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Quasi-experimental evidence from Norway

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Abstract:
The replacement of custodial with non-custodial sanctions holds the potential to reduce recidivism as well as other costs associated with imprisonment. However, the causal impacts on recidivism of non-custodial sanctions in general, and electronic monitoring (EM) programs in particular, remain unclear. We estimate the effect of EM on recidivism by exploiting an EM program that was gradually introduced in Norwegian counties from 2008, using difference-in-differences and instrumental variable designs. Results show that introducing EM reduced 2-year recidivism rates by about 10 percent, which corresponds to about 19 percent for those actually serving on EM. We find no effects on recidivism intensity or severity. Subsample analyses show that the effect estimates are strongest among offenders without previous imprisonment or recent unemployment spells, and although between-groups differences are statistically non-significant, this suggest that avoiding prison stigma and maintaining workplace relations can be important to reduce recidivism and promote desistance. The reliability of our results is somewhat challenged by unstable pre-implementation trends and signs that more people are convicted to EM-qualifying sentences when EM is introduced.

Keywords: Electronic monitoring, non-custodial sanctions, recidivism, difference-in-differences, instrumental variable

JEL classification: K49, J19

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Sammendrag

I september 2008 introduserte Kriminalomsorgen straffegjennomføring med elektronisk kontroll (også kalt fotlenke) som et prøveprosjekt i seks fylker i Norge. Med et uttalt mål om å redusere soningskøen og bedre den enkeltes forutsetning for et kriminalitetsfritt liv, åpnet denne straffegjennomførings-formen for at ubetingede fengselsstraffer på inntil 120 dager kunne sones utenfor fengsel[1] – gitt at den domfelte søkte om dette og ble godkjent i en egnethetsvurdering gjennomført av Kriminalomsorgen.

Hvorvidt fotlenkeordningen har ønsket effekt på tilbakefall til ny kriminalitet er et viktig kriminalpolitisk spørsmål. Når vi sammenligner de som får anledning til å sone straffen på fotlenke med de som soner straffen i fengsel, finner vi at de på fotlenke har 47 prosent lavere tilbakefallsrate (målt etter to år) og at de i tillegg begår færre og mindre alvorlige lovbrudd (jf. Rasmussen mfl., 2016; Rokkan, 2012b). En slik direkte sammenligning tar imidlertid ikke hensyn til seleksjonsprosessen som sørger for at kun bestemte lovbrytergrupper (med lav tilbakefallsrisiko) tilbys å sone med fotlenke. Dette gjør det vanskelig å vite hvorvidt de som soner med fotlenke har lavere tilbakefall *fordi* de soner straffen sin på denne måten, eller om de ville hatt det uansett. Denne distinksjonen mellom korrelasjon og kausalitet er viktig når man ønsker å si noe om effektene av ulike politiske ordninger og tiltak.

I denne analysen søker vi derfor å ta hensyn til slike metodiske utfordringer ved å benytte et kvasi-eksperimentelt design. Vi utnytter at den gradvise implementeringen av fotlenkeordningen mellom 2008 og 2011 skaper variasjon i både eksponering og kapasitet over tid og sted, og estimerer effekten av fotlenke på tilbakefall i en forskjell-i-forskjeller og en instrumentvariabelanalyse. Resultatene viser at innføringen av fotlenkeordningen *ikke* førte til noen endringer i tilbakefallsintensiteten og -alvorlighetsgraden, men at tilbakefallsraten målt etter to år falt med 2.38 prosentpoeng. Dette tilsvarer en nedgang på ca. 10 prosent i pilotfylkene, noe som igjen innebærer omlag 19 prosent blant de som faktisk soner på fotlenke. Det finnes mange ulike mekanismer som kan forklare denne nedgangen, men analysen vår er ikke egnet til å utforske disse videre.

Det finnes to viktige forbehold ved denne analysen, som innebærer at resultatene må tolkes med en viss grad av forsiktighet. For det første er tilbakefallstrendene noe usystematiske i både pilotfylkene og sammenligningsfylkene, og for det andre finner vi tegn til at dommere i pilotfylkene har endret atferden sin etter fotlenkeinnføringen. Dette gjør det vanskelig å utelukke at resultatene påvirkes av andre strafferettslige forhold enn fotlenkeordningen i seg selv.

[1] Ordningen åpnet også for å avslutte lengre fengselsstraffer med fotlenke (såkalt delgjennomføring), men her fokuserer vi kun på helgjennomføring.

1 Introduction

The desire and need to reduce prison populations and correctional costs has led questions of non-custodial alternatives to "traditional" imprisonment to the forefront of criminal justice debates (see e.g. Tonry, 1998; Van Kalmthout, 2012; Villettaz, Gilliéron and Killias, 2015). In addition to limiting the direct fiscal costs of large prison systems, an increased adoption of non-custodial sanctions holds the potential to reduce the substantial collateral consequences of incarceration that impact offenders, families and communities alike. Incarceration has repeatedly been shown to be detrimentally associated with e.g. union stability, employment relations, income, health, and children's development (cf. Hagan and Dinovitzer, 1999; Lopoo and Western, 2005; Massoglia, Remster and King, 2011; Schnittker and John, 2007; Wakefield and Wildeman, 2013; Western, 2006; Western, Kling and Weiman, 2001) – suggesting that the punishment of imprisonment does not end at release.[2] Opportunity structures as well as formal and informal social ties are key aspects of criminological theories addressing reentry and desistance (Agnew, 1992; 2001; Laub and Sampson, 2001; Sampson and Laub, 1993), and one might therefore argue that non-custodial alternatives to prison can improve the convict's prospects of a crime-free life.[3]

In spite of a long tradition for implementing and evaluating non-custodial sanctions, as well as strong theoretical foundations for and a widespread academic belief in the superiority of non-custodial sanctions in promoting convicts' desistance from crime, the empirical evidence of such a causal relationship is still inconclusive. The main problem, of course, is that convicts serving non-custodial sanctions do so for a reason – typically that they are convicted for a less severe crime or that they are regarded more likely to desist from crime in the future. Because it is difficult to fully account for such selection on unobservables in empirical studies, estimates of the beneficial causal effect of non-custodial sanctions will tend to be upward biased. In line with this, Villettaz et al. (2015) conclude that recidivism is lower for non-custodial sanctions in most comparisons, although this conclusion is less clear in methodologically more reliable studies (e.g. controlled and natural experiments) than in quasi-experiments. They highlight that small sample sizes along with insufficient control of pre-intervention differences remains prominent challenges, and that more research is needed in order to draw general causal conclusions.

[2] Note that the negative relationship between imprisonment and various outcomes can be a result of selection as well as by causality; see e.g. Bhuller et al. (2016).

[3] Though it may reduce the convict's likelihood of recidivism (i.e. specific deterrence), non-custodial sanctions may reduce general deterrence.

One important source of information on the effectiveness of non-custodial sentences can be found in the introduction and expansion of so-called electronic monitoring[4] (EM) programs over the last couple of decades. Currently a part of the penal system in most western countries (Renzema and Mayo-Wilson, 2005), EM has been used to replace custodial as well as non-custodial sanctions (such as community service) and in some cases also to widen the "correctional net" by being implemented as a new sentencing option (cf. Bales et al., 2010; Black and Smith, 2003; Renzema and Mayo-Wilson, 2005). Usually based on either global positioning system (GPS) or radio frequency (RF) technology, EM programs that replace traditional incarceration typically monitor the offender's whereabouts while he or she serves the sentence at home and participates (to various degrees) in rehabilitative/integrating programs and activities. Although intuitively and theoretically promising, the empirical EM literature suffers from many of the same shortcomings as the general literature on non-custodial sanctions (e.g. Di Tella and Schargrodsky, 2013; Renzema and Mayo-Wilson, 2005; Whitfield, 2001).[5] The causal effect of EM on recidivism (and other outcomes) therefore remains uncertain.

With an aim of increasing our more general knowledge of the potential of non-custodial vs. custodial sanctions in reducing reoffending, this study focuses on EM as an alternative to incarceration and make use of a policy intervention that was implemented in six of Norway's nineteen counties from September 2008. This introduced "front-door" EM as an alternative way of serving unconditional prison sentences of no more than 120 days[6], and "back-door" EM as a way of ending longer unconditional prison sentences (Øster and Rokkan, 2015). Due to theoretical and pragmatic considerations it is only the front-door part of the program that is assessed in this paper.

The admission to EM was decided by the Norwegian Correctional Services (NCS) and not the courts, and front-door EM does hence represented a true, low-cost alternative to prison. The Norwegian intervention was gradually expanded within some of the pilot counties in 2010, before four more counties were enrolled in 2011.[7] Although EM was not randomized, the gradual nature of the implementation creates variation in EM exposure and capacity over both time and place – creating a quasi-experimental setting. We make use of this variation to estimate both difference-in-differences and instrumental variable models in order to elicit causal effects of EM on recidivism.

[4] Also referred to as "electronic tagging" (e.g. Marklund and Holmberg, 2009; Taylor and Ariel, 2012).

[5] In the EM literature randomized studies are very rare, and the only exception seems to be Killias et al. (2010).

[6] 120 days might seem very short in an international perspective; however, Norwegian prison sentences are relatively short. In 2014, 58 percent of everyone released from prison were released within 90 days or less, 87 percent within one year, and only 1 percent after 5 years or more (Norwegian Correction Service, 2014).

[7] Note that we refer to the counties that implemented EM as pilot, EM and treatment counties, and the counties that did not implement EM as non-pilot, non-EM and comparison counties.

We use population-wide data as provided by the NCS and Statistics Norway, and measure three different aspects of reoffending behavior for up to three years; the recidivism rate, the recidivism intensity and the recidivism severity. We also explore differences in the effect of EM in theoretically interesting subsamples.

Our results show that the mere correlation between EM and reoffending behavior is clear: Offenders serving on EM have lower recidivism rates and reoffend with less intensity and severity than offenders serving in prison. This is what we would expect, as the EM program is targeted at a low-risk segment of the prison population. We also find, however, that offenders serving in EM counties had a larger drop in recidivism rates when EM became available than did offenders serving in non-EM counties. We find similar results when using the available number of RF bands to instrument for serving on EM, and our results thus suggest that EM did causally reduce recidivism rates. Moreover, our subsample analysis shows that the estimates are strongest for offenders without previous prison experience and without recent unemployment spells. Although associated with statistical uncertainty, this may be taken to suggest that EM reduces recidivism by exempting the individual of the stigma and other collateral consequences associated with incarceration, as well as by enabling him or her to maintain employment relations. This is useful policy insight not only to the Norwegian system, but also to other countries using EM or other non-custodial sanctions as part of their penal system. We undertake a number of specification and robustness checks, and in general our findings seem robust although some concerns remain related to unstable pre-implementation trends and signs that more people are convicted to EM-qualifying sentences when EM is introduced.

The remainder of this article is structured in the following way. Section 2 briefly describes our theoretical expectations and previous research, and we outline several mechanisms that imply varying impacts of EM on recidivism across specific subsamples. Section 3 provides an overview of the Norwegian EM program, focusing on its implementation, contents and target population, and Section 4 describes our empirical strategy. Data sources, data management and sampling procedures are described in Section 5, before we present our results and robustness checks in Section 6. Finally, Section 7 includes a discussion of theoretical and policy implications, as well as limitations and suggestions for future research. Supplementary material is provided in five appendices.

2 Theoretical expectations and previous research

The use of electronic monitoring became commonplace in the US in the late 1980's and in Europe in the 1990's. The current literature on EM is quite extensive (see e.g. Di Tella and Schargrodsky, 2013, Killias et al., 2010, Renzema and Mayo-Wilson, 2005 for reviews), but the causal effects of EM on recidivism (and other outcomes) remain unclear. For instance, while Renzema and Mayo-Wilson's

review from 2005 concludes that "applications of EM as a tool for reducing crime [among moderate to high-risk offenders] are not supported by existing data" (Renzema and Mayo-Wilson, 2005, p. 215), more recent research using quasi-experimental designs have provided some support for reductions in recidivism when compared to custodial sentences (e.g. Di Tella and Schargrodsky, 2013; Jørgensen, 2011; Marklund and Holmberg, 2009). Also, in a randomized trial conducted in Switzerland in 2000, Killias et al. (2010) found that those assigned to EM reoffend less and are more likely to become married than those assigned to community service ($p<0.10$), suggesting that EM strengthens family ties and/or weakens unfavorable peer influence (cf. Huckelsby, 2008; Martin, Hanrahan and Bowers, 2009).

Although recent research seems to be suggestive of a beneficial effect of EM on recidivism, it is important to note that most studies come somewhat short of providing credibly unbiased estimates of the effects of EM. This is driven by issues of methodological rigorousness, small sample sizes and probably also what Renzema (2013:249) calls "the slippery nature of EM" – "EM" is not an exclusive label, and a quite diverse selection of programs fall within it. Two of the most important distinctions between EM programs are their so-called "net widening"[8] effect, as well as their inclusion of rehabilitative activities/treatments. Evaluations of EM programs in the other Scandinavian countries, which are similar to the Norwegian program, suggest that EM can indeed be an effective way to reduce recidivism. Sweden implemented front-door EM in the mid 1990's and back-door EM in 2001, and while we have not been able to locate any evaluations of the front-door implementation, Marklund and Holmberg (2009) find, using a matched control design, that those admitted to the a back-door program recidivate to a significantly less extent than those serving their entire sentence in prison. Denmark implemented a front-door EM program targeted at traffic offenders in 2005, which was extended to also include young offenders (under the age of 25) in 2006 before EM was made universally available in 2008. Comparing offenders with similar offending records and current offence characteristics, Jørgensen (2011) finds that the young offenders who were targeted in this second reform have significantly lower recidivism rates and intensities if their sentence started after EM became available to them. Moreover, two studies using regression discontinuity designs indicate that the Danish program has increased upper secondary education completion rates (Larsen, 2016) and decreased social welfare dependency[9] rates (Andersen and Andersen, 2014) among (male) offenders in the same age group. Together, these findings suggest that completing a sentence on EM can improve

[8] The term net-widening is commonly used in critical criminology to describe the process wherein correctional programs are implemented in ways that increase the total number of people who are subject to correctional control. An EM program is typically net-widening if it is implemented as a supplement rather than an alternative to existing programs (Blomberg and Mestre, 2014).

[9] The authors argue that social welfare dependency is almost always synonymous with unemployment in a social democratic welfare state such as in Denmark, and that the study hence can be interpreted as an investigation of the effect of electronic monitoring on unemployment (Andersen and Andersen, 2014:353).

the transition from prison life to outside life, and that serving on EM rather than in prison can reduce the stigma associated with a prison history.

Two existing studies from Norway show that those on EM have lower recidivism rates when compared to offenders serving short unconditional sentences in prison (Rasmussen et al., 2016) and to all NCS clients (c.f. Rokkan, 2012b; Graunbøl et al., 2010[10]). However, both these evaluations have relatively low internal validity (Sherman et al., 1997), and the selection (on unobservables) into EM makes it difficult to interpret the results causally. The causal effect of EM on recidivism in Norway therefore remains unknown. We address these obvious sources of selection bias in two ways. First, we apply a difference-in-differences approach contrasting the change in recidivism from before to after introduction of the EM program across similar convicts in counties offering and not offering EM. Second, we use the temporal expansion in the number of available ankle bands to instrument for being on EM. By using these transparent approaches to elicit credible causal effect estimates of EM we hope to complement the existing literature from the other Scandinavian countries as well as to provide useful insights to other countries that seek to implement EM as part of their non-custodial penal policies.

2.1 Serving time at home or in prison: Why should it matter for reoffending?

The main question we seek to answer in this study is whether serving a sentence on EM (i.e. outside prison) instead of in prison changes post-sentence reoffending behavior. Numerous possible reasons for this are present in the criminological literature, and in the following we structure these into seven main theoretical mechanisms. Although unable to empirically test the validity of each of these mechanisms separately, we argue that careful subsample analysis can provide some insight into their relative explanatory power. We suggest that age, previous imprisonment and social integration are three such moderator variables that could provide insights into *why* or *how* EM increases or reduces recidivism, and do therefore structure our subsample analysis accordingly.

2.1.1 Electronic monitoring reduces recidivism

Several arguments suggest that serving a sentence on EM instead of in prison will reduce recidivism. First, to serve ones sentence outside of prison makes it possible to avoid the negative consequences of serving (or having served) time behind bars. Such negative effects have received plentiful attention in the literature, and include psychological factors (Schnittker and John, 2007), the loss of (or lack of growth in) human capital (Lopoo and Western, 2005; Western, Kling and Weiman, 2001), the accumulation of criminal capital in the form of e.g. peer relations and skills, (see e.g. Bayer, Hjalmarson and Pozen, 2009; Morselli, 2009), as well as stigmatization by family, friends and employers post release (Grogger, 1995; Pager, 2003; LeBel, 2011). All of these are damaging effects

[10] See Kristoffersen (2013) for a summary in English.

that could be avoided if the offender serves his or her sentence outside prison, and we will in the following refer to this as *the protection mechanism*. If relevant, we expect to see stronger effects in two related groups of offenders: those who are young and those who have not previously been imprisoned. This is based on an assumption that EM will keep these offenders out of prison at an age and/or point in their criminal career when their social relations, networks and identities have not yet been altered by spending time behind bars. Jørgensen's (2011) and Andersen and Andersen's (2014) findings of a beneficial effect of EM for young offenders can suggest that such a mechanism is important.

Second, to serve ones sentence outside of prison makes it possible to maintain social bonds to family, friends, employers and other actors in "conventional" society during atonement. Advocates of social control theories (Hirschi, 1969; Samson and Laub, 1993, Laub and Sampson 2003) highlight these bonds – and especially the informal ones – as important prerequisites if the individual is to refrain from criminal activities. The potential for EM to reduce recidivism through the maintenance of such social bonds will in the following be referred to as *the integration mechanism*. If such a mechanism is relevant we would expect to see stronger effects among offenders who have strong social ties to family and/or employment to maintain, as suggested by the findings of Killias et al. (2010).

Third, and based on the knowledge that the offender voluntarily applies to serve on EM, one might argue that the opportunity to serve the sentence as desired can serve as a so-called "hook for change" (Giordano, Cernkovich and Rudolph, 2002). A "hook for change" is defined as a structural opportunity that – if seized by the individual – can serve as a catalyst for change in the offender's criminal trajectory.[11] As one can expect higher age and previous prison experience to proxy a stage in the life course and/or criminal career where the desire to change is more substantial than earlier on (cf. the phenomenon of aging out of crime), we expect this mechanism – which we refer to as the *hook for change mechanism* – to create stronger declines in recidivism among older offenders and offenders with at least one previous imprisonment.

Fourth and finally, and with a focus on the contents and context of the rehabilitative activities in EM, one additional crime-reducing mechanism can be outlined. To serve on EM requires the offender to follow a weekly schedule of work and other prosocial activities, which may – as compared to rehabilitative activities performed in prison – establish and teach the offender everyday skills (such as time management) that could be more directly transferable to post-sentence life. We refer to this as *the skill mechanisms*, and if relevant, we expect to see stronger effects among offenders who have weak

[11] This concept hence places more focus on agency than e.g. Sampson and Laub's (1993) concept of "turning points".

social ties prior to serving the sentence. This is based upon the assumption that EM provides such everyday skills and employment relationships to those who could benefit from them the most.

2.1.2 Electronic monitoring increases recidivism

Several arguments also suggest that serving on EM instead of in prison will increase recidivism. Based on deterrence theory or rational choice theory (e.g. Becker, 1968; Nagin, 1998), sentences need to be tough in order for them to work – as only harsher sentences will motivate or deter or the offender from reoffending.[12] Based on these theories, under what we will call *the deterrence mechanism*, EM might increase recidivism insofar as offenders perceive a sentence served on EM as less harsh than a sentence served in prison. A recent qualitative evaluation of the Norwegian program shows that serving on EM by no means is experienced as effortless (Rasmussen et al., 2016), but the *choice* to serve on EM still suggests that it is perceived preferable to imprisonment. We would expect the deterrence mechanism to create stronger effects among younger offenders and offenders without previous prison experience, as the presumably more lenient character of the EM program is experienced at a point in time when their benchmark for (re)offending costs is yet to be formed. Moreover, we expect to see larger effects among offenders with strong social ties given that EM can make the atonement so similar to everyday life that its specific deterring effect is negligible.

Second, to serve on EM can increase recidivism because it enables the co-occurrence of capable offenders and criminogenic settings (and possibly also former co-offenders) more than would a sentence served in prison (see e.g. Wikström, 2004). While it is indeed possible to commit crimes while in prison, and while the activity requirement in the EM program ensures that the offender does not roam around freely, one can argue that the opportunities to commit crimes nonetheless are more plentiful on EM. Relatedly, to serve on EM blurs the boundaries between the work/family sphere and the criminal sphere more than would an incarceration, and this has been shown to complicate the transition back to everyday life (e.g. Sørensen and Kyvsgaard, 2009). We will in the following refer to this as *the situational mechanism*, and we expect it to create stronger increases in recidivism among offenders with previous imprisonments (i.e. offenders who have already recidivated at least once in the environment in which the sentence on EM is served) and offenders who are married or employed prior to atonement (i.e. offenders whose separation between criminal life and conventional life can be blurred by EM).

[12] The idea that tough sentencing reduces reoffending on the individual level is in the literature referred to as *specific deterrence*, while the deterring effect on the public in general is referred to as *general deterrence*. Although we focus on the specific deterrence effect in our argument, it is important to note that the introduction of EM could increase recidivism also among offenders serving in prison or in the public in general insofar as it reduce the expected costs of offending and hereby lower general deterrence.

Finally, and based on the everyday nature of the rehabilitative contents of the EM program, to serve on EM might create feelings of alienation, frustration and distrust among offenders who struggle or fail to comply with the contents of EM (Agnew , 1992; 2001). Returns to prison due to technical violations are rare (Øster and Rokkan, 2012), but that does not mean that the pressure of "ordinariness" while on EM can be alienating to some. This might in turn increase risks of social deviance and/or reoffending, and we hence refer to this as *the strain mechanism*. Based on the assumption that strain is something that builds up over time, we expect this mechanism to manifest itself through stronger increases in recidivism among older offenders, offenders with previous imprisonment and/or offenders with weak social integration, as these groups are likely to have struggled with similar issues also in the past.

2.1.3 Theoretical expectations: Summary

We have summarized all seven mechanisms, the direction of their anticipated effect on recidivism as well as their expected validation in the three subsamples in Table 1 below.[13] For instance, we expect the protection mechanism to reduce recidivism, and this effect to be either only present or stronger (i.e. more negative) among young offenders and offenders without previous imprisonments. It is worth noticing that the same aspects of EM can be expected to produce quite different results within the same group, depending on the mechanism. For instance, the very aspects of EM that are expected to be crime-reducing for adult offenders in the hook for change mechanism are expected to be criminogenic in the strain mechanism.

Table 1: Theoretical mechanisms and expected subsample variations

Mechanism	Direction	Age		Prev. imprisonment		Social integration	
		Young	Old	None	One or more	Weak	Strong
Hook for change	-		X		X		
Integration	-						X
Protection	-	X		X			
Skill	-					X	
Deterrence	+	X		X			X
Situational	+				X		X
Strain	+		X		X	X	

We will return to this table in Section 7.1, where we discuss the theoretical implications of our findings further. We now move on to a more detailed description of the EM policy in Norway as well as the socio-political context in which it was implemented.

[13] It is also possible that the effect of an unconditional prison sentence is the same irrespective of whether it is served on EM or in prison, for example if living under constant risk of supervision (like EM) has the same effect on recidivism as being under constant supervision (like in prison); cf. the idea of Panopticon (Foucault, 1977).

3 The Norwegian context

All custodial and non-custodial sentences in Norway are carried out by The Norwegian Correctional Service (NCS), a government agency financed by the Ministry of Justice and Public Security. The activities of the NCS are built upon the so-called "normality principle"[14], holding reintegration and rehabilitation as core goals (Norwegian Correctional Service, 2015). These goals are pursued by providing education, work training, health care, financial guidance and various programs during atonement, as well as by collaborating with other welfare providers outside the correctional system.[15]

The NCS is structured into regional units with between three and five counties in each, and has a total prison capacity of just below 4000 places distributed between 43 prisons. High capacity demands are typically driving utilization rates closer to 100 percent (Norwegian Correctional Service, 2014, pp. 33-34), leaving Norway with an incarceration rate of approximately 70 per 100 000 (World Prison Brief, 2016). The challenges to meet the demands for prison capacity have been addressed – although admittedly not solved – by e.g. building new prisons, allowing for cell-sharing, implementing early release policies, increasing resources to probation and parole, renting capacity in prisons abroad, as well as implementing EM.

Approximately 7000 unconditional prison sentences to either prison or preventive detention[16] are initiated in Norwegian prisons each year, and Bhuller et al. (2016) document that prisoners in Norway have broadly similar observable characteristics as prisoners in other countries. However, there are two main factors that set the Norwegian system apart. First, most offenders serve relatively short prison sentences - the average conviction length in the period we consider was between 141 and 183 days (Statistics Norway, 2016c) – and probation laws enable sentences of 60 days or more to be terminated after 2/3 time as long as conditions of good behavior etc. are met. Second, Norway does – to quite a large extent – incarcerate traffic offenders. This is a particularly low risk population that falls within the target group of EM (Rokkan, 2012a), making the issue of selection particularly relevant. For instance, the practice of incarcerating traffic offenders is one of the main reasons why Norway can showcase the lowest recidivism rate among the Nordic countries (see Graunbøl et al., 2010).

[14] The normality principle can be summarized in three main points; that the punishment *is* the restriction of liberty, meaning that the offender maintains the same rights as all others who live in Norway; that no one shall serve sentences under stricter circumstances than necessary; and that the "inside life" should resemble "outside life" as much as possible (Norwegian Correctional Service, 2015).

[15] For a more thorough overview of the NCS's values and practices, see White paper 37 (2007/2008).

[16] Preventive detention is an indefinite sentence that may be given to dangerous, accountable offenders in order to ensure public safety. The sentence is rare, and only given to a handful offenders each year (Norwegian Correctional Service, 2012).

3.1 The EM implementation

EM was put forward as an alternative way of serving an unconditional prison sentence already in 1998 (White paper nr. 27 (1997-1998)), primarily prompted by the challenges of the prison system to meet the demands for prison capacity (Rokkan, 2012b). Moreover, EM was promoted as a key measure to ensure a positive development in the lives of (re)offenders, with a presumed potential to enhance the possibilities for rehabilitation and a crime-free life (Rokkan, 2012a).

The EM intervention was initially introduced as a pilot project in six[17] of Norway's nineteen counties in September 2008, leaving the remaining counties unaffected. The six participating counties were then given about 20 ankle bands each, before the capacity was expanded by 10 bands in two of the counties in 2010. In 2011 four more counties were enrolled, leaving the total capacity at 215 ankle bands until one more county was enrolled in October 2012 and a national implementation began in May 2014. The local implementation dates and capacities (i.e. the number of ankle bands) are summarized in Table 2, and the total capacity and number of offenders starting a sentence on EM in the treatment and comparison counties in Figure 1. Our data on EM (which include the 2008-2011 cohorts) enable us to use all three changes occurring before the 2012 extension, providing us with variation in EM eligibility (0/1) as well as EM capacity (number of bands per county) over both time and place.

Table 2: Stages in the Norwegian EM implementation (Rokkan 2012b)

County	Date	Capacity
Vestfold	September 1 2008	22
Oslo	September 22 2008	21
	January 1 2010	31
Oslo/Akershus[a]	October 1 2011	46
Hedmark	October 6 2008	22
Rogaland	October 20 2008	21
	January 1 2010	31
Troms	November 3 2008	22
Sogn og Fjordane	November 17 2008	22
Hordaland	April 1 2011	30
Agder	October 1 2011	20
Oppland	October 1 2012	20
National	May 1 2014	121

[a]: When implementing EM in 2011, Akershus went into a collaboration with the existing EM county in their region (Oslo). This meant that the overall capacity of the region increased, while the ankle bands had to be shared between two counties rather than one. This was also a common procedure when the program was extended to the whole country in 2014.

3.2 Admission to EM

In the Norwegian system electronic monitoring is not a sentence given by a judge; it is a way of serving an unconditional prison sentence. As such, all offenders who live[18] in a county offering EM and are to

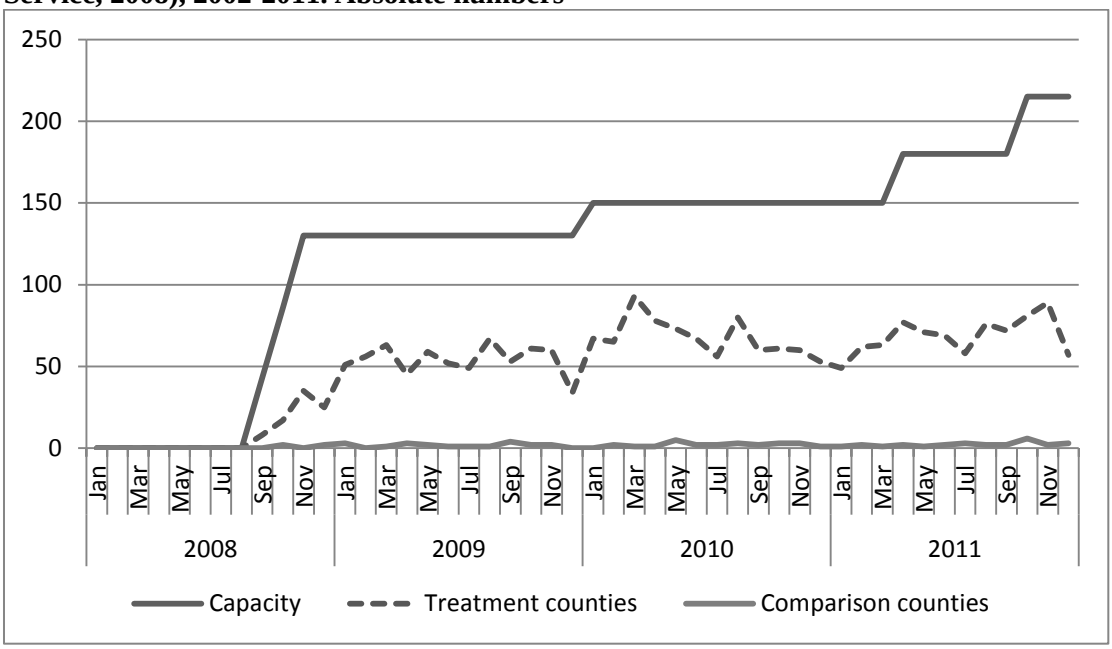
[17] One county in each of the Correctional Service's six regional units was included. The counties were selected by the Regional offices in cooperation with the regional labor unions.

[18] Note that it is the place of residence and not the place of offence that determines eligibility (Norwegian Correctional Service, 2013, § 7-9).

serve[19] an unconditional prison sentence of no more than four months are informed that they can apply to the NCS in order to serve their sentence on EM instead of in prison. Øster and Rokkan (2012) show that nearly 80 percent of this target group applies, and that just over half of the applicants are accepted (c.f. Rasmussen et al., 2016). The admission process comprises two main steps; one determining whether the applicant falls within the target group or not, and one determining whether he or she is deemed fit to serve the sentence outside of prison. This latter step is based on a personal aptitude assessment, which is carried out by the NCS staff (see Section 3.4 below and Norwegian Correctional Service (2013, § 7-9) for more details). This selection process implies that the offenders who are accepted to serve on EM differ, by intention of the admission process, from those not accepted, and these differences are likely to both be correlated with recidivism and to be inadequately captured by observable control variables. It is therefore crucial for causal interpretations to use variation from the geographical and temporal expansion of the program, and not whether an individual served on EM or not.

The total number of initiated sanctions on EM over time is displayed in Figure 1, along with the development in total capacity. For the number of initiated sanctions broken down by county, see Table 1.2 in Skardhamar (2013, p. 8).

Figure 1: Total number of ankle bands (Rokkan 2012b) and the number of initiated sanctions on front-door EM in treatment and comparison counties in the NCS data (Norwegian Correctional Service, 2008), 2002-2011. Absolute numbers



The implementation in September 2008 is clearly evident in that the number of sanctions on (front-door) EM as well as the total capacity is zero for all counties for all previous months. There is a

[19] Both those who were convicted after the program was introduced *and* those who were waiting to serve a prison sentence at the time of introduction, were eligible.

gradual increase in the number of initiated sanctions after the introduction, which stabilizes after about 6 months. As most of our analyses are based on calendar year, we therefore expect to see an effect of EM on recidivism in 2009 at the earliest. It is worth noticing that there are some offenders who live in the comparison counties that serve on EM (cf. Skardhamar, 2013), but that these numbers are low.[20] The middle, stipulated line represent the number of sanctions on EM that are *initiated* each month, but as the average time spent on EM is 29 days, we can expect this to be rather similar to the total number of offenders on EM within a given month. Hence, the full capacity is not being used, although we see an increase in the number of initiated sanctions as the capacity is expanded both in 2010 and 2011.[21] Rasmussen et al. (2016) find that the capacity utilization increased over time in all counties, but that there are persisting differences between counties in how many bands they have available. These differences are reflected in that only the counties with high capacity utilization increased their capacity in 2010, while the others did not (see Table 2).

3.3 Serving on EM

The EM program in Norway uses radio frequency technology, which is based on signals being sent from a locked ankle band to a stationary communication unit installed somewhere within the offender's home.[22] In addition to wearing the ankle band, employees of the NCS make unannounced visits to both the offender's home and workplace numerous times a week. The offender is obliged to provide urine samples for alcohol and drug tests, and alcohol and any kind of substance use is strictly forbidden (Norwegian Correctional Service, 2013).

In addition to the locked ankle band, the Norwegian EM program also includes a substantial focus on qualitative *contents*. To serve on EM is hence not comparable to a mere house arrest, and it is a requirement that the offender engages in three main activities; 1) between 15 and 40 hours of "activity", typically work outside of the home, per week,[23] 2) appointments as arranged by NCS, including drug programs etc., and 3) voluntary activities related to personal needs (e.g. grocery shopping, doctor's appointments etc.). These activities are structured into a weekly time schedule that is developed in collaboration between the offender and a correctional officer. Any failure to comply with the schedule

[20] In principle this could bias the recidivism in the comparison group, which in our case would entail that we slightly underestimate any beneficial effects of EM.

[21] Note that some bands are used by offenders who *end* their sentence on EM, i.e. by the back-door component of the program. The total capacity utilization is therefore slightly higher than seen in this figure.

[22] This technology ensures physical proximity between the sender and receiver at times during the day when the offender should be indoors, but it does not provide information about the exact whereabouts as would e.g. GPS technology. See e.g. Renzema and Mayo-Wilson (2005) for a discussion of different technologies used in EM systems, and Rokkan (2012a) for an overview of the debate on technology in Norway.

[23] "Activity" is broadly defined, and includes enrollment in school, education, participation in programs and volunteering, in addition to regular, paid work (Rokkan, 2012b). NCS assists offenders in finding suitable work if they cannot do so themselves.

offsets the electronic alarm, and is seen as a violation of the EM conditions. Based on the character of the violation, the offender might have to serve the remainder of the sentence in prison.[24]

3.4 Eligibility

The Norwegian EM program is largely inspired by those in Sweden and Denmark, where EM was implemented as an alternative to short-medium custodial sentences in 1994 and 2006, respectively (Rokkan, 2012b). In order to qualify for EM, all offenders (at least age 18[25]) must meet numerous formal conditions as well as passing a more discretionary and individual judgment conducted by the NCS.

The main formal requirement is that the *current unconditional prison sentence(s)* (for which the offender applies for EM) has to be no more than 4 months/120 days long in total. Moreover, the offender should, as a main rule, not be convicted of sexual crimes, violent crimes or crimes committed from the home or against any family members. The prison sentence could not be a result of violation of conditions in a previous sentence (e.g. violation of requirements in a parole, community service, drug program, etc.) (Norwegian Correctional Service, 2013).

More discretionary, the type and severity of any *previous offenses* should be taken into account when the NCS evaluates the application. Emphasis is placed on against whom the offenses were committed, whether any previous offense would disqualify for EM, as well as the time passed since the commitment of any previous offense (Norwegian Correctional Service, 2013).

There are also some formal requirements related to the offender's *living conditions* while serving on EM, including that the offender has a permanent work or residence permit in Norway, and that he or she is not formally deported from the country. Somewhat more discretionary, he or she must have a suitable place of residence in an EM county, though holiday houses, rental homes and occasionally also the homes of family or friends might be approved as long as they have electricity, running water and sufficient phone coverage for the EM technology to work. The written consent from any household member over the age of 18 is required.

Finally, the offender has to agree to the activity and time schedule described in Section 3.3. All technological equipment is provided by NCS, meaning that no financial responsibility is placed on the offender.

[24] Returns to prison are rare, and occurred in less than 5 percent of the sanctions served on EM between 2008 and 2012 (Øster and Rokkan, 2012).

[25] None of the formal requirements – only a more discretionary individual evaluation – were imposed for offenders who were younger than 18 at the time of application.

4 Empirical strategies

The goal of this analysis is to provide plausible estimates of causal effects of EM on recidivism, by means of quasi-experimental designs.[26] By intention of the program and by the evaluation process wherein the NCS staff undertakes informed and competent judgments about who to admit to EM, we expect offenders admitted to reoffend less than those rejected. While some of these differences may be adequately captured by observable control variables, crucial differences between offenders are likely based on unobservable, discretionary judgments that we cannot adequately capture by including controls. Hence, we will use two related quasi-experimental methods to try to credibly elicit causal effects of EM on recidivism: difference-in-differences and instrumental variables (Angrist and Pischke, 2009).

4.1 Difference-in-Differences analysis

The difference-in-differences (DD) estimator contrasts the change in recidivism from before to after the introduction of EM across a treatment and comparison group. The main idea behind this strategy is that changes in recidivism in the pilot counties that are *not* caused by the EM implementation can be adequately captured by contrasting these changes to the corresponding changes in a group of counties that did not implement EM. In its simplest form the DD-model would use one pre and one post period defined by the same date in both the treatment and comparison group, e.g. the September 1 2008 official implementation date. However, the current EM program went through three main changes in the period between 2008 and 2011, and to limit the analysis to the 2008 implementation would discard a lot of variation (and observations) that would increase the precision and power of our results. We therefore choose to expand the basic DD approach and apply the following DD model:

$$y_{i,t} = \alpha + \beta\, county_i + \tau\, time_t + \lambda\, treat X post_{i,t} + \mu\, X_i + \varepsilon_{i,t} \qquad (1)$$

The outcome variable ($y_{i,t}$) denotes recidivism for offender *i* 1-3 years after release. We allow for more flexibility than a traditional treat-dummy by including one dummy for each of the counties of residence ($county_i$). [27] We also control flexibly for time, and include dummies for calendar year (2002-2011) and calendar month (1-12), as well as their interaction ($time_t$). This allows us to account very flexibly for time trends in recidivism, both over years and by season. Mainly with the intention to improve precisions, we will also control for the exogenous variables sex, age when starting the sentence and conviction length (*X*). The error term $\varepsilon_{i,t}$ is assumed to have conditional mean zero.

[26] The ideal – but typically not available – design to estimate causal effects would be a random assignment of eligible offenders into a *treatment group* serving on EM and a *control group* serving in prison. The literature on quasi-experiments, to which we carefully align, does instead emphasize the advantages of relying on variation that can be transparently judged as exogenous or not, rather than applying traditional regression methods with control variables for estimation in such a non-experimental setting (Angrist and Pischke, 2009).

[27] Selection into the treatment counties is a key concern. Please see sensitivity analysis in Section 6.4.

The variable $treatXpost_{i,t}$ is set to 1 for offenders who lived in a county offering EM *and* who began the sentence after the county had started offering EM (zero otherwise). The parameter of interest λ thus captures the overall average effect on recidivism resulting from a county *offering* EM. This estimates the so-called intention to treat effect (ITT), which is the average effect of *offering* EM over both those who actually served on EM and those who still served in prison. Since we have little or no reason to expect any effect of offering EM on those who actually did not serve on EM, the ITT underestimates the effect of serving on EM on recidivism (we address the plausibility of this assumption more closely in Section 4.2).

4.1.1 Identifying assumptions

The main identifying assumption for the DD-approach is that the trends in recidivism would have been the same in the treatment and comparison counties had the treatment counties not introduced EM. There are several potential threats to this assumption. First, there may be unique features of the EM counties that made them preferred counties for the EM pilot, and insofar as these factors are also related to recidivism, the DD estimator may erroneously attribute any observed effects on recidivism to EM rather than to these other factors. We have not seen any such considerations being mentioned in official descriptions of the EM implementation (e.g. Øster og Rokkan, 2012), but we nonetheless explore the relevance of such concerns by checking for patterns in recidivism rates just before the introduction of EM.[28] Note that any secular trend would only introduce bias in our effect estimates to the extent that the temporal patterns were not also present in the comparison counties.

Second, and relatedly, there might have been other correctional changes occurring around the same time as the EM implementation that may also affect recidivism. EM counties might for instance start offering additional programs; or, the counties not offering EM may start offering something else instead. We are not aware of any such programs, and the funding of EM was "fresh" in the sense that it was added to the national budget, it did hence not distract resources from neither the offering nor the non-offering counties (White paper nr. 37 (2007/2008)). Relatedly, judges may change their behavior and issue more (or less) unconditional sentences after the EM implementation, in particular in "borderline" cases that would otherwise have been fit for e.g. community service. If so, the population of convicted offenders in the EM counties would comprise marginally less (or more) crime-prone offenders than in the counties without EM, possibly biasing our estimates. To explore the relevance of this, we use a version of Eq. (1) to estimate effects of EM on unconditional prison, conditional prison[29] and community service; see Section 6.4.2.

[28] Information on unmet demands on the prison system (e.g. waiting lists for the serving of sentences) would also be relevant, but this is unfortunately not available on the county level and can therefore not be explored.

[29] This is a prison sentence that is postponed for a given trial/probation period (typically 2 years). If the offender does not reoffend or break other conditions as set by the court during this period, the prison sentence is seen as served; if not, the remainder of the sentence must be served in prison. As such, a conditional prison sentence bears resemblance to probation in the US system.

Third, offenders might self-select into the counties offering EM (e.g. by moving) or into the post-period (e.g. by trying to postpone the serving due to knowledge of the upcoming EM introduction). To explore the possible relevance of such composition effects, we rerun all analyses based on the county of residence the year *before* EM was known to be introduced, hereby disabling such self-selection. This procedure would have the advantage of reducing bias due to self-selecting, but since it attributes some offenders to a county offering (/not offering) EM, it would attenuate any effect estimates. Moreover, we examine whether moving patterns in our sample suggest that people move to EM counties in order to qualify; see Section 6.4.

4.2 Instrumental variable analyses

Instrumental variable (IV) estimators rely on sources of exogenous variation in an endogenous explanatory variable (being on EM or not) to consistently estimate the effect of the explanatory variable on the outcome variable (recidivism). The main idea behind this method in our setting, is that being admitted to EM or not is endogenous at the individual level (admission at the discretion of the NCS officer), but that the offering and capacity of EM in the offenders' county provides an exogenous increase in these offenders' likelihood of actually serving on EM. In the following we will utilize the variation from the number of bands available in each county, hence including not only the expansion from zero but also later extensions in the number of ankle bands. Since this model utilizes more variation than the mere implementation (captured by the *treatXpost* parameter in model 1), we expect it to better predict EM and thus provide more precise estimates.[30] Under the assumption that the EM capacity does not affect recidivism in any other way than through EM, we can use the increase to consistently estimate an effect on recidivism of serving on EM.

The approach can be illustrated in by the following model, which is estimated using 2SLS:

$$EM_{i,t} = \alpha + \beta\, county_i + \tau\, time_t + \lambda\, bands_{i(c),t} + \mu\, X_i + \varepsilon_{i,t} \quad (2a)$$

$$y_{i,t} = a + b\, \widehat{EM}_{i,t} + c\, county_i + d\, time_t + e\, X_i + f_{i,t} \quad (2b)$$

In the first stage (2a) whether the offender serves on EM (1) or not (0) is estimated using variation from the introduction and expansion of EM, where $bands_{i(c),t}$ specifies the number of bands available in the county of the offender at time *t*. In the second stage (2b) the effect of serving on EM on recidivism is estimated using predicted – not actual – EM, i.e. $\widehat{EM}$ from the first step. The parameter of interest is now b, which captures the effect of actually serving on EM on recidivism. This estimates the so-called local average treatment effect (LATE), which is the average effect of *serving* on EM (instead of

[30] We have also run the IV analysis using the implementation (*treatXpost)* as instrument in the first step, and this does – as expected – decrease precision somewhat but have no substantial implications for the qualitative results.

prison) on recidivism for those who are moved from prison to EM by the introduction and expansion of the EM program.

4.2.1 Identifying assumptions

The IV requires a valid first stage, mainly meaning that the number of bands (including the DD case where bands go from zero to a positive number) significantly predicts the probability of serving on EM. This can be tested in the data, and F-tests from all first stage models are included in Appendix D. The main identifying assumption, which needs to be assumed and *cannot* be tested in the data, is the so-called "exclusion restriction", i.e. that the offering of EM has no independent effect on recidivism.[31] Specifically, this means that the variable *bands* in Eq. (2a) can be excluded from Eq. (2b).

There are several reasons to be concerned with the validity of the exclusion restriction in our setting. First, if EM with associated activity requirements (work, education) affects those in prison (i.e. in non EM-counties), e.g. if it makes it harder for them to obtain work after release, the EM program may also raise their subsequent crime. This seems, however, far-fetched in our setting. Second, the number of bands may be correlated with underlying factors, which are inadequately captured by the observables in Eq. (2a), implying biased estimates. Though it is hard to see what underlying factors this might be, this cannot be fully ruled out. Third, if EM is perceived as less punitive than prison and hereby reduces general deterrence, recidivism may increase as EM is introduced and expanded. In isolation this may contribute to a bias in our estimates of effects of serving on EM on crime. There is no way we can explore this further, but general deterrence effects – though prominent in theory – are notoriously hard to document credibly in empirical work (Telle, 2013). Relatedly, and as mentioned above, if judges become more prone to unconditional sentences when knowing that EM is available, this can increase general deterrence. We will explore changes in sentencing practices in Section 6.4.2.

4.3 Standard Errors

Traditional estimates of standard errors assume that the error terms in Equations (1) and (2) are independent across observations. This assumption may not hold in our situation, as outlined by e.g. Cameron and Miller (2015). In the following we will therefore allow error terms to be correlated

[31] Consistent estimates require the fulfilment of two other assumptions; Monotonicity and the Stable Unit Treatment Value Assumption (SUTVA). Monotonicity, i.e. that there should be no "defiers" in the EM group, ensure that the sign of the effect of the EM program on actual EM is the same for all observations. In our setting it seems innocent to assume that the introduction of EM or the increase in capacity did not *decrease* the likelihood of serving on EM for any offenders. SUTVA requires that the treatment status of one unit does not affect the potential outcomes of other units, and that the treatments for all units are comparable. The treatment should be equal for all units in that they all get the same number of days on EM as they should have served in prison, and interference between offenders should be minimized by the treatment being delivered without offenders being in touch with each other. We hence see it as unlikely that these assumptions compromise our results.

within counties and years (cluster on county and year), and we will also undertake some checks of robustness on how to cluster (see Section 6.4).

5 Data

5.1 Data sources

The data for this analysis are retrieved from NCS's own databases (Norwegian Correctional Service, 2008), Statistics Norway's crime statistics (Statistics Norway, 2015, 2016a, 2016b), and the event history database FD-trygd (Akselsen, Lien and Siverstøl, 2007). Information from the various sources are combined using the unique personal identification number (PIN) issued to all Norwegian residents and immigrants with permanent residency. This leaves us with a population-wide dataset covering all sanctioned offenders apart from e.g. tourists, asylum seekers and illegal immigrants without a valid PIN.[32] The data management process is done in three main steps.

First, the NCS's databases on custodial and non-custodial sanctions (Norwegian Correctional Service, 2008) are used to define our sample. We include all sentence records[33] from unconditional prison sentences that are served either in prison or on EM, as long as they have a starting date between January 1 2002 and December 31 2011 (which is the latest date available to us in our data). This leaves us with a total of 58 694 records, whereof 2 448 have been served on EM. For these records we keep information on EM status (whether the sentence has been serving on EM or in prison), the type of offence (violence, drug, etc.), the start and ending dates of the sentence, as well as the conviction length. As the reoffence data (Statistics Norway, 2015) are available until December 31 2013, the entire sample except for those who start their sentence in November and December 2011 (N=105) is followed for at least 24 months. We choose to start the follow-up in 2002 to allow for a sufficient account of pre-implementation trends, while at the same time avoiding a break in the crime statistics from 2001 to 2002.

Second, we use register data as maintained by Statistics Norway to restrict the NCS data and define our main analytic sample. Crime data (Statistics Norway, 2015, 2016a, 2016b) are used to account for previous offending and sanctioning. Moreover, demographic and socioeconomic information is retrieved from the population wide database FD-trygd (see Akselsen, Lien and Siverstøl, 2007) to account for eligibility criteria that are related to e.g. age and county of residence. We also use this

[32] People without a permanent residence permit do not qualify for EM, and this should hence be unproblematic.
[33] Keeping all records mean that a few (re)offenders are represented in the data more than once; in the final analytic sample, 97 percent of the offenders are repeated once and 3 percent twice. This introduces dependence across observations for the same individual, a problem that is to some extent handled by our clustering on county and year. Nevertheless, we have also done the main analysis clustering on individual, yielding virtually identical results.

database to collect information for control variables and sub-sample analyses, like sex, marital status and employment.

Third, the crime statistic on investigated offences (Statistics Norway, 2015) is used to measure reoffending. This statistic includes one record for each committed offence with a known primary suspect at the end of the police investigation, providing us with a complete picture of registered (re)offending defined as the commitment of a new crime (and not a technical violation etc.). We prefer these data (as opposed to e.g. (re)imprisonment data) as they, first, capture less severe crime and, second, include information on the date of (re)offence, which allows us to accurately measure the timing and extent of reoffending. However, it is important to remember that this is a front-end recidivism measure that will yield higher recidivism rates than more back-end measures such as reconvictions or reimprisonments.

5.2 Defining variables

This analysis relies on a wide set of variables, and we will in the following differentiate between outcome, assignment, moderator and control variables.

5.2.1 Outcome variables

The analysis seeks to estimate the effects of EM on recidivism, and while recidivism rates are the most common measure in existing research, it is often argued that this measure only captures a small part of a complicated and diverse phenomenon (Farrington and Davies, 2007; Harris, Lockwood and Mengers, 2009). For instance, if fewer people reoffend (and the recidivism rate goes down), it is hard to argue for beneficial effects on crime and reintegration if those who still reoffended commit more crimes, more serious crimes, and/or more harmful crimes. To provide a more nuanced account of reoffending behavior, we therefore measure recidivism by means of three various outcomes that are pivotal in the criminal career paradigm (Piquero, Farrington and Blumstein, 2003). All outcome variables are based on the statistics on investigated offences (Statistics Norway, 2015), and are defined as follows.

First, we measure the *recidivism rate* as the proportion of the sample that commits at least one new offence within the follow-up period. This measure hence captures the criminal propensity in the sample. For those who *do* reoffend we include two additional measures. The *recidivism intensity* is measured as the total number of offences committed within the follow-up, giving an indication of the activity level of the offender. This measure is winsorized at the 95th percentile due to outliers, i.e. more extreme values are replaced by the value at the 95^{th} percentile. Finally, the *recidivism severity* is measured as the most sever offence committed within the follow-up period. This measure is based on a severity indicator in the data on investigated offences, which is based on the maximum sentences in

the Criminal Code. It should be noted that this variable holds no direct interpretation, and only serves as a proxy. We choose to use the most sever offense (as opposed to e.g. the mean severity of all offences) to avoid that numerous petty crimes deflates the individual mean. As such, we attempt to measure the criminal capacity of the offender. In order to provide detailed accounts of reoffending, and to capture any tempo effects in our results, we measure reoffending for up to three years after release.[34] However, only the counties that implemented EM in 2008 will contribute to the 3-year measurements, as data limitations restrict us from following the 2011 counties for more than 2 years. Hence, and as in most other recidivism studies (cf. Andersen and Skardhamar, 2015; Armstrong and McNeill, 2012; Farrington and Davies, 2007), we consider the 1- and 2- year measures to be our main outcomes. Descriptive statistics of our outcome variables are summarized in Table 3.

5.2.2 Assignment variables

The assignment variables are used to define our main analytic sample and determine the treatment status of each sentence record (i.e. whether it belongs in the treatment or comparison group, and in the pre or post period).

County denotes the county in which the offender had his or her permanent address (as registered in the National Population Register) as of Jan 1 in the year the serving of the sentence was started. This information is used to determine EM eligibility/treatment status (treat=1).

Time variables (calendar *month* and calendar *year*) are based on the date of starting the sentence.[35] These variables are used to assign sentences to EM eligibility, and *post* is set to 1 if the sentence is started on or after the date EM was implemented in a given county. Moreover, the calendar year and calendar month of starting the sentence, as well as their interaction (*monthXyear*), are included as dummies in order to control for time trends in recidivism over years and by season.

Capacity denotes the total number of ankle bands that are available in the offender's county of residence at his or her date of conviction. This capture both the transition from 0 to 1 (i.e. the implementation) and later extensions, and is included as a continuous[36] instrumental variable in the IV analysis. Note that ankle bands cannot be transferred between counties unless in the rare occasions where they are located within the same NCS region (see Table 2).

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[34] Another option could be to start the clock at the starting date of the sentence, but few(er) offences (none in our sample) is committed while serving a sentence. 130 reoffences (whereof 2 are committed by someone on EM) are committed *on* the ending date, and while these theoretically could have been committed while in custody/under supervision, we see it as unlikely. These offences are captured by the recidivism measures we currently apply.

[35] We use the starting date rather than the conviction date to define the post-variable because this correctly ascribes offenders who had previously been convicted and were waiting to serve their sentence at the time of implementation to the post period.

[36] We tested other function forms as well; however, this did not substantially improve the model fit or alter the overall results.

The *conviction length* (in days) is drawn from the NCS data on custodial sentences, while we use the *actual sentence length* (in days) for those on EM due to data limitations. This is not ideal, as the NCS guidelines are based on conviction length, but the practical implications for our final sample are negligible.[37]

The *age (in years) when starting the sentence* is calculated using the date of birth from the National Population Register and the starting date of the sentence in the NCS data. The EM guidelines are based on the age when *applying* for EM, but the application date is not available in our data. Short waiting time should limit the skewness in this variable. We use this variable to create the dummy *under 18*, which is set to 1 if the offender has not turned 18 at the time of starting the serving.

The type of crime for which the offender is currently convicted is captured in two variables based on the offence category in the NCS data; *Violent* is set to 1 if the current offence is categorized as murder, attempted murder, violence, common or aggravated assault; and *Sexual* is set to 1 if the current offence is categorized as a rape, incest or sexual offence.

The type(s) of crime(s) for which the offender has previously been sanctioned is retrieved from the statistic on criminal sanctions (Statistics Norway, 2016b). We use all court convictions with a conviction date prior to the current conviction date, and use information on the main offence in the conviction to set *previous violent* to 1 if the conviction was for a violent offence and *previous sexual* to 1 if the conviction was for a sexual offence.
Information on previous imprisonments is retrieved from the statistics on imprisonments (Statistics Norway 2016a). We use all releases from both convictions and custody that have a release date prior to the current starting date, and set the dummy variable *released24* to 1 if the offender has been released (from either conviction or custody) within 24 months of starting the current sentence.

The NCS guidelines states that the offender should not have charges for new crimes cf. the Criminal Procedure Act § 822 (Norwegian Correctional Service, 2013, § 7-2). This event is proxied by the dummy variable *recent charges*, which is set to 1 if there are any records in the data of investigated offences (Statistics Norway, 2015) with an offence date between the current offence date and the starting date of the current sentence.

Finally, the dummy variable *during atonement* is set to 1 if there are any records in the data of investigated offences (Statistics Norway, 2015) with an offence date during previous atonements as

[37] The conviction and sentence length should only differ for those convicted to *exactly* 60 days (135 EM records and 1838 prison records), due to this being the threshold for early release.

registered in the data on criminal sanctions (Statistics Norway, 2016b). This variable is used to capture previous offending while serving a sentence, as described in Norwegian Correctional Service (2013, §7-3).

5.2.3 Moderator variables

The moderator variables are used in our subsample analysis in order to assess the relevance of the mechanisms outlined in Section 2. As we recall, these should capture the offender's age, social integration prior to starting the sentence, as well as previous prison experience.

The offender's *age when starting the sentence* is used to generate two dummy variables which form the basis of two separate subsample analyses. *Max 25* is set to 1 if he or she is 25 years or younger and 0 otherwise, and *over 50* is set to 1 if the offender is 51 years or older when starting the sentence and to 0 otherwise. The groups in the two dummies are hence overlapping, and the goal is to explore theoretically interesting patterns in these subgroups (cf. Andersen and Andersen, 2014; Jørgensen, 2011; Larsen, 2016)

Social integration is proxied by two well-known sources of social control; labor market attachment and marital status (cf. Hirschi, 1969; Sampson and Laub, 1993; Laub and Sampson, 2003). Labor market attachment is captured using two different measures, both of which are based on information gathered the year prior to starting the sentence in order to avoid endogeneity. First, we set the dichotomous variable *unemployed* to 1 is the offender was registered as unemployed for at least 1 month during this year.[38] Second, we set the variable *low income* to 1 if the income from paid work (including wages and capital income) is less than 1 time the base rate of the National Insurance Scheme.[39] The union status is captured in the dummy variable *married*, which is set to 1 if the offender is registered as married as of Jan 1 the year of starting the sentence and 0 otherwise. We do not have information on cohabitation, and single, divorced and cohabiting offenders are hence included in this comparison category.

The dummy variable *previously imprisoned* is set to 1 if there is any release record from either a conviction and/or custody in the data on imprisonments (Statistics Norway, 2016a) that have a release date prior to the current starting date. We use all prison spells due to the assumptions of the protection mechanism.

[38] We have tested whether there are variations depending on the number of months, but the significant importance seems to be between 0 and 1 month.

[39] This is an annually updated, inflation adjusted amount that is used to i.a. determine social benefit eligibility and calculate pensions. See e.g. Havnes and Mogstad (2011) for another example of using this amount to determine relative earnings.

5.2.4 Control variables

With the main intention to improve precisions in our main models we use three control variables; the offenders *sex* (male=1), the *age at starting serving the sentence* and the *conviction length* (in days). The latter two variables are introduced as sets of dummies to avoid imposing restrictive functional forms.

Additionally, we include four more control variables in our "naïve regression" (Section 6.2). The *age at first offence* (as registered in the data on investigated offences) is included as a continuous variable with quadratic terms. *Previously imprisoned*, *low income* and *married* are included as defined above.

5.2.5 Sensitivity/robustness variables

In our sensitivity and robustness checks we make use of additional variables. *treatXpost-1* is a DD variable based on the county of residence the year *before* starting the sentence, which we use to assess possibly selective relocation of residency. In our analysis of judges'/courts' practices we use four outcome variables; *max120* is set to 1 if the offender is convicted to no more than 120 days of unconditional prison (either as the only sentence or a combination sentence), *max60* is set to 1 if the offender is convicted to no more than 60 days of unconditional prison (either as the only sentence or a combination sentence), *community service* is set to 1 if the offender is convicted to community service, and *conditional* is set to 1 if the offender is convicted to a conditional prison sentence. In these models we use a DD-variable (*treatXpost*) based on the date of conviction.

5.3 Defining main analytic sample

The goal of our sampling process is to create treatment and comparison groups that are as similar as possible, in which all offenders would qualify for EM had EM been implemented. We start with all unconditional prison sentences served either in prison (N=56 246) or on EM (N=2448) with a starting date between January 1 2002 and December 31 2011 (N=58 694). The only clear-cut requirement in the official guidelines of the NCS (Norwegian Correctional Service, 2013) is that of a conviction length of no more than 120 days/4 months of unconditional prison, but applying only this criterion we are left with a total number of about 6000 sentences served in the treatment counties after EM was implemented – whereof only 39 percent were served on EM. It is therefore clear that the remaining, more discretionary requirement – or variables correlated with them – have been important in the assignment process as well, and we use these guidelines to further restrict our sample in the following way.

First, we operationalize as many of the guidelines[40] as our data allows (see Section 5.2). Second, we test the distribution of our operationalizations in the (actual) EM sample to explore how the guidelines have been practically executed by NCS staff. In doing so, we hope to move toward a sample of those who had a realistic chance of being admitted to EM. We find that the practices of NCS staff do indeed differ somewhat from what we could expect based on the guidelines, and we hence adapt our sample criteria accordingly (see Table 10 in Appendix A for a further description). In sum, we end up excluding offenders who are convicted to more than *60* days of unconditional prison; who are currently convicted for violent or sexual offenders, or who has been so in the past; who are less than 18 years at the time of starting the sentence; who committed new offences between the time of conviction and starting the current sentence; who have reoffended during atonements in the past; and who have been released from prison within 24 months of the current conviction. We obviously impose the exact same requirements on all offenders regardless of their county of residence, EM status etc., and are left with a total of 24 329 sentence records in our main analytic sample, whereof 1857 are served on EM. This equals 40 and 73 percent of the initial prison and EM sample, respectively.

This analytic sample is described in Appendix B, and reassuringly, this description suggests that our sampling procedure has been successful in creating a treatment and comparison group that is close to identical on all control and moderator variables. Divergence is primarily found in the moderator variables, with the treatment group having a slightly lower proportion of offenders who were unemployed or had low income from paid work the year before starting the sentence (32 vs. 35 and 33 vs. 35 percent, respectively), and a higher proportion of offenders who were married (16 vs. 13 percent) and had previously been imprisoned (8 vs. 7 percent). Similarly, the change in the treatment group from the pre to the post period is negligible on all variables but the moderator variables, with a substantially higher proportion of offenders being unemployed (28.6 vs. 47.6 percent) and having previous prison records (6.4 and 12.7 percent) in the post period. We see it as likely that most of these differences are driven by annual trends in e.g. labor market conditions that will be netted out by the time controls.

6 Results

Our results are presented in three main subsections; one for descriptive patterns, one for naïve regressions, and one subsection for the effect estimates relying on the quasi-experimental methods. [41] Sensitivity analyses and robustness checks are then performed in a fourth subsection.

[40] A translation of all guidelines (which currently are available only in Norwegian) and our operationalizations (or the lack thereof) are summarized in Table 9 in Appendix A.

[41] Note that we, due to a large number of dummy variables (e.g. 142 for time (year, month and year*month)) and a high number of models (3 outcomes*3 follow-up lengths for each model) have chosen not to include esimates for the control variables in the manuscript. These are available from the authors upon request.

6.1 Descriptive patterns

In order to have a point of reference when interpreting the effect estimates, we summarize the mean values of all recidivism outcomes in Table 3 below.

Table 3: Descriptive statistics, recidivism measures. Main analytic sample (N=24 329)

	Recidivism rate			Recidivism intensity[a]			Recidivism severity		
	1 year	2 years	3 years	1 year	2 years	3 years	1 year	2 years	3 years
Min/max	0/1	0/1	0/1	1/6	1/9	1/11	10/201	10/201	10/201
Mean	0.1509	0.2428	0.3227	2.10	2.59	2.98	61.66	63.27	64.09
S. D.	0.3079	0.4290	0.4675	1.50	2.20	2.75	27.01	27.71	27.90
N	**24329**	**24224**	**22034**	**3671**	**5895**	**7280**	**3671**	**5895**	**7280**

[a]: Before winsorization the maximum value was 299, and the mean was 2.42, 2.94, and 3.42 after 1, 2 and 3 years, respectively.

As we can see, the recidivism rate for our study sample ranges from 15 percent after one year to 32 percent after 3 years. These numbers are substantially lower than what has previously been found for Norwegian releasees (Andersen and Skardhamar, 2015), reflecting the low-risk character of this sample. Out of all the offenders who committed a new offence during the follow up, the mean number of reoffences ranges from 2 new offences after one year to nearly 3 offences after 3 years. The severity measure holds no direct interpretation, but we see that the overall trend is that of increased severity as we expand the follow-up period. There are some variations between the treatment and the comparison group in their reoffending behavior, with the recidivism rates being slightly higher and the recidivism severity slightly lower in the treatment counties (see Table 11 in Appendix B).

To explore one of the key underlying assumptions of our analytical strategy, namely that trends in recidivism are (and would have continued to be) similar in the treatment and comparison counties in the absence of EM, we move on to estimating recidivism in the two groups around the time of implementation. In an ideal scenario we would observe smooth and consistent annual recidivism trends that changed (or did not change) around the time of implementation. However, the recidivism trends in our data (see Appendix C) are quite "noisy" – especially when broken down into shorter time periods (results not shown). In the following we therefore account for some (but admittedly not all) of this noise by assessing recidivism trends on a relative (not calendar) time scale, aggregating rates by year, and controlling for time trends (calendar year and month of starting the sentence) as well as the sex and age composition of the sample.

In creating the relative time scale we separate between the three changes occurring in the EM program between 2008 and 2011: The implementation in six counties in the fall of 2008, the expansion in

capacity in two counties in 2010, and the implementation on four more counties in 2011.[42] We then estimate recidivism outcomes as measured after 1-3 years for everyone who started their sentence up to 7 years before and 3 years after the implementation in a given county (i.e. for as long as our data allow), combining records and presenting average[43] trends. This structure implies that some records in the original data contribute to more than one implementation/change; for instance, those who start their sentence on Feb 1 2009 will be in year 1 for the 2008 implementation, -1 for the 2010 extension and -3 for the 2011 implementation. The N is therefore larger here than is our main analyses. It is worth noticing that the estimates for period +3 must be interpreted with some caution, as data limitations entail that these are solely driven by those starting their sentence 3 years after the 2008 implementation. Please see Table 13 in Appendix C for an overview of how the different implementations contribute to this analysis.

Due to space limitations the nine figures of recidivism trends are included in Appendix C. As we see from these figures, there seem – despite the fairly noisy pre-implementation trends – to be a drop in the recidivism rates in the EM counties after the implementation that exceeds those of the comparison counties. However, the intensity and severity measures show an increase in the treatment group from the year before to the year after the implementation that exceeds that in the comparison group, before the estimates drop in the second and third year after the implementation. In sum, the descriptive figures suggest that there was a decline in recidivism rates in the EM counties after EM was introduced, while the patterns for recidivism intensity and severity are more mixed. The trends are not as clear as we would prefer, which makes it harder to make certain conclusions based on mere visual presentations. Nonetheless, these figures all suggest that any effect of EM on recidivism is likely driven by changes in averages over time rather than sharp changes from one year to another.

6.2 Naïve regressions

To empirically examine the relationship between (actually) serving on EM and reoffending, we perform a regular OLS regression for each outcome variable and control for important observable characteristics. We use a dichotomous (EM) variable representing those who *actually* served on EM (N= 1857). The EM estimates are summarized in Table 4 below.

[42] As the implementation/expansion dates vary somewhat between treatment counties on both 2008 and 2011, we set the "implementation date" (i.e. time 0) in the comparison counties to the most frequent date in the treatment counties: September 22 2008, January 1 2010 and October 1 2011.

[43] The time trends vary somewhat for the three changes (results not shown), but the overall picture is similar.

Table 4: Estimates from naïve regression of recidivism on actually serving on EM or not. Simple and extended models. Main analytic sample (N=24 329)

	Simple model		Extended model		N
	b	SE	b	SE	
Rate					
1 year	-0.0526*	0.0086	-0.0488*	0.0095	24 329
2 years	-0.0805*	0.0104	-0.0606*	0.0113	24 224
3 years	-0.0918*	0.0138	-0.0655*	0.0148	22 034
Intensity					
1 year	-0.1790	0.1117	-0.0852	0.1191	3671
2 years	-0.2686*	0.1287	-0.1333	0.1357	5895
3 years	-0.4992*	0.1465	-0.2194	0.1538	7280
Severity					
1 year	-2.2022	2.0123	-0.2432	2.1517	3671
2 years	-4.1396*	1.6212	-2.1619	1.7331	5895
3 years	-3.8331*	1.4881	-0.6634	1.5901	7280

Note: Each line and column represents results from one regression.
Estimates of the control variables are not reported, but included in all extended models.
Robust standard errors are clustered on county and year.
* indicates $p<0.05$.

From the raw differences between the EM and prison sample (represented by the estimates in the simple models) we can clearly see that those on EM have lower recidivism rates than those who serve their sentence in prison, and that those who reoffend do so less frequently and less severely.[44] This corresponds to the finding in Rokkan (2012b) and Rasmussen et al. (2016). Moving on to the extended models, however, we see that these differences are reduced when accounting for some of the individual characteristics of those on EM and those in prison. And while the differences in recidivism rates remain statistically significant, the differences in recidivism intensity and severity do not. The results hence suggest that some (and for certain measures, maybe all) of the observed difference in reoffending behavior among those on EM and those in prison is driven by selection rather than causation.

6.3 Effect estimates

We now move on to our quasi-experimental models trying to capture the causal effect of EM on recidivism. Effect estimates from both the DD and IV analyses are shown in Table 5 below. Specification tests and robustness results are provided in Section 6.4, and the F-tests from the first stage of the IV model are summarized in table 14 in Appendix D.

[44] If we use an unrestricted sample of everyone on EM vs. everyone serving an unconditional prison sentence of 120 days or less in prison, the differences are even more substantial. For instance, the relative difference in the 2-year recidivism rate is 46.8 percent, while it (as seen in table 4) is 32.4 percent in our current sample.

Table 5: Effect of EM introduction on reoffending behavior, 1-3 years after release. Simple and extended models. Main analytic sample (N=24 329)

	1 year				2 years				3 years			
	Simple		Extended		Simple		Extended		Simple		Extended	
	b	SE	b	SE	b	SE	B	SE	b	SE	b	SE
Rate												
DD	-.0135	.0108	-.0164	.0107	-.0194	.0113	-.0238*	.0112	-.0005	.0144	-.0048	.0140
IV	-.0291	.0162	-.0348*	.0162	-.0380*	.0185	-.0461*	.0184	-.0060	.0248	-.0144	.0240
Intensity												
DD	-.0242	.1110	-.0604	.1125	.0774	.1282	.0473	.1290	.0261	.1440	-.0238	.1444
IV	.1316	.2835	.0550	.2933	.3461	.2777	.2967	.2903	.2233	.2844	.1435	.3003
Severity												
DD	1.2696	1.5562	1.339	1.674	.9240	1.474	.5720	1.549	1.3190	1.290	.7780	1.340
IV	2.7318	3.5874	3.0514	3.8708	2.0636	2.7848	1.1661	2.8929	2.6037	2.3953	1.5224	2.5135
N	**24 329**		**24 329**		**24 224**		**24 224**		**22 034**		**22 034**	

Note: Each line and column represents results from one regression.
Estimates of the control variables are not reported, but included in all extended models.
Robust standard errors are clustered on county and year.
* indicates $p<0.05$.

As we see from the table, there are no statistically significant estimates of the effect of EM on neither recidivism intensity nor severity, irrespective of which estimator we use. This hence confirms our expectations from the naïve regressions. For the rate measures, however, both the DD and IV models consistently estimate that EM led to a statistically significant decline in the 2-year recidivism rates. The ITT effect, which is expressed in the DD-estimate of -2.38 percentage points, equals a 9.8 percent decline in the 2-year recidivism rates in the treatment counties after EM was implemented. Moreover, the LATE, which is given by the IV estimates of 3.48 and 4.61 percentage points after one and two years respectively, equals a relative decline of 23.1 and 19.0 percent among those who actually served on EM. If we calculate the LATE from a model where the first stage is based on the DD estimate, i.e. if we effectively attribute the overall DD estimate solely to those who actually serve on EM (assuming no effect on those actually serving in prison), we get a relative decline of 17.8 percent after 1 year and 16.1 after 2 years.

It is worth highlighting that the effect of EM on recidivism does not seem to last into the third year in neither model. It is important to remember that only those who lived in the counties that implemented EM in 2008 contribute to this estimate, and that the gradual increase in initiated EM sanction after the implementation (as seen in Figure 1) suggests that we would see effects for those who started their sentence in 2009 at the earliest. We can therefore not know whether this result would be replicated also for later cohorts, but we encourage future analyses to explore this further. If such a tempo effect is present, this would be of particular policy concern as it suggests that EM only have short term effects on recidivism while long-term outcomes remain the same.

6.3.1 Subsample analyses

To explore the theoretical expectations discussed in Section 2, we split our main analytic sample into theoretically interesting subgroups that enable us order to better understand which mechanism(s) that are likely to drive these results. We will present only the results related to the 2-year recidivism rates, as there are no statistically significant effects in the subsamples for the other measures. All estimates (from models including covariates) are summarized in Table 6 below.

Table 6: Effect estimates, 2-year recidivism rates. By subsample. Extended models. N=24 224

	DD		IV		N
	b	SE	b	SE	
Age					
25 or younger	-0.0227	0.0224	-0.0421	0.0365	**7443**
Over 25	-0.0210	0.0129	-0.0447	0.0234	**16781**
50 or younger	-0.0195	0.0126	-0.0356	0.0204	**20671**
Over 50	-0.0441	0.0236	-0.1020*	0.0465	**3553**
Social integration					
Unemployed[a]	-0.0118	0.0198	-0.0338	0.0327	**8075**
Not unemployed	-0.0324*	0.0119	-0.0547*	0.0201	**16149**
Low income	-0.0170	0.0215	-0.0379	0.0476	**8070**
Not low income	-0.0223	0.0140	-0.0411*	0.0200	**16154**
Married	-0.0111	0.0285	0.0208	0.0426	**3586**
Unmarried	-0.0239	0.0121	-0.0476*	-0.0273	**20638**
Prison experience					
Previously imprisoned	-0.0090	0.0481	-0.1188	0.1189	**1740**
Not previously imprisoned	-0.0243*	0.0109	-0.0402*	0.0176	**22484**

Note: Each line and column represents results from one regression.
Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year.
* indicates $p<0.05$.

As we see from Table 6 there are indeed some heterogeneous patterns between various subgroups in our sample. All estimates are negative, but in most cases they only reach statistical significance for one subgroup. Both the DD and IV estimates are statistically significant for offenders *without* previous imprisonments and offenders *without* registered unemployment the year before starting the sentence, as is the IV estimate (but not the DD estimate) for offenders *over* 50, *married* offenders and offenders *without* low income from paid work. When we run these models on the full sample using interaction terms (results not shown) these estimated differences are, however, not statistically significant. It is also worth noticing that the statistically significant estimates typically can be found in the largest subsample group, and the lack of statistical significance in (and between) some groups can hence be a matter of statistical power. We encourage these subsample differences to be explored further in future analyses.

6.4 Specification tests and sensitivity analyses

Before we move on to a discussion for our results we wish to address whether they are likely to be robust to our methodological choices and/or violations of the identifying assumptions of our models. We again focus on the two-year recidivism rates, as these are ones reaching statistical significance.

Table 7: Sensitivity tests, 2-year recidivism rate. Extended models. Main analytic sample (N=24 329)

	DD		IV		N
	b	SE	b	SE	
Baseline	-0.0238*	0.0112	-0.0440*	0.0180	24 224
Sample criteria					
120 days or less	-0.0213*	0.0097	-0.0465	0.0281	48 636
60 days or less	-0.0258*	0.0098	-0.0548*	0.0246	41 480
Current offence	-0.0323*	0.0120	-0.0566*	0.0216	28 392
Previous offending	-0.0238*	0.0112	-0.0440*	0.0180	24 224
Clustering of S.E.					
County	-0.0238*	0.0099	-0.0440*	0.0153	24 224
Personal Identification Number	-0.0238*	0.0121	-0.0440*	0.0208	24 224
Timing					
County year-1	-0.0265*	0.0119	-0.0528*	0.0198	24 224
treatXyear					
2003	0.0036	0.0205			24 224
2004	-0.0176	0.0222			24 224
2005	0.0153	0.0246			24 224
2006	0.0063	0.0216			24 224
2007	-0.0263	0.0197			24 224
2008	-0.0231	0.0221			24 224
2009	-0.0144	0.0206			24 224
2010	-0.0110	0.0210			24 224
2011	-0.0254	0.0188			24 224

Note: Each line and column represents results from one regression unless otherwise specified.
Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year unless otherwise specified.
* indicates $p<0.05$.

A first concern is that our definition of the group of offenders likely to be eligible for EM (i.e. our sample criteria) is either important for our estimates or has compromised the external validity of our results. The second panel of Table 7shows the effect of EM on the 2-year recidivism rate when the models are run on four different samples, where we gradually apply more sample restrictions until we arrive at the main analytic sample. As we can see the estimates and their p-values remain almost unchanged irrespective of which sample criteria we use, which we find to be reassuring.

Moreover, we rerun all analyses using different several kinds of clustered standard errors (see the third panel of table 7). We find that this does not affect the standard errors in any substantial way.

A third key issue is whether the EM implementation led a certain group of offenders to move into EM counties in order to qualify, hereby making EM eligibility endogenous to individual characteristics (and potentially also recidivism risk). We address the impact of self-selection by re-running the analyses using a treatment-variable (*treatXpost*) that is based on the offender's permanent residence

address as of Jan 1 the year *before* starting the sentence. If offenders have moved to EM counties in order to qualify, and the pre- and post-composition in these counties differ systematically, we would expect the estimates to change. As we see from the fourth panel of Table 7 the estimates increase somewhat in size, but they are all well within one standard error of our main results. Examining moving patterns (results not shown) we also find that 94 percent of the sanctions on EM were served by offenders who lived in an EM county both the year of and the year before starting the sentence.

6.4.1 Pre-implementation trends

The main identifying assumption for the DD-approach is equal trends in recidivism in the treatment and comparison counties in the absence of EM. The recidivism trends in the figures in Appendix C are more inconsistent than we would ideally like; for instance, those who started their sentence 1-12 months before EM was implemented or extended (i.e. during year -1) had higher recidivism rates if they lived in treatment counties than if they lived in comparison counties. And although the rates are not higher than one can observe for these counties also in previous periods, the lack of consistency in these trends gives reason for some concern. To examine whether there are reason to believe that the EM counties were selected to participate in the pilot due to their pre-implementation recidivism trends, we rerun equation (1) using an interaction term between *treat* and *year* instead of *treatXpost.* If the interaction terms are significant before EM is implemented, this would challenge the validity of our DD analysis. Reassuringly, these estimates (shown in the lower panel of Table 7) are not statistically different from zero for any of the pre-implementation years. This is also true for the post-implementation years (2009-2011), and the effect we capture in our models is hence driven by the average negative effect of *all* post-implementation cohorts rather than just one year. However, if we run regressions on all pre-implementation years, we do not find significant negative effects.

6.4.2 Changes in sentencing practices

Another, more profound challenge to both our DD and IV analyses is that the introduction of EM may have affected reoffending behavior in other ways than the introduction and extension of EM itself. One key way in which this may happen is if judges changed their behavior in court because of the EM introduction; more lenient judges might for instance be more likely to convict someone to 120 instead of e.g. 130 days of prison due to the EM criteria, while the opposite could be true for judges less in favor of EM/non-custodial sentences. It is also possible that the introduction of EM shifted some offenders from other sentencing options to unconditional prison; either because the judge preferred some aspects of EM and saw it as improbable that the offender would not be admitted, or because judges favored EM's ability to ease the pressure on the prison system.

To explore these possibilities further, we use the register data on all[45] passed criminal sanctions (Statistics Norway 2016b) to assess changes in passed court convictions in the EM counties after the implementation of EM. We first explore a shift in the distribution of conviction length, by estimating the likelihood of being convicted to an unconditional prison sentence of a) 120 days or less (i.e. a theoretically qualifying sentence), and b) 60 days or less (i.e. a factual qualifying sentence) as compared to longer unconditional prison sentences. Second, we explore a change in the likelihood of receiving an unconditional sentence of maximum 120 or 60 days as compared to all other sanctions. Finally, we explore a change in the likelihood of being convicted to community service or conditional prison as compared to any other sentence, as these are sentence types from which we could expect to see a spill-over to EM (see Rokkan 2011, p. 14). Estimates are summarized in Table 8 below. [46]

Table 8: Changes in sentencing practices after the EM implementation. DD-estimates, extended models. N=204 064

Sample	Outcome variable	b	S.E.	N
All unconditional prison sentences	Unconditional prison to <=120 days	-0.0017	0.0059	91 539
	Unconditional prison to <=60 days	-0.0033	0.0071	91 539
All passed convictions	Unconditional prison <=120 days	0.0126*	0.0048	204 064
	Unconditional prison <=60 days	0.0096*	0.0044	204 064
	Community service/conditional prison	-0.0119*	0.0058	204 064
	Community service	-0.0078	0.0042	204 064
	Conditional prison	-0.0041	0.0062	204 064

Note: Each line represents results from one regression.
Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year.
* indicates p<0.05.

As we see from the upper panel of the table, there is no significant change in the likelihood of being convicted to a shorter, qualifying unconditional prison sentence as opposed to a longer unconditional sentence after EM was implemented. However, looking at all court convictions (as shown in the lower panel), there is a statistically significant *increase* in the likelihood of being convicted to a qualifying unconditional prison sentence and a statistically significant *decrease* in the likelihood of being convicted to community service and conditional prison. This suggests a shift from these other (non-custodial) sentencing options to unconditional prison after EM was implemented,[47] although the estimates for the two sentencing options considered here are not statistically significant when measured separately.[48]

[45] Please note that we cannot make any restrictions to these data similar to those we do to the main study sample. This is because the assessments of EM "suitability" cf. current and previous offending etc. are made by the NCS and not by the courts.

[46] These analyses are based on a version of Equation (1) where $time_t$ and age_i are based on the date of conviction (not the date of starting the sentence) and the conviction length is excluded from X_i.

[47] Such a shift entails that the implementation of EM may have increased general deterrence.

[48] The estimate for community service is marginally non-significant, with a p-value of 0.067.

The validity of these results does, of course, hinge on the same assumptions as in our main analysis. To explore pre-implementation trends for all outcomes we have rerun the above models using an interaction term between *treat* and *year* (*treatXyear*) instead of *treatXpost*, and estimates are included in Appendix E. Focusing on the apparent shift from non-custodial sentencing options to short unconditional prison sentences, these analyses show that the results in Table 8 must be interpreted with some caution. First, although the estimates for short unconditional sentences are statistically significant in the post-implementation years (2009-2011) and not the previous years, it is worth noticing that the estimates are positive in all pre-implementation years and that they increase in size over time. Second, the estimates for both non-custodial sentencing options are negative and statistically significant in nearly all of the pre-implementation years – although they seem to increase somewhat in size after the implementation. As such, it is possible that the sentencing analysis to some extent captures different trends in sentencing behavior between the treatment and comparison counties.

If judges changed their behavior as a result of the EM implementation this has two main implications for our results. First, the interpretation of our findings is challenged by the question of what exactly is the counterfactual (to EM): While our theoretical reasoning is based on an assumption that the counterfactual is a prison sentence served in prison, this analysis suggests that the counterfactual for some offenders can be another non-custodial sanction – for which EM would likely imply a *higher* level of control and rehabilitative content. Since we would not know the counterfactual in each case, it is difficult to explore this further empirically, but it is useful to keep in mind that our theoretical expectations may not hold for some offenders in our sample if introduction of EM actually transferred them from a sentence to e.g. social work to a possibly more intrusive sentence on EM.

Second, the validity of our results may be compromised if a change in sentencing practices systematically alters the composition of our treatment group. The direction of a bias would, of course, depend on what type(s) of offenders are convicted differently because of EM, and whether they differ from others in the target group. While it is intuitive to assume that those convicted to non-custodial sentences have lower recidivism risks than those who are not, and that the estimated change in sentencing practices hence biases our results downwards, it is important to keep in mind that our main analytic sample comprises a low-risk subgroup of those convicted to unconditional prison. At the same time, the implementation is likely to affect the sentencing of offenders that are perceived to be "on the margin" of custodial and non-custodial sentencing options, by shifting some of those in the high-risk spectrum of the non-custodial groups into the low-risk spectrum of the custodial (i.e. EM) group (Andersen and Andersen, 2014).

Using the NCS data of all sentence records (Norwegian Correctional Service, 2008) to look at the recidivism rates of offenders released from various sentencing types, we find that those released from

a community or conditional prison sentence reoffend to a *higher* extent than the offenders in our main study sample (but lower extent than everyone released from unconditional prison). Compared to our sample, and accounting for differences in age and gender, the 2-year recidivism rate is 14.0 percentage points higher among those convicted to unconditional prison and 18.7 percentage points higher among those convicted to community service.[49] It hence seems that the change in sentencing practices following the EM implementation would move individuals with higher rather than lower recidivism rates into our sample, thus biasing our estimates *upwards* rather than downwards. This implies that we could be underestimating the effect of EM on recidivism

7 Discussion

This analysis set out to estimate the causal effect of EM on recidivism, placing itself within a wider literature on the effectiveness of custodial vs. non-custodial sentences and of EM programs more specifically. To answer this question we made use of the plausibly exogenous variation in EM eligibility that resulted from the introduction of a Norwegian EM program in 2008. The program was not net-widening, and did include a large focus on rehabilitative and integrating activities that were performed in the community instead of in prison.

Our results consistently show that EM reduced 2-year recidivism rates by about 10 percent in the treated sample (ITT) and 19 percent among those on EM (LATE). This coincides with the overall literature on non-custodial vs. custodial sentences, in which most quasi-experimental studies find negative effects of non-custodial sanctions on recidivism (cf. Villettaz et al., 2015). Moreover, the study confirms the overall Scandinavian literature (Andersen and Andersen, 2014; Jørgensen, 2011; Larsen, 2016; Marklund and Holmberg, 2009), which points to pro-social effects of introducing EM in Sweden and Denmark. However, we do not find any effect on neither recidivism intensity nor severity, and this suggests that EM has the potential to steer people out of a criminal lifestyle but not to significantly change the characteristics of the reoffending behavior of those who persist.

7.1 Theoretical Mechanisms

In order to explore the relevance of the theoretical expectations discussed in Section 2, we reran the main analyses in theoretically interesting subsamples. However, as we remember from Section 6.3.1, there were no statistically significant differences between either group. We nonetheless believe two main insights should be considered from this analysis.

[49] These numbers are based on a linear model with controls for gender (female=1) and age at release (quadratic variable), using data on all sentencing records (Norwegian Correctional Service, 2008) for community service, conditional prison or EM qualifying unconditional prison (as defined in our sample criteria) with an end date between 2002 and 2008 (N=51 375). This pattern of higher recidivism rates in the two non-custodial groups remains unchanged if we run the analysis using the data on passed criminal sanctions (Statistics Norway, 2016b).

First, the estimates are negative both in the full sample and in all subsamples, and we do hence not find any support for the deterrence, situational and strain mechanisms which all suggested increases in recidivism as a result of EM (cf. Jørgensen, 2011; Marklund and Sweberg, 2009; Killias et al., 2010). Second, and although we are unable to draw certain conclusions based on the data at hand, the main results seem to be driven by changes in certain groups of offenders. Combining the information on where we find statistically significant estimates (Table 7) and the theoretical expectations in Table 1, this suggests the following support for some of the mechanisms that produce negative effects of EM on recidivism.

The estimates are negative and statistically significant among those who have never been imprisoned before but non-significant among those who have – suggesting support for the protection mechanism. The integration mechanism also receives some support in that we find statistically significant declines in recidivism among those who are not registered as unemployed and those who have an income exceeding the base rate of the of the National Insurance Scheme, but not among those without registered unemployment and lower income from paid work. This suggests that EM can reduce recidivism because it enables offenders with links to the labor market to maintain those links while serving their sentence – an interesting possibility we hope will be pursued in future analyses. On the other hand, the protection mechanism is contradicted in that the IV analyses show statistically significant declines among those who are *unmarried* rather than married. This can indicate that to serve a sentence at home is more challenging to the life/crime balance when in a marital relationship, as suggested by Sørensen and Kyvsgaard (2009), hence providing support for the situational mechanism. The estimated effect sizes are consistently larger for offenders over the age of 50, although only statistically significant in the IV models. This suggests that EM might serve as a "hook for change" for these offenders cf. the hook for change mechanism. It is worth noticing that we do not find any particularly beneficial effects among young offenders, as was the case in Jørgensen (2011) and Andersen and Andersen (2015). While seemingly unique in the Scandinavian context, this is more in line with the international literature which suggests small or zero effects of programs and treatment on young offenders (e.g. Uggen and Staff, 2001). In sum, and although associated with statistical uncertainty, our subsample analysis may be taken to suggest that policies which are able to keep offenders without previous imprisonments out of prison and/or enable offenders with existing employment relations to maintain these while serving their sentence[50] is worthy of further policy attention.

[50] Consistent with similar mechanisms, Bhuller et al. (2016) find that imprisonment reduces residivism significantly among individuals who were not working prior to incarceration, while there is no effect on individuals who were working.

7.2 Study limitations

There remains some concern as to whether the identifying assumptions of our models are met. The pre-implementation trends are not as consistent as one would like, and there could still be features of the EM implementation that we have not become aware of or been able to capture in our data. Moreover, the signs that sentencing practices changed after the EM implementation still comprise a potential source of bias, although this may very well be biasing our effect estimates toward zero. As such, our analysis faces some of the methodological challenges that characterize the EM and non-custodial literature more generally (Di Tella and Schargrodsky, 2013; Renzema and Mayo-Wilson, 2005; Whitfield, 2001). We do, however, find it reassuring that neither of the sensitivity analyses or robustness checks have compromised our results, suggesting that the results do not hinge on particular choices in setting up the samples or models.

There are several other limitations to our analysis that we wish to highlight. First, it is important to remember that our outcome measures are based on administrative registers that only include information on (the unknown proportion of) offences that are discovered, reported, investigated and solved by the police. The offences that are *not* included in our data will, however, only bias our effect estimates as long as these "dark numbers" are unevenly distributed between our treatment and comparison groups. One way to deal with this shortcoming is to supplement the registers with self-reported offending, but such data are not available.

Second, and as regards to the external validity of our results, we are not able to distinguish between the effects of the RF technology and the other component of the Norwegian EM program – to do this we would need more variation in both the technologies used and other services provided. It is hence challenging to determine whether we could expect to see similar effects if one was to implement the same program in another penal (and social, economic and cultural) setting. Renzema and Mayo-Wilson's (2010, p. 219) review states that EM reduces recidivism primarily when implemented in systems that are characterized by an overall focus on treatment and rehabilitation, and as this is the case for the Norwegian system we believe the external validity of our findings is highest for other systems with similar characteristics.

Third, we are not able to determine whether the lack of a statistically significant decline in 3-year recidivism rates is due to a tempo-effect of EM on recidivism in the full sample, the presence of such an effect in the counties enrolled in the 2008 pilot, or simply a lack of statistical power associated with

the lower number of observations. We would encourage a replication of the analysis using later cohorts than 2011, in order to determine whether this is the case.[51]

Fourth and finally, our subsample analysis is only able to provide limited insight into which mechanisms that can be relevant – while the "black box" between EM and recidivism is yet to be opened and dissected (Hedström, 2005). We encourage future studies to investigate these (and other) mechanisms further, as a greater understanding of their explanatory powers would enhance our knowledge on why EM programs (and non-custodial sentences more generally) can or cannot make a substantial difference to the lives of offenders.

7.3 Policy implications

This analysis suggests that non-custodial alternatives to prison, here represented by an EM program, can have favorable effects on recidivism. This is encouraging in several respects, as it supports the idea that non-custodial alternatives to prison can be beneficial to the system, the public and the offender alike.[52] Such a win-win scenario encourages the search of other non-custodial sentencing options, and our subsample analyses suggest that non-custodial options that enable the offender to maintain employment relations can be particularly worth exploring. Moreover, a potential next step could be to expand the target group of the current program to include other groups of offenders, as has been done in e.g. Denmark.

To this positive conclusion we have three main objections. First, an important, yet unexplored issue is whether and how implementing EM (or other non-custodial sanctions) has unintended consequences for offenders who are *not* in the program's target group, here being those who serve longer sentences in prison. EM can – by removing the "straightest" offenders from prison – make prison conditions harsher or less rehabilitative for those who remain, and this can in turn affect individual outcomes as well as overall crime rates. We do not suggest that one should keep offenders who would recidivate to a lesser (or equal) extent after a non-custodial sentence in prison in order to "make prison nicer" for everyone, merely that this is an area worthy of both scientific scrutiny, policy attention, and if better documentation evolves, compensatory efforts.

Second, if later studies confirm a tempo effect of diminishing effects of EM over time, this would be of particular policy concern. If serving on EM simply serves to "set offenders off on a good note",

[51] Most studies typically apply a follow-up of no more than two years, and that in a two-year follow-up our conclusion would be unambiguous.

[52] Note that even a restrictive interpretation of our findings, in which the negative effect estimates could be discarded due to the remaining methodological challenges, we find no support to claims that EM *increases* recidivism. Due to positive experiences made by offenders and correctional officers alike, as well as desireable impacts on prison capacity and correctional spending (Rasmussen et al., 2016), an overall positive conclusion would persist.

while the long-term outcomes remain unchanged, this would call for increased attention to what measures can be taken in order to maintain lower recidivism over time.

Third, implementing EM can have a negative impact on general deterrence. It is important to keep in mind that the implementation of (seemingly) less punitive sentencing options can increase crime rates in general, hence posing a potential threat to public safety.

Finally, and on a more general note, a useful policy insight comes from the obvious challenges in deriving causal effects of this EM program. It is an outspoken policy goal both in Norway and elsewhere to determine "what works" in criminal justice, and in order to do so we would strongly encourage a closer connection between the implementation and evaluation stages of a given program. This would enable all research designs – from the highest[53] to the lowest level of scientific rigorousness (Sherman et al., 1997) – to be considered from the get-go, hereby improving the prospects of achieving as high an internal validity as ethically and practically feasible.

7.4 Conclusion

This study uses plausible exogenous variation in EM eligibility to estimate causal effects of EM on recidivism. Using a pilot in Norway as the framework for a difference-in-differences and instrumental variables design, the results indicate that implementing EM reduced 2-year recidivism rates by about 10 percent in the counties that took part in the implementation, translating into a reduction of about 19 percent for those who served their sentence on EM. However, we find no effects on recidivism intensity nor severity, suggesting that EM has the potential to steer (some) people out of a criminal lifestyle but not to significantly change the characteristics of the reoffending behavior of those who persist. The results are somewhat challenged by inconsistent pre-implementation trends in recidivism, and by analyses which suggest that judges have increased their use of EM qualifying sentences after EM was implemented. Subsample analyses, although statistically imprecise, show that the effects are strongest among offenders without recent unemployment spells and/or without previous imprisonments. This may be taken to suggest that policies which keep offenders out of prison and/or enable offenders with existing employment relations to maintain these while serving their sentence, are worthy of further attention. In sum, this study adds to a growing body of literature that suggests that it is possible to reduce prison populations and correctional spendings without making compromises on behalf of recidivism or other criminal justice goals; a finding that is promising in both the Norwegian and international setting.

[53] This promotes randomized experiments as the "gold standard" for causal inference; see e.g. Berk (2005) and Sampson (2010) for alternative views.

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Appendix A: Sampling procedure

Table 9: EM criteria and operationalizations

Official stating of guidelines	§	Operationalization of official guidelines	Final operationalization
CURRENT OFFENCE			
Unconditional prison sentence of max. 4 months If numerous sentences: total length max. 4 months	7-2	Max 120 unconditional days in sentence identifier	Max 60 unconditional days in sentence identifier
Not convicted to preventive detention or fine atonement	7-2	Only convictions to prison included	
Exceptions from all rules for offenders under the age of 18 at the time of application	7-2	Age 17 or younger at time of starting the sentence *included*	Age 17 or younger at time of starting the sentence *excluded*
No risk of evasion	7-3	Not available in data	
<u>"General rule":</u> No sexual offences Exceptions *can* be made for offenders under the age of 23. No violent offences Exceptions *can* be made if the sentence is less than 60 days No offences committed from the home/against family members Exceptions *can* be made for offenders under the age of 23. Not convicted due to violation of conditions for other sanctions	 7-3 7-3 7-3 7-2	 Sexual=0. Exceptions ignored Violent=0. Exceptions ignored Not available in data Exceptions ignored Not available in data	
<u>"Should be taken into consideration":</u> Type of offence Young age Severity of offence Time since offence Common conceptions of justice Interests of victims and their relatives Need for societal protection Health issues Behavior during preparatory proceedings	 7-3 7-3 7-3 7-3 7-3 7-3 7-3 7-3 7-3	 See own crime type criterion See own age criterion Insufficient information in guidelines Insufficient information in guidelines Not available in data Not available in data Not available in data Not available in data Not available in data	
PREVIOUS OFFENDING			
Previous sanctions for offenses that would disqualify for EM should be "taken into consideration"	7-3	Previous violent=0 Previous sexual=0	
"General rule": No charges for new crimes cf. the Criminal Procedure Act § 82	7-2	Recent charges=0	
<u>"Should/can be emphasized":</u> Previous offences committed while serving a sentence A history of technical violations A history of "no-show"	 7-3 7-3 7-3	 During atonement=0 Not available in data Not available in data	
LIVING CONDITIONS			
Permanent work- or residence permit	7-2	Norwegian PIN required	
Suitable place of residence in EM county	7-2 7-3	County=an EM county Suitability requirement ignored	
Employment or other suitable activity during atonement	7-4	Ignored	
Written consent of any household members over the age of 18	7-3	Not available in data	
Not compromising the best interests of children	7-3	Not available in data	
"Rule of thumb": Not deported from the country	7-1	Not available in data	

All paragraphs in the table refer to Norwegian Correctional Service (2013). All operationalizations refer to variables defined in Section 5.2.

Table 9 includes information on all absolute and more discretionary guidelines for admitting offenders to EM. The leftmost column denotes the guidelines as stated in the official document (own translations, with discretionary language in quotation marks to improve transparency), separated into three sections depending on whether they relate to the current offence, previous offending or to the living conditions of the offender. The middle column denotes our operationalizations, including information on which guidelines we don't have information on and which are defined too vaguely to be properly assessed. The rightmost column denotes the final operationalization of a given guideline (if different from the initial).

A few notes on the transition from the middle to the rightmost column in Table 9 are in order. Firstly, the official guideline set the cutoff of sentence length to 120 days, while we set it to 60 days in our final analysis. This is due to the practical execution of the guidelines, which shows that only 3 percent of the offenders on EM have a sentence length of more than 60 days.[54] Secondly, and based on the same logic, the guidelines enable all rules on sentence length, type of offence, previous offending etc. to be disregarded in cases where the offender is under 18 years old at the time of application. However, no offenders in the actual EM group are this young, and offenders under the age of 18 are therefore *excluded* rather than included. Thirdly, all violent offences – both current and previous ones – are excluded, even though some offenders convicted of common and aggravated assaults have been permitted to serve on EM and these offenders have been described elsewhere as "in the target group" of EM (see Rokkan 2012a). The occurrence of assaults is, however, substantially more common in the prison sample, and we therefore prefer the stricter criterion.[55] Fourth, some of the guidelines are deliberately not used for sample criteria although we do have some vaguely related information in the data. We ignore the employment/activity criterion as all offenders were to receive ample assistance in finding "suitable work", meaning that it is very unlikely that anyone would fail to meet EM eligibility because of this. Moreover, the exceptions that can be made to some criteria are also ignored because they seem to have been very rarely used in practice. Fifth and finally, issues of health, individual behavior and sense of justice cannot be assessed in our data.

Table 10 shows the occurrence of the various sample criteria in the prison and EM samples, which we have presented in two ways. First, in the middle ("Iterating") column, we have summarized simple cross tabulations showing the proportion of offenders in the prison and EM samples that fall within a given sample criterion (listed in the left-most column). For instance, we see that 97 percent of the EM sample is convicted to no more than 60 days of unconditional prison, while the same is true for 70

[54] It is worth noticing that other sample criteria make the difference between a 60 and 120 cutoff almost negligible (1857 vs. 1908 EM records (a 2.7% increase) and 22 427 vs. 23 909 prison records (a 6.6% increase), respectively).

[55] After all other simple criteria are imposed, only 99 EM sentences and 2301 prison sentences (a 5 and 9 percent difference, respectively) distinguish a sample where these particular kinds of violent offences are included or excluded.

percent of the prison sample. Second, in the right-most ("Stepwise") column we show the proportion of the original EM and prison samples that remain after a given sample criterion *and those in previous rows* in the table have been imposed. For instance, about 60 percent of the original prison sample (N=56 246) and 90 percent of the original EM sample (N=2 448) remain after offenders that are convicted to more than 60 days of unconditional prison *and* who are convicted of sexual and/or violent offences are excluded. The bottom row of the right-most column shows that after all sample criteria have been applied we are left with about 40 and 76 percent of these two samples.

Table 10: The occurrence of the sample criteria in the EM and prison samples. Percentages

Criterion	**Iterating**		**Stepwise**	
	Prison	EM	EM	Prison
Sentence length: Max 120	82.53	99.89	82.53	99.89
Sentence length: Max 60	69.85	97.06	69.85	97.06
Current offence: Sex	2.76	0.37	69.24	96.69
Current offence: Violence	17.89	6.62	59.65	90.16
Previously convicted: Sex	1.28	0.53	59.65	90.16
Previously convicted: Violence	16.11	6.33	52.97	84.23
Under 18 years when starting sentence	0.09	0	52.94	84.23
New charges	27.1	7.27	42.86	78.80
Previous offending during atonement	12.09	3.64	41.44	77.25
Released from prison within 24 months of current sentence	13.22	2.49	39.95	75.86
N	**56 246**	**2 448**	**56 246**	**2 448**

Appendix B: Descriptive statistics

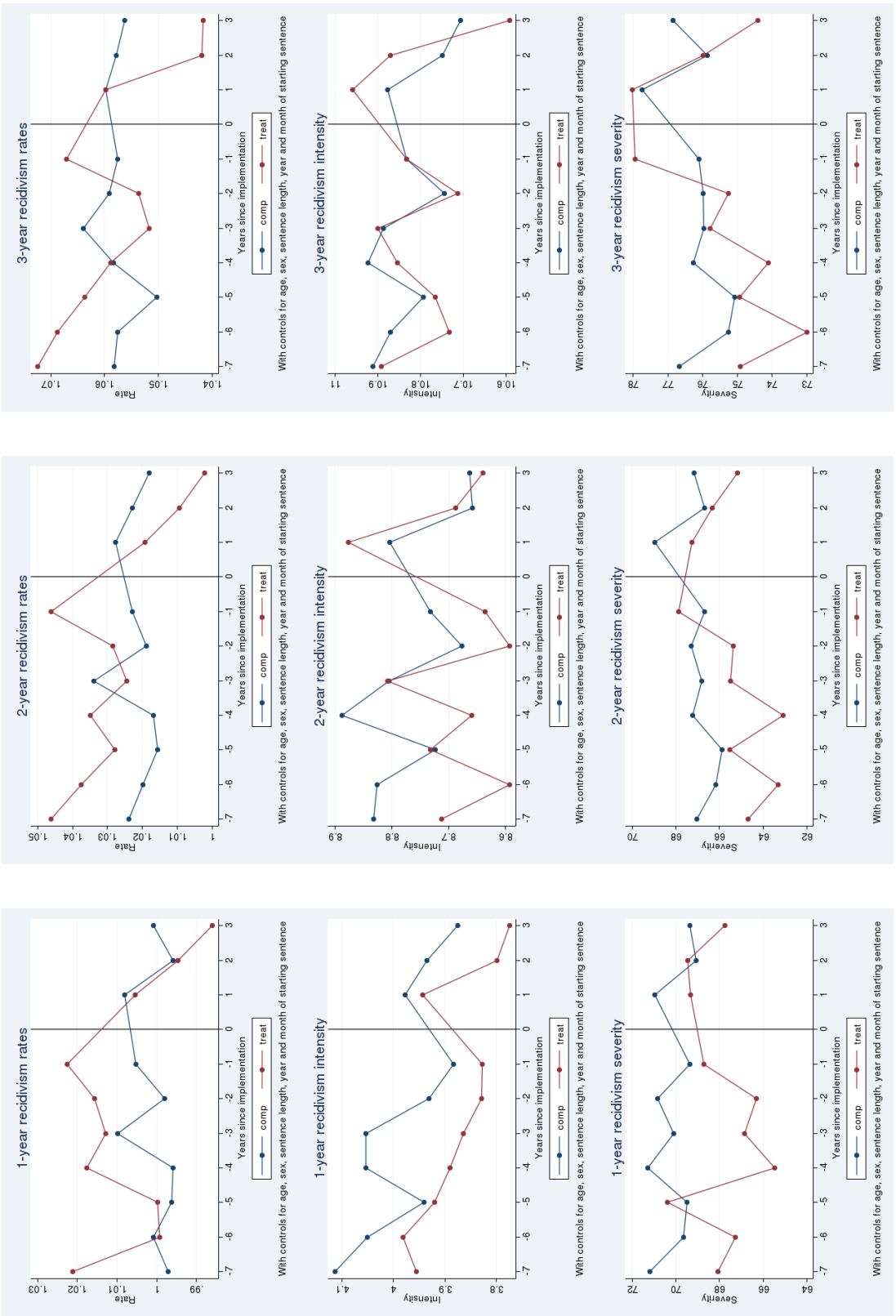
Table 11: Descriptive statistics of main analytic sample, treatment and comparison group. Percentages and means (with standard deviations). N=24 329

		Comparison	Treatment
OUTCOME VARIABLES			
Recidivism rate	1 year	14.61 %	15.44 %
	2 years	23.84 %	24.61 %
	3 years	30.52 %	30.73 %
Recidivism intensity	1 year	2.15 (1.53)	2.07 (1.47)
	2 years	2.61 (2.24)	2.57 (2.17)
	3 years	2.97 (2.76)	2.98 (2.74)
Recidivism severity	1 year	63.03 (27.83)	60.71 (26.39)
	2 years	64.21 (28.53)	62.61 (27.09)
	3 years	64.53 (28.38)	63.77 (28.54)
ANALYTICAL VARIABLES			
EM (actual)		0.55 %	12.84 %
Capacity		0.00	5.31 (0.09)
Year of starting sentence	2002	10.71 %	9.57 %
	2003	9.30 %	9.58 %
	2004	9.67 %	10.10 %
	2005	10.19 %	10.80 %
	2006	10.75 %	9.71 %
	2007	10.98 %	10.63 %
	2008	10.58 %	10.21 %
	2009	9.31 %	9.63 %
	2010	9.51 %	10.87 %
	2011	9.01 %	8.90 %
County of residence	Agder	0.00 %	10.41 %
	Akershus	0.00 %	16.63 %
	Hedmark	0.00 %	7.53 %
	Hordaland	0.00 %	13.66 %
	Oslo	0.00 %	17.78 %
	Rogaland	0.00 %	13.20 %
	Sogn og Fjordane	0.00 %	3.12 %
	Troms	0.00 %	8.16 %
	Vestfold	0.00 %	9.51 %
	Other	100.00 %	0.00 %
treatXpost		0.00 %	20.60 %
CONTROL VARIABLES			
Sex	Male	88.5 %	88.26 %
Age at starting sentence		34.82 (12.09)	35.48 (12.83)
Age at first offence		28.81 (13.12)	29.08 (12.97)
Conviction length (days)		25.69 (10.32)	25.84 (10.50)
MODERATOR VARIABLES			
Age at starting sentence 25 or younger		32.69 %	29.20 %
Age at starting sentence 55 or older		14.78 %	14.63 %
Unemployed at least one month the year before starting the sentence		34.81 %	32.40 %
Low income from paid work		34.52 %	32.48 %
Union status	Married	13.24 %	16.00 %
Previously imprisoned		6.60 %	7.71 %
N		**10 314**	**14 015**

Table 12: Descriptive statistics of treatment group, pre and post EM implementation. Percentages and means (with standard deviations). N=14 015

		Post=0	Post=1
OUTCOME VARIABLES			
Recidivism rate	1 year	15.57 %	14.93 %
	2 years	25.14 %	22.50 %
	3 years	31.01 %	29.11 %
Recidivism intensity	1 year	2.08 (1.48)	2.01 (1.45)
	2 years	2.95 (2.17)	2.93 (2.18)
	3 years	3.69 (3.02)	3.37 (2.54)
Recidivism severity	1 year	60.71 (26.49)	60.71 (26.01)
	2 years	64.49 (27.25)	63.56 26.41)
	3 years	67.11 (27.69)	65.29 (26.79)
ANALYTICAL VARIABLES			
EM (actual)		0.37 %	60.93 %
Capacity		0.00 (0.00)	25.79 (5.96)
CONTROL VARIABLES			
Sex	Male	88.76 %	86.35 %
Age at starting sentence		35.22 (12.77)	36.39 (13.01)
Age at first offence		29.17 (12.99)	28.74 (12.87)
Conviction length (days)		25.93 (10.26)	26.57 (11.36)
MODERATOR VARIABLES			
Age at starting sentence 25 or younger		26.64 %	23.14 %
Age at starting sentence 55 or older		8.40 %	9.01 %
Unemployed at least one month the year before starting the sentence		28.58 %	47.35 %
Low income from paid work		32.05 %	34.15 %
Union status	Married	15.48 %	17.98 %
Previously imprisoned		6.43 %	12.68 %
N		**11 128**	**2 887**

Appendix C: Recidivism trends



Note that the x-axis in the nine figures above represents the number of years since implementation – not the outcome as measured after a given number of years, and that the vertical line at year 0 in all plots hence denotes the timing of implementation.

Table 13 denotes the relative contribution of each implementation/change to the figures. We see that the 2008 implementation has the largest overall contribution to the analyses (42 percent as compared to 31 and 27 percent for the 2010 extension and 2011 implementation, respectively), and that there are fewer offenders that start their sentence in the earlier years. It is particularly worth noticing that there are no observations in the 2010 extension that are followed for three years, and no observations in the 2011 implementation that are followed for two (or three) years. This is due to the data limitations of this particular analysis, and future analysis using richer data for these (and later) cohorts are encouraged.

Table 13: Relative distribution of implementation stages

Years since implementation	2008 implementation	2010 extension	2011 implementation	Total
-7	7.53 %	11.12 %	13.37 %	9.93 %
-6	9.76 %	11.07 %	13.82 %	11.14 %
-5	10.01 %	12.18 %	13.54 %	11.46 %
-4	11.20 %	10.00 %	15.33 %	11.99 %
-3	10.13 %	11.19 %	12.84 %	11.10 %
-2	10.56 %	9.90 %	13.91 %	11.28 %
-1	10.07 %	10.58 %	12.18 %	10.75 %
1	9.91 %	13.37 %	5.02 %	9.46 %
2	11.09 %	10.58 %	-	8.06 %
3	9.75 %	-	-	4.84 %
N	**18 117**	**13 158**	**11 770**	**43 045**
Percent	**42.09 %**	**30.57 %**	**27.34 %**	**100.00 %**

Appendix D: IV-analysis

Table 14: F-statistics for first stage IV-analyses. F-values and degrees of freedom. Main analytic sample (N=24 329)

	Simple model		Extended model	
	F	D.f.	F	D.f.
Rate				
1 year	305.077	199	311.497	199
2 years	359.577	199	365.723	199
3 years	1022.650	179	925.268	179
Intensity		199		199
1 year	227.164	199	224.186	199
2 years	363.387	199	341.419	199
3 years	334.056	199	334.974	199
Severity		199		199
1 year	227.164	199	224.186	199
2 years	368.387	199	341.419	199
3 years	334.056	199	334.974	199

Note: Each line and column represents results from one regression.
Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year.

Appendix E: Outcome trends, sentencing analysis.

Table 15: Convictions to a qualifying EM sentence vs. all unconditional sentences. N=91 539

		Unconditional prison <=120 days		Unconditional prison <=60 days	
		b	SE	b	SE
treatXyear	2003	-0.0004	0.0157	0.0041	0.0202
	2004	0.0076	0.0118	0.0117	0.0156
	2005	-0.0122	0.0110	-0.0143	0.0153
	2006	-0.0141	0.0106	-0.0170	0.0142
	2007	-0.0084	0.0116	-0.0077	0.0143
	2008	-0.00085	0.0170	-0.0065	0.0208
	2009	0.0208	0.0106	0.0077	0.0133
	2010	-0.0100	0.0121	-0.0146	0.0147
	2011	0.0068	0.0145	0.0181	0.0156

Note: Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year.
* indicates p<0.05.

Table 16: Convictions to a qualifying EM sentence vs. all other sentences. N=204 064

		Unconditional prison <=120 days		Unconditional prison <=60 days	
		b	SE	b	SE
treatXyear	2003	-0.0014	0.0117	0.0008	0.0116
	2004	0.0219*	0.0108	0.0217*	0.0107
	2005	0.0093	0.0095	0.0059	0.0100
	2006	0.0124	0.0105	0.0084	0.0098
	2007	0.0160	0.0110	0.0137	0.0105
	2008	0.0118	0.0114	0.0108	0.0115
	2009	0.0280*	0.0108	0.0204*	0.0102
	2010	0.0336*	0.0112	0.0254*	0.0103
	2011	0.0287*	0.0130	0.0304*	0.0116

Note: Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year.
* indicates p<0.05.

Table 17: Convictions to community service and conditional prison vs. all other sentences. N=204 064

		Community service		Conditional prison		Community service or unconditional prison	
		b	SE	b	SE	b	SE
treatXyear	2003	0.0053	0.0101	-0.0123	0.0149	-0.0067	0.0141
	2004	-0.0037	0.0092	-0.0401*	0.162	-0.0436*	0.0151
	2005	0.0014	0.0089	-0.0464*	0.0137	-0.0448*	0.0135
	2006	-0.0196*	0.0090	-0.0191	0.0128	-0.0386*	0.0137
	2007	-0.0019	0.0089	-0.0167	0.0142	-0.0185	0.0140
	2008	-0.0044	0.0094	-0.0283*	0.0135	-0.0328*	0.0143
	2009	-0.0076	0.0097	-0.0300*	0.0150	-0.0375*	0.0149
	2010	-0.0057	0.0096	-0.0485*	0.0158	-0.0541*	0.0154
	2011	-0.0036	0.0119	-0.0356*	0.0148	-0.0391*	0.0135

Note: Estimates of the control variables are not reported, but included in all models.
Robust standard errors are clustered on county and year.
* indicates p<0.05.

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