be impacted as well, though the direction of these effects is theoretically ambiguous. For instance, children could benefit from increased family resources and avoid the emotional trauma or social stigma associated with having a parent sent to prison, as suggested by Wildeman (2010) and Murray et al. (2012). On the flip side, children may potentially suffer from being exposed to a bad role model at home. Similarly, spouses could experience either positive or negative effects from their partner serving time at home under curfew rather than in prison. For instance, there might be a reverse "added worker" effect, where the improved labor market outcomes of the convicted head of the household could reduce the spouse's labor supply (e.g., Lundberg, 1985). Conversely, the increased ability to share household responsibilities and childcare may enable the spouse to allocate more time to work, making the overall impact uncertain. Irrespective of the direction of these effects, conducting a comprehensive assessment of the costs and benefits associated with EM must take into account these potential family-related spillover effects.

Following Dobbie et al. (2018b), we empirically investigate this question by examining the outcomes of children who were aged 11–14 at the time of their parent's trial. This period in a child's life may be particularly vulnerable to disruptions in the home environment. Table 5 shows estimates of the impact of the EM reform on the outcomes of offenders' children. "Crime arrest at ages 15–17" is a dummy for the offender's child having been arrested between the ages of 15 and 17, while "Criminal conviction at ages 15–17" is a dummy for the child having been convicted between the same ages. "Compulsory school GPA" is the percentile rank (by cohort) of the child's final grade point average (GPA) in compulsory school (measured at age 16). "High school diploma at age 19" is a dummy for having completed high school by the age of 19. "Employment at age 25" is a dummy for being employed at age 25. "Log Earnings at age 25" is total (log) annual labor earnings at age 25. As in Table 2, column (1) in Table 5 reports the outcome means and standard deviations, while columns (2) through (5) report the estimates from models with different sets of controls. Standard errors are clustered at the family level.

Most of the estimates for the impact of parental exposure to the reform on children's outcomes are statistically insignificant, which is expected given the relatively small sample size of children in our dataset (12,530). However, we do observe a statistically significant increase in the probability that the child obtains a compulsory school degree at age 16, of 3 percentage points (3.6 percent of the sample mean). Additionally, we find that parental exposure to the reform leads to an approximately 25 percent increase in the child's earnings at age 25 and a roughly 16 percent increase in disposable income. The overall pattern of the other estimates also suggests improved outcomes and the precision is sufficient to rule out large adverse effects of the reform on children. For example, the upper bound of the 95 percent confidence interval for the probability that the child is arrested rules out increases of more than 1.7 percentage point, or 5.9 percent relative to the sample mean. These findings are broadly consistent with the results in Dobbie et al. (2018b), who find negative effects of parental

Table 5
Effects on offenders' children.

	Outcome mean	Difference-in-differences estimates			
	(1)	(2)	(3)	(4)	(5)
Panel A. Teen crime					
Crime arrest at ages 15–17	0.292	−0.029	−0.025	−0.022	−0.020
	(0.455)	(0.020)	(0.019)	(0.019)	(0.019)
Criminal conviction at ages 15–17	0.256	−0.017	−0.013	−0.010	−0.008
	(0.437)	(0.018)	(0.018)	(0.018)	(0.018)
Panel B. Teen educational outcomes					
Compulsory school GPA (percentile rank)	28.275	1.931*	1.409	1.153	1.122
	(25.111)	(1.089)	(1.063)	(1.064)	(1.065)
Compulsory school degree at age 16	0.825	0.036**	0.032*	0.030*	0.030*
	(0.380)	(0.017)	(0.017)	(0.017)	(0.017)
High school diploma at age 19	0.368	0.027	0.020	0.012	0.011
	(0.482)	(0.021)	(0.021)	(0.021)	(0.021)
Panel C. Adult labor market outcomes					
Employment at age 25	0.600	0.013	0.011	0.010	0.010
	(0.490)	(0.021)	(0.021)	(0.021)	(0.021)
Log earnings (100s SEK) at age 25	5.501	0.286**	0.265*	0.254*	0.253*
	(3.182)	(0.140)	(0.139)	(0.140)	(0.139)
Log disposable income (100s SEK) at age 25	6.966	0.160**	0.157**	0.161***	0.163***
	(1.407)	(0.062)	(0.062)	(0.062)	(0.062)
Controls					
Sentencing year FEs	–	Yes	Yes	Yes	Yes
Sentence length FEs	–	Yes	Yes	Yes	Yes
Demographic characteristics	–	Yes	Yes	Yes	Yes
Socioeconomic characteristics	–	–	Yes	Yes	Yes
Court FEs	–	–	–	Yes	Yes
Crime type FEs	–	–	–	–	Yes
N	12,530	12,530	12,530	12,530	12,530

Notes: This table reports OLS estimates for the difference-in-differences (DiD) model that we use to assess the impact of increased access to electronic monitoring on the outcomes of offenders' children. The children are aged 11–14 at the time of the trial. Column (1) reports the means and standard deviations of the outcomes of interest. Columns (2) to (5) present the coefficients on the interaction between the treatment group indicator and an indicator for the post-reform period. The treatment group consists of the children of offenders who were sentenced to prison for up to three months between 1992 and 1998, while the control group consists of the children of offenders who were sentenced to prison terms ranging from 4 to 12 months during the same period. Teen criminal behavior is measured by an indicator for having been arrested at ages 15–17 and an indicator for having a conviction at ages 15–17. Teen educational outcomes are measured by the percentile rank (by cohort) of the final grade point average (GPA) in compulsory school, an indicator for having a compulsory school degree by age 16, and an indicator for having a high school diploma at age 19. Adult labor market outcomes are measured by as an indicator for being employed at age 25, total (log) annual labor earnings (in SEK) at age 25, and (log) disposable income (in SEK) at age 25. Average earnings at age 25 are 13,966 USD. The estimates in column (2) are from a model that controls for sentencing year fixed effects, sentence length fixed effects (in months), and pre-determined demographic characteristics (year of birth, gender, immigrant status). Column (3) additionally controls for socioeconomic characteristics (educational attainment, earnings and employment in the year before the trial). Column (4) expands the set of controls to include court fixed effects. Column (5) further controls for crime type fixed effects. Standard errors clustered at the family level are reported in parentheses. */**/*** denote significant at the 10/5/1 percent level.

incarceration on children's outcomes. However, our estimates are less precise and generally smaller in magnitude.[40]

Table 6 presents the results of the effects of the reform on the outcomes of the other (non-convicted) parent. The estimates are relatively imprecise and, for the most part, are not statistically significant. There is, however, a significant negative effect on the (log) disposable income of the offender's partner, of approximately 4.8 percent. This finding could potentially be attributed to decreased social transfers resulting from the offender's increased earnings.

In summary, while our analysis reveals significant positive effects of EM on specific outcomes for the members of the offender's family, the key takeaway is that our estimates are precise enough to rule out large adverse effects.

4. Social benefit calculations

This section brings together our various sets of results to estimate the social benefits derived from EM. The back-of-the-envelope calculations that we perform should be interpreted with caution, as there can be several potential sources of bias when incorporating estimates into a welfare analysis. To minimize these risks, we adopt a conservative approach by selecting the lower limits of the estimated benefits when we have more than one option. We distinguish between the direct benefits that are linked to the fiscal savings from EM and the indirect benefits arising from improved labor market outcomes and reduced crime. All figures presented in this section are reported in current (2023) prices and converted to US dollars using the current exchange rate (10.22 SEK/USD).

4.1. Direct benefits

The direct benefits from EM can be assessed by comparing the costs per client in the EM program to the cost of an equivalent length of time in prison. According to the Prison and Probation Service, the daily costs per client enrolled in EM was 113 USD in 1998 (Brottsförebyggande Rådet (BRÅ), 1999, page 48). In contrast, if these clients had been placed in a relatively low-cost minimum security prison, the average daily cost would have been 184 USD (Brottsförebyggande Rådet (BRÅ), 1999, page 48). Our own calculations show that the average length of a prison sentence for offenders in the treatment group was about 50 days. Therefore, we estimate the average costs of an offender placed on EM to be 5,650 USD (50*113) and the resulting fiscal savings to be 3550 USD (50*(184 − 113)) per EM admission.

[40] A difference between the two studies is that the populations considered are not directly comparable: while Dobbie et al. (2018b) examine the effects of incarceration for the universe of offenders sentenced to prison, our study focuses on relatively low-risk offenders who were sentenced to short prison terms and subsequently approved for EM.

Table 6
Effects on the other (Non-Convicted) parent.

	Outcome mean	Difference-in-differences estimates			
	(1)	(2)	(3)	(4)	(5)
Panel A. Crime					
Arrested for a crime	0.065	−0.012	−0.012	−0.011	−0.011
	(0.247)	(0.011)	(0.011)	(0.011)	(0.011)
Convicted of a crime	0.130	0.002	0.004	0.004	0.005
	(0.336)	(0.015)	(0.015)	(0.015)	(0.015)
Panel B. Labor market outcomes					
Employment	0.546	0.006	0.002	−0.003	−0.004
	(0.449)	(0.020)	(0.020)	(0.020)	(0.020)
Log earnings (100s SEK)	5.262	0.063	0.038	0.013	0.009
	(3.080)	(0.143)	(0.141)	(0.141)	(0.141)
Log disposable income (100s SEK)	7.437	−0.047*	−0.050*	−0.048*	−0.048*
	(0.625)	(0.026)	(0.026)	(0.027)	(0.027)
Controls					
Sentencing year FEs	–	Yes	Yes	Yes	Yes
Sentence length FEs	–	Yes	Yes	Yes	Yes
Demographic characteristics	–	Yes	Yes	Yes	Yes
Socioeconomic characteristics	–	–	Yes	Yes	Yes
Court FEs	–	–	–	Yes	Yes
Crime type FEs	–	–	–	–	Yes
N	10,600	10,600	10,600	10,600	10,600

Notes: This table reports OLS estimates for the difference-in-differences (DiD) model that we use to assess the impact of increased access to electronic monitoring on the outcomes of the other (non-convicted) parent. Column (1) reports the means and standard deviations of the outcomes of interest. Columns (2) to (5) present the coefficients on the interaction between the treatment group indicator and an indicator for the post-reform period. The sample is restricted to the partners of offenders with children aged 11–14 at the time of their trial. The treatment group consists of the partners of offenders who were sentenced to prison for up to three months between 1992 and 1998, while the control group consists of the partners of offenders who were sentenced to prison terms ranging from 4 to 12 months during the same period. All outcomes are measured as averages over the three years following the trial. Criminal behavior is measured by an indicator for having been arrested and an indicator for having a conviction. "Employment" is an indicator for being registered as formally employed. "Earnings" is total (log) annual labor earnings (in SEK). "Disposable income" is total (log) post-tax income from labor, capital, and transfers (in SEK). The estimates in column (2) are from a model that controls for sentencing year fixed effects, sentence length fixed effects (in months), and pre-determined demographic characteristics (year of birth, gender, immigrant status). Column (3) additionally controls for socioeconomic characteristics (educational attainment, earnings and employment in the year before the trial). Column (4) expands the set of controls to include court fixed effects. Column (5) further controls for crime type fixed effects. Robust standard errors are reported in parentheses. */**/*** denote significant at the 10/5/1 percent level.

4.2. Indirect benefits

While there are large fiscal savings from EM, a comprehensive welfare analysis should also consider the indirect benefits resulting from the improved outcomes of the individuals involved. Quantifying these indirect benefits requires assigning a monetary value to our estimated effects. If we assume that the interaction between treatment status and the post-reform period is uncorrelated with the error term in Eq. (1) and does not directly influence the outcomes of the offenders, except through increasing the take-up of EM, we can rescale our Intention to Treat (ITT) estimates in Table 2 by the effect of the reform on the EM take-up in Fig. 3 (0.301) to obtain the implied 2SLS estimates. With this estimate in hand, we can calculate the average benefits among those individuals who are actually placed on EM. We focus on convictions and labor market outcomes, as assigning monetary values is less straightforward for other outcomes.[41] Since the first-stage and reduced-form estimates are based on two different samples, we refer to $\hat{\theta}$ as the two-sample two-stage least squares (TS-2SLS) estimate of EM participation on offenders' outcomes. The TS-2SLS standard errors are calculated using the delta method.[42]

The results, reported in Table A7 in the Appendix, show that the use of EM has economically meaningful effects on the outcomes of offenders. The TS-2SLS estimate in column (1) indicates that EM reduces the probability of re-conviction by 4.7 percentage points. This estimate is smaller than the corresponding 2SLS estimate in Di Tella and Schargrodsky (2013), who find 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 the re-offending rate found by Williams and Weatherburn (2020) in Australia. The difference in the results may be attributed to several factors, including differences in institutional contexts (e.g., the effect could be partly offset by the extensive Swedish welfare state) and potential variations in the characteristics of compliers. While there are no similar studies examining labor market outcomes, the TS-2SLS estimates reported in columns (2) and (3) indicate that EM increases the likelihood of employment by 11.6 percentage points and raises annual earnings by 4,503 USD. This means that over the three-year period during which offenders are followed, the reform resulted in a total earnings increase of approximately 13,509 USD (4503*3).

We have also documented that parental exposure to EM has a significant and positive impact on the earnings of offenders' children. Scaling the ITT estimate in Table 5 (column 5) by the first-stage estimate in Fig. 3, and multiplying the resulting value by the average earnings at age 25 in our sample (13,966 USD) suggests that the annual earnings of children whose parents are placed on EM increase by 11,739 USD (13966*0.253/0.301). Under the conservative assumptions that these increased earnings only concern children who were aged 11–14 at the time of their parent's trial (which corresponds to the sample used in Table 5), that they are only observed for three years (just as for the parents) between the ages of 25 and 28, and that the average number of children aged 11–14 per offender in our sample is 15 percent (12530/80986), the earnings gain would amount to 5,283 USD (11739*3*0.15) per EM admission. Note that in these welfare calculations, we choose to ignore changes in the disposable income of

[41] Given the nearly identical results for earnings and log earnings when evaluating the effects at the sample mean, we opt for using earnings for simplicity when calculating the benefits of EM.

[42] We calculate the variance of $\hat{\theta}$ using the formula $\mathrm{Var}(\hat{\theta}) = \mathrm{Var}(\hat{\beta}/\hat{\pi}) \approx [\hat{\pi}^2\,\mathrm{Var}(\hat{\beta}) + \hat{\beta}^2\,\mathrm{Var}(\hat{\pi})]/\hat{\pi}^4$, where $\hat{\pi}$ and $\hat{\beta}$ denote the first-stage and reduced-form estimates, respectively. The standard error of the TS-2SLS estimate is then calculated as the square root of $\hat{\theta}$, with the variances in the formula replaced by their respective estimates.

the children and the other non-convicted parent, as these likely just reflect changes in social transfers.

To monetize the benefits of EM in terms of crime reduction, we rely on the estimates provided in Mueller-Smith (2015), which take into account the comprehensive costs associated with crime, including the property loss, productivity losses, and the resources allocated to the legal system for arresting, charging, and convicting offenders. To be conservative in our cost estimations, we consider only the lower bounds of the values reported in Mueller-Smith (2015).[43]

The TS-2SLS estimate in column (1) of Table A7 suggests that the use of EM prevented 0.047 re-convictions per EM admission during our three-year follow-up period. To translate our findings regarding criminal conviction into actual crime averted, we adjust this number by the crime clear-up rate, which remained at 16 percent throughout our study period, according to the National Council for Crime Prevention (Brottsförebyggande Rådet (BRÅ), 2010). If we assume that each conviction corresponds to one crime, we can estimate that each EM admission prevents 0.29 crime (0.047/0.16). To calculate the benefits resulting from the reduction in crime, we then multiply this figure by the cost estimates in Mueller-Smith (2015), yielding an overall savings of 5713 USD per EM admission.[44]

4.3. Total social benefits

The back-of-the-envelope calculations presented in this section suggest that the total indirect benefits of EM amount to approximately 24,500 USD per EM admission, which is about four times as large as the costs of the EM program and about seven times larger than the direct benefits from EM in terms of its fiscal savings. The most important component of the indirect benefits is the improved earnings for the offenders, which make up over half of the indirect benefits.

While the private returns to this type of program are relevant to the welfare analysis, it is also important to ask how EM affects the government budget. Combining the earnings gains for the offenders and their children, we estimate the private returns to be approximately 18,800 USD per EM admission. Since the average income tax in Sweden is about 30 percent of gross earnings, we conclude that using EM as a substitute to incarceration increases tax revenues by approximately 5,640 USD per EM admission, i.e., an amount equivalent to the cost of the program per admission and about 60 percent larger than the fiscal savings from EM.

An alternative and more formal way to characterize the welfare consequences of the EM reform is to calculate its marginal value of public funds (MVPF). Hendren and Sprung-Keyser (2020) define MVPF as $\Delta W/(\Delta E - \Delta C)$, where ΔW represents the benefits that the reform provides to individuals in the population (i.e., the offenders and their children), ΔE denotes the government's upfront expenditure on the reform (i.e., the average costs of EM per admission), and ΔC represents the reduction in government costs due to the reform (i.e., the cost of prison avoided and the cost of crimes averted). A policy with an MVPF of x means that the policy delivers x USD of benefits per dollar of net government spending.

Our estimates suggest that $\Delta W = 13{,}509 + 5283 = 18{,}792$ USD, $\Delta E = 5650$ USD, and $\Delta C = 9200 + 5713 = 14{,}913$ USD. Notably, ΔC is greater than ΔE. In this case, the policy pays for itself, which is defined to be an infinite MVPF (see Hendren and Sprung-Keyser, 2020).

Note that these calculations assume that the program only provides indirect benefits over a three-year follow-up window. If one is willing to assume that the benefits persist over a longer period, the benefits of EM would need to be adjusted upward accordingly.

5. Concluding remarks

Electronic monitoring is widely used throughout the world to combat the high costs of large prison systems. Yet, in light of the theoretical uncertainties surrounding the effects of electronic monitoring on offenders themselves, there is surprisingly little rigorous empirical evidence, especially concerning labor market outcomes. Moreover, data limitations have prevented past research from learning about the underlying mechanisms and from studying potentially important spillover effects on partners and children.

We present evidence from Sweden's early nationwide adoption of EM in 1997, which made individuals sentenced to a maximum of three months in prison eligible for EM as an alternative to incarceration. Using offenders above the eligibility cutoff as a control group, our difference-in-differences estimates indicate that the expanded access to EM significantly lowered criminal recidivism by 2.2 to 4.7 percent and boosted earnings and employment by 22.1 and 13.1 percent, respectively. The key mechanism driving these improvements seems to be that EM provides an opportunity for offenders to maintain their labor market ties. While the benefits of EM are concentrated on the offenders themselves, the reform does not generate adverse consequences for their family members. In fact, parental exposure to the reform improves the educational attainment and early adulthood earnings of their children. Our back-of-the-envelope calculations suggest that the social benefits derived from EM outweigh the direct fiscal savings from reduced prison expenses by a factor of approximately seven. Notably, the labor market channel plays a central role in driving these social benefits, surpassing the impact on recidivism.

Similar to other Nordic countries, Sweden has a much more generous social welfare system than most other OECD countries. This system encompasses 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 the state covering roughly 90 percent of the costs for most families. Every child has access to tuition-free education from elementary school through higher education and means-tested social aid programs provide economic support, preventing families from falling into poverty as a last resort. These programs are considerably more extensive and comprehensive than their counterparts in many other countries. It is therefore conceivable that the social benefits of EM in countries with less generous welfare systems could be even larger. This notion is supported by the observation that our 2SLS effects on recidivism appear smaller compared, e.g., to the effects of EM in Argentina, where (Di Tella and Schargrodsky, 2013) find a 15 percent reduction in recidivism. That said, unlike Sweden, many countries exclude offenders convicted of violent crimes from eligibility for EM. These are offenders who may have particularly poor prospects to rehabilitate under EM. Moreover, despite the large welfare state, there is no specific support 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). As a result, our analysis may

[43] The upper and lower bounds of the cost estimates for different types of crimes in Mueller-Smith (2015) are as follows: 41,046 to 109,903 USD for assaults, 9598 to 9974 USD for larceny, 2544 USD for drugs, and 25,842 USD for DUI. The fact that we do not find significant effects on arrests in year 3 (see Panel A of Table A1 in the Appendix) supports our "conservative" assumption of not considering benefits from crime reduction beyond the third year.

[44] To perform this calculation, we weight the cost estimates for specific crimes reported in Mueller-Smith (2015) by the composition of crime types in the treatment group in our data (see Table 1). We make the conservative assumption that all violent crimes are assaults, all property crimes are larceny, and all drug-related crimes are possession. We also do not assign a monetary value to the 20 percent of other crimes that are included in our data. The estimated benefit of crime reduction per EM admission is then computed as 0.29*(0.214*41046+0.187*9598+0.051*2544+0.348*25842).

understate the social costs of EM. This is also an important avenue for future research.[45]

Declaration of competing interest

All authors of this paper declare no conflict of interest.

Data availability

The authors do not have permission to share data.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2023.105051.

References

Agan, A., Starr, S., 2018. Ban the box, criminal records, and racial discrimination: A field experiment. Q. J. Econ. 133 (1), 191–235.

Ahrsjö, U., 2023. Youth crime, community service and labor market outcomes. Unpublished manuscript, Stockholm University.

Aizer, A., Doyle, J., 2015. Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. Q. J. Econ. 130 (2), 759–804.

Andersen, L., Andersen, S., 2014. Effect of electronic monitoring on social welfare dependence. Criminol. Public Policy 13 (3), 349–379.

Apel, R., Arjan, A., Blokland, J., Nieuwbeerta, P., Van Schellen, M., 2010. The impact of imprisonment on marriage and divorce: A risk set matching approach. J. Econ. Lit. 26 (2), 269–300.

Arteaga, C., 2022. Parental incarceration and children's educational attainment. Rev. Econ. Stat.

Ballester, C., Calvó-Armengol, A., Zenou, Y., 2006. Who's who in networks. Wanted: The key player. Econometrica 74 (5), 1403–1417.

Barnes, G., C., H.J.M., C., A.L., L., K.D.T., 2012. The effects of low-intensity supervision for lower-risk probationers: Updated results from a randomized controlled trial. J. Crime Justice 35 (2), 200–220.

Bartels, L., Martinovic, M., 2015. Electronic monitoring: The experience of Australia. Eur. J. Probation 9 (1), 80–102.

Bayer, P., Hjalmarsson, R., Pozen, D., 2009. Building criminal capital behind bars: Peer effects in juvenile corrections. Q. J. Econ. 124 (1), 105–147.

Becker, G.S., 1968. Crime and punishment: An economic approach. J. Polit. Econ. 76 (2), 169–217.

Bhuller, M., Dahl, G., Løken, K., Mogstad, M., 2018. Intergenerational effects of incarceration. Am. Econ. Rev. Pap. Proc. 108 (2), 234–240.

Bhuller, M., Dahl, G., Løken, K., Mogstad, M., 2020. Incarceration, recidivism and employment. J. Polit. Econ. 128 (4), 1269–1324.

Billings, S., Deming, D., Ross, S., 2019. Partners in crime. Am. Econ. J.: Appl. Econ. 11 (1), 126–150.

Boyle, D.J., Ragusa-Salerno, L.M., Lanterman, J.L., Marcus, A.F., 2013. An evaluation of day reporting centers for parolees. Criminol. Public Policy 12 (1), 119–143.

Brottsförebyggande Rådet (BRÅ), 1993–2015. Kriminalstatistisk, 1993–2015. Sveriges officiella statistik, Stockholm.

Brottsförebyggande Rådet (BRÅ), 1999. Intensivövervakning Med Elektronisk Kontroll: UtväRdering Av 1997 Och 1998 åRs Riksomfattande Försöksverksamhet. BRÅrapport 1999:4.

Brottsförebyggande Rådet (BRÅ), 2010. Kriminalstatistik 2009. BRÅrapport 2010:15.

Brottsförebyggande Rådet (BRÅ), 2017. Att Minska Isolering I HäKte. BRÅrapport 2017:6.

Bungerfeldt, J., 2014. Old and new uses of electronic monitoring in Sweden. Crim. Justice Matters 95 (1), 4–5.

Chalfin, A., McCrary, J., 2017. Criminal deterrence: A review of the literature. J. Econ. Lit. 55 (1), 5–48.

De Troyer, M., 2020. Living and Working under Electronic Monitoring. The European Trade Union Institute.

Di Tella, R., Schargrodsky, E., 2013. Criminal recidivism after prison and electronic monitoring. J. Polit. Econ. 121 (1), 28–73.

Dobbie, W., Goldin, J., Yang., C., 2018a. The effects of pre-trial detention on conviction, future crime, and employment: Evidence from randomly assigned judges. Amer. Econ. Rev. 108 (2), 201–240.

[45] 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".

Dobbie, W., Grönqvist, H., Niknami, S., Palme, M., Priks, M., 2018b. The Intergenerational Effects of Parental Incarceration. NBER Working Paper No. 24186.

Drago, F., Roberto, G., Vertova, P., 2009. The deterrent effects of prison: Evidence from a natural experiment. J. Polit. Econ. 117 (2), 257–280.

Fallesen, P., Andersen, L., 2017. Explaining the consequences of imprisonment for union formation and dissolution in Denmark. J. Policy Anal. Manag. 36 (1), 154–177.

Freeman, R., 1999. The economics of crime. In: Ashenfelter, O., Card, D. (Eds.), Handbook of Labor Economics. In: Vol. 3C, North Holland Publishers, Amsterdam, Netherlands.

Georgiou, G., 2014. Does increased post-release supervision of criminal offenders reduce recidivism? Evidence from a statewide quasi-experiment. Int. Rev. Law Econ. 37 (C), 221–243.

Glaeser, E., Sacerdote, B., Scheinkman, J., 1996. Crime and social interactions. Q. J. Econ. 111 (2), 507–548.

Grogger, J., 2018. The effect of arrests on the employment and earnings of Young men. Q. J. Econ. 110 (1), 51–71.

Hendren, N., Sprung-Keyser, B., 2020. A unified welfare analysis of government policies. Q. J. Econ. 135 (3), 1209–1308.

Henneguelle, A., Monnery, B., Kensey, A., 2016. Better at home than in prison? The effects of electronic monitoring on recidivism in France. J. Law Econ. 59 (3), 629–667.

Kling, J., 2006. Incarceration length, employment and earnings. Amer. Econ. Rev. 96 (3), 863–876.

Kuziemko, I., 2013. How should inmates be released from prison? An assesment of parole versus fixed sentence regimes. Q. J. Econ. 178 (1), 371–424.

Kyckelhahn, T., 2011. Direct Expenditures by Criminal Justice Function, 1982–2007. Technical Report, Bureau of Justice Statistics, Washington, D.C.

Landersjo, R., 2015. Does incarceration length affect labor market outcomes? J. Law Econ. 58 (1), 205–234.

Lochner, L., 2004. Education, work, and crime: A human capital approach. Internat. Econom. Rev. 45 (3), 811–843.

Lofstrom, M., Raphael, S., 2016. Crime, the criminal justice system, and socioeconomic inequality. J. Econ. Perspect. 30 (2), 103–126.

Lundberg, S., 1985. The added worker effect. J. Labor Econ. 3 (1), 11–37.

Marie, O., 2009. The best ones come out first! early release from prison and recidivism: A regression discontinuity approach. Unpublished manuscript, Royal Holloway University of London.

Mueller-Smith, M., 2015. The criminal and labor market impacts of incarceration. Unpublished manuscript, University of Michigan.

Murray, J., Farrington, D., Sekol, I., 2012. Children's antisocial behavior, mental health, drug use, and educational performance after parental incarceration: A systematic review and meta-analysis. Psychol. Bull. 138 (2), 175–210.

Nagin, D.S., 2013. Deterrence: A review of the evidence by a criminologist for economists. Annu. Rev. Econ. 5, 83–105.

Norris, S., Pecenco, M., Weaver, J., 2021. The effects of parental and sibling incarceration: Evidence from Ohio. Amer. Econ. Rev. 111 (9), 2926–2963.

Penal Reform International / Thailand Institute of Justice (PRI/TIJ), 2020. Global Prison Trends 2020. Penal Reform International and Thailand Institute of Justice.

PEW, 2016. Use of electronic offender-tracking devices expands sharply. PEW Charitable Trust: Issue Brief.

Renzema, M., Mayo-Wilson, E., 2005. Can electronic monitoring reduce crime for moderate to high-risk offenders? J. Exp. Criminol. 1, 215–237.

Rivera, R., 2023. Release, detain or surveil? The effects of electronic monitoring on defendant outcomes. Unpublished manuscript, Columbia University.

Roth, J., Sant'Anna, P., Bilinski, A., Poe, J., 2023. What's trending in difference-in-differences? A synthesis of the recent econometrics literature. J. Econometrics 235 (2), 2218–2244.

Statistiska Centralbyrån (SCB), 1985–1992. Rättsstatistisk Årsbok, 1985–1992. Sveriges officiella statistik, Stockholm.

Stevens, M., 2017. Breaking bad: Mechanisms of social influence and the path to criminality in juvenile jails. Rev. Econ. Stat. 99 (5), 824–838.

Wennerberg, I., 2013. High level of support and high level of control: An efficient Swedish model of electronic monitoring? In: Nellis, M., Beyens, D., Kaminski, D. (Eds.), Electronically Monitored Punishment: International and Critical Perspectives. Routledge, London.

Western, B., Kling, J., Weiman, D., 2001. The labor market consequences of incarceration. Crime Delinquency 74 (3), 410–427.

Wildeman, C., 2010. Paternal incarceration and children's physically aggressive behaviors: Evidence from the fragile families and child wellbeing study. Social Forces 89 (1), 285–309.

Williams, J., Weatherburn, D., 2020. Can electronic monitoring reduce reoffending? Rev. Econ. Stat. 104 (2), 232–245.

Yang, C., 2017. Local labor markets and criminal recidivism. J. Public Economics 147 (C), 16–29.